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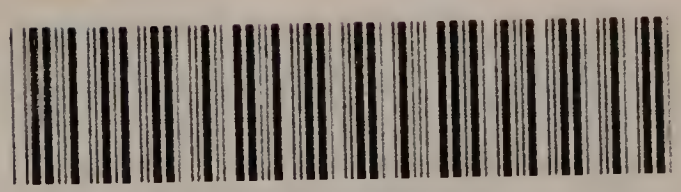
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WILLIAM CROOKES, F.R.S., &c.

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THE CHEMICAL NEWS.

VOLUME XXIII.

EDITED BY WILLIAM CROOKES, F.R.S., &c.

No. 580.—FRIDAY, JANUARY 6, 1871.

ON THE HEAT DEVELOPED IN THE COMBINATION OF ACIDS AND BASES.*

By THOMAS ANDREWS, M.D., F.R.S., Hon.F.R.S.E.,
Vice-President of Queen's College, Belfast.

In a paper communicated to the Royal Irish Academy in 1841, I gave an account of a large number of experiments on the heat disengaged when acids and bases, taken in the state of dilute solution, enter into combination, and when bases, insoluble in water, are dissolved in dilute acids. The following general conclusions or laws were deduced from those experiments:—

Law 1.—The heat developed in the union of acids and bases is determined by the base and not by the acid, the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat; but different bases, different quantities.

Law 2.—When a neutral is converted into an acid salt, by combining with one or more atoms of acid, no change of temperature occurs.

Law 3.—When a neutral is converted into a basic salt, by combining with an additional proportion of base, the combination is accompanied with the evolution of heat.†

Three years later, I laid before the Royal Society of London the results of an experimental investigation of the heat developed when one base is substituted for another in chemical compounds. The law deduced from this inquiry is implicitly involved in the foregoing, of which it may indeed be regarded as a necessary consequence. It was enunciated in the following terms:—

Law 4.—When one base displaces another from any of its neutral combinations, the heat evolved or abstracted is always the same, whatever the acid element may be provided the bases are the same.‡

Finally, the law of metallic substitutions, first announced in the *Philosophical Magazine* for August 1844, was then stated in a paper published in the *Philosophical Transactions* for 1848.

Law 5.—When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat developed is always the same; but a change in either of the metals produces a different development of heat.

In 1845, a paper appeared by Graham on the heat disengaged in combinations, the second part of which refers to the heat produced when hydrate of potash is neutralised by different acids.|| The results arrived at by this distinguished chemist exhibit a close agreement with those contained in my first communication to the Royal Irish Academy.

The concluding part of the elaborate memoir of MM. Favre and Silbermann on the heat disengaged in chemical actions is chiefly devoted to the same subject. A large number of experiments are described, which are nearly a repetition of those I had previously published. Their results bear a general resemblance to those given by myself in 1841; but they widely differ in the details. The authors of this able memoir fully recognise the accuracy of my fourth law, which asserts the equality of thermal effect when one base is substituted for another. "M. Andrews," they observe, "avait en effet établi que, quelque soit l'acide d'un sel la quantité de chaleur dégagée par la substitution d'une base à une autre pour former un nouveau sel est la même, lorsque l'on considère les deux mêmes bases."*

In a preceding paragraph of the same memoir, the authors object to what they conceive to be my first law, and state that it is not in accordance, with the results of their investigations. As the question is one of some importance, I may, perhaps, be permitted to quote the passage in the original language. "Ses conclusions, savoir: que la chaleur dégagée par l'équivalent d'une même base combinée aux divers acides est la même, ne s'accordent pas avec les résultats de nos recherches, et ne nous paraissent pas pouvoir être admises." No doubt, through inadvertence, MM. Favre and Silbermann have here given an inaccurate statement of my first law. It did not declare that precisely the same amount of heat is disengaged by all the acids in combining with the same base, but that the heat is determined by the base, "the same base producing, when combined with an equivalent of different acids, *nearly* the same quantity of heat." A comparison of the results of MM. Favre and Silbermann with those in my original memoir will show that I had fully recognised and described the deviations from the other acids, exhibited, on the one hand, in excess, by the sulphuric acid, and, on the other, in deficiency, by the tartaric, citric, and succinic acids. "If we refer," I remarked, in the original memoir of 1841, "to the first, second, and fourth tables, as being the most extensive, from the large number of soluble compounds formed by potash, soda, and ammonia, it will be observed that the sulphuric acid developes from 0·8° to nearly 1° more than the mean heat given by the other acids; while the tartaric, citric, and succinic acids fall from 0·4° to 0·55° short of the same. A minute investigation of the influence of the disturbing sources of heat will, no doubt, discover the causes of these discrepancies. The high numbers for sulphuric acid are probably connected with that acid's well-known property of developing much heat when combining with successive atoms of water. All the other acids develope nearly the same amount of heat in combining with the same base, the greatest divergences from the mean quantity being, in the case of potash, + 0·24° and − 0·13°; in that of soda, + 0·26° and − 0·14°; and in that of ammonia, + 0·17° and − 0·05°. These differences are almost within the limits of the errors of experiment."†

* Communicated by the Author. Abstract of a paper read before the Royal Society of Edinburgh.

† *Transactions of the Royal Irish Academy*, vol. xix., p. 228.

‡ *Philosophical Transactions* for 1844, p. 21.

|| *Memoirs of the Chemical Society*, vol. ii., p. 51.

* *Annales der Chimie et de Physique*, 3ème ser., xxxvii., p. 497 (1853).

† *Transactions of the Royal Irish Academy*, vol. xix., p. 240.

But although there is a superficial agreement between my original results and those of MM. Favre and Silbermann, they will be found, when examined closely, to differ widely in detail, and on points of great importance. I had found that the oxalic acid disengages almost exactly the same amount of heat in combining with the soluble bases as the hydrochloric, nitric, and many other mineral acids, and this observation I have always regarded as one of the main foundations of Law 1. MM. Favre and Silbermann, on the contrary, have inferred from their experiments that "the following organic acids —the oxalic, formic, valeric, and citric—disengage sensibly the same quantity of heat, but it is less (plus faible) than that given by the foregoing mineral acids" —among which they enumerate the nitric and hydrochloric. According to my experiments, no distinction of this kind can be admitted between acids derived from the mineral and organic kingdom, inasmuch as the oxalic acid developes at least as must heat in combining with the bases as the hydrochloric, nitric, and several other strong mineral acids.

The experiments to be described in this paper were made some years ago, but their publication has been deferred from accidental circumstances. I have, however, recently repeated a few of the more important of them, with a slightly modified form of apparatus. The solutions were taken in so dilute a state that the heat disengaged never exceeded 3·5° C. A standard solution of sulphuric acid was prepared and carefully analysed, by precipitating a given weight with a soluble salt of barium, and weighing the sulphate of barium. The strength of the alkaline solutions was adjusted with great care by means of this standard acid. The same solution of each alkali was employed in all the experiments, and the quantity used in each experiment was determined by careful weighing. The acid solution was of such a strength that, after being mixed with the alkali, an excess of two or three per cent of acid was present. The alkaline solution was contained in a light glass vessel, in which a large platinum crucible holding the acid was carefully floated. By giving a rapid rotation, by means of a light stirrer, to the acid solution in the platinum crucible, a perfect equilibrium of temperature was soon established between the two liquids. The initial temperature of the solutions was usually about 1·5° below that of the air, and the final temperature of the mixture about 1·5° above it. The corrections for the heating and cooling action of the surrounding medium were determined with great care. The mechanical process of adding the acid to the alkaline solution produced no change of temperature, and as the heat disengaged in the combination raised the liquid almost instantly to the maximum temperature, the whole correction required was for cooling. The first temperature was read one minute after the addition of the acid to the alkaline solution, the mixture being stirred during the whole of that time. If δ represents the correction, and ε the excess of temperature above the air in centigrade degrees, the value of δ will be given by the following expression :—

$$\delta = \varepsilon \times 0{\cdot}012°.$$

As a proof of the accuracy of the method of mixture adopted in this inquiry, I may mention that, being desirous to know whether the dilute acids employed in these experiments produced any change of temperature when mixed with water, I made the experiment with nitric acid by the method just described, substituting water for the alkaline solution, with the unexpected result of a fall of 0·01°. On varying the conditions of the observation, so as to obtain a larger effect, it was ascertained not only that a diminution of temperature had actually occurred, but that the observed fall represented approximately its true amount. When hydrochloric acid of equivalent strength was diluted to the same extent, an elevation of temperature of 0·05° was produced.

The accuracy of experiments of this kind, where the whole thermal effect observed amounts only to 2° or 3°, depends greatly on the thermometer employed. Unless its indications are perfectly trustworthy in every part of the scale, the labour of the inquirer will only end in disappointment. I have, therefore, taken every precaution to secure this important object. The tube of the thermometer was calibrated and divided with care, according to an arbitrary scale, by means of a dividing iustrument contrived for the purpose, and provided with a short screw of great accuracy made by Troughton and Simms. The divisions, etched finely on the glass, corresponded to about 0·05° C., and the readings could be made with certainty to less than 0·01°. The division of the scale, corresponding to 0°, was determined from time to time in the usual way; and another point, about 30° C., was fixed by comparison with four other thermometers similarly constructed, whose scales extended from the freezing to the boiling point of water. The readings of these four instruments, when reduced to degrees, rarely differed from each other within the limits to which they could be read, or 0·02°. The reservoir of the thermometer used in these experiments was 75 millimetres long, and, when immersed in the liquid, occupied nearly its entire depth.

As some uncertainty always exists with regard to the thermal equivalent of glass vessels, I made two sets of comparative experiments—one with a thickly varnished copper vessel, and the other with a vessel of platinum. The mean result of these experiments coincided almost exactly with the result obtained when the glass vessel was employed.

The weight of the glass vessel which contained the alkaline solution was 58 grammes, and corresponded thermally to 11·4 grms of the solutions formed. The thermal equivalent of the reservoir of the thermometer and of the stirrer was 0·9 grms. The alkaline solution weighed 160 grms, and contained the equivalent of 1·738 grms of SO_3. The acid solution weighed 42·5 grms. Hence the entire thermal value of the apparatus, in terms of the solution formed, was—

Solution	202·5
Glass vessel	11·4
Thermometer and stirrer	0·9
	214·8 grms.

A correction (additive) of 1-240th was made to the direct readings for the mercury in the stem of thermometer.

In the following table I have collected the results of my experiments, arranging the acids in the order of their thermal action :—

Acid.	Potash. Deg.	Soda. Deg.	Ammonia. Deg.
Sulphuric acid	3·378	3·353	2·976
Oxalic acid	3·058	3·040	2·648
Hydrochloric acid ..	3·021	3·982	2·623
Nitric acid	2·993	2·929	2·566
Acetic acid	2·852	2·832	2·492
Tartaric acid	2·732	2·710	2·376

It is interesting to observe how closely the results in the three vertical columns agree relatively with one another. The acids follow in the same order under each base, and even the differences in the amount of heat disengaged by the several acids in combining with the different bases approximate in many cases closely to one another. Thus the heat given out when the sulphuric acid combines with potash exceeds that given out when the oxalic acid combines with the same base by 0·320°, the corresponding differences in the case of soda and ammonia being 0·313° and 0·328°. If, in like manner, we compare the differences between the heat disengaged by the acetic and tartaric acids, we fall upon the numbers 0·012°, 0·122°, and 0·116°. Even in the case of the oxalic, hydrochloric, and nitric acids, which disengage so nearly the same amount of heat, the same order is observed with the three bases. It must be particularly remarked that the oxalic acid disengages from 0·022° to

0·058° more heat in combining with these bases than the hydrochloric acid, and from 0·065° to 0·111° more than the nitric acid. The conclusion of MM. Favre and Silbermann, that the organic acids (oxalic, formic, acetic, &c.) disengage sensibly less heat than the mineral acids, is thus entirely disproved; and the original results recorded in my work of 1841, according to which the oxalic acid disengages at least as much heat as the nitric, phosphoric, arsenic, hydrochloric, hydriodic, boracic, and other mineral acids (with the exception of the sulphuric acid) are fully confirmed. The tartaric, citric, and succinic acids, it is true (as was also shown in the same work), give out about $\frac{1}{14}$th less heat than the average of the other acids; but the acetic and formic acids fall scarcely $\frac{1}{30}$th below the mean, and the oxalic acid is always above it. These results, in all their main features, are fully corroborated by the experiments which were performed with a more perfect apparatus and a more exact thermometer than I had at my command in my earlier investigations. A reference to the same paper will show that, while acids, differing so widely from one another as the oxalic, phosphoric, arsenic, nitric, hydrochloric, and boracic acids, scarcely present any sensible difference in the quantities of heat which they disengage in combining with the bases; and while of the other acids examined the sulphuric acid (and probably also the sulphurous acid) presents an extreme deviation of about $\frac{1}{8}$th above the mean, and the tartaric acid group a deviation of about $\frac{1}{30}$th below it, the bases, on the contrary (and the subsequent researches of Favre and Silbermann have confirmed this result), differ altogether in thermal power from one another. Thus, equivalents of the oxides of magnesium and of silver give out 4·1° and 1·8° of heat respectively in combining with nitric acid, the former oxide having therefore 2·3 times the thermal power of the latter. Yet, as is well known, both these bases fully saturate the acid, and the resulting solutions are even neutral to test-paper. For these reasons, I have no doubt whatever that the first law, as enunciated in 1841, is the expression of a true physical law, and that in the combination of acids and bases in presence of water the heat disengaged is determined by the base and not by the acid. It is true that in this, as in similar physical inquiries, experimental results cannot immediately be obtained free from complication or disturbing influences. The same remark applies to the experimental proof of the great law discovered by Dulong and Petit, which connects the specific heats and atomic weights of the elementary bodies, and also to that of the remarkable relations discovered by Kopp between the composition and boiling points of many organic liquids. We have already seen an illustration of one of these disturbing influences, in the fact that dilute nitric acid, when mixed with water, gives a slight fall of temperature; hydrochloric acid, a rise; and the differences of specific heat in the solutions formed will to a small extent modify the results. But the cause of the higher thermal power of sulphuric acid I have not been able to discover, and future researches must decide whether it depends upon some disturbing cause, or, which is less probable, upon its possessing an exceptionally high thermal power. One condition is, however, essential, or Law 1 will not apply. The acid and base must be capable of combining when brought into contact, and of forming a stable compound. In the paper so often referred to, I showed that hydrocyanic acid and potash, which fail to fulfil this condition, do not disengage the normal amount of heat when mixed; and the same observation will doubtless be found to apply to a large number of metallic oxides, which form unstable compounds with, and imperfectly neutralise the bases.

As regards the experimental proofs of the other laws, even those of the fourth law, the truth of which is admitted by MM. Favre and Silbermann, they are only approximative; and here also we meet occasionally with peculiar and unexpected results. Thus a slight fall of temperature occurs, as Hess showed long ago, in the conversion of the neutral sulphate of potash into the acid salt; and I found, as indeed might have been expected from their alkaline reaction, that in the conversion of the ordinary phosphates and arseniates into super salts, a disengagement of heat occurs, amounting to about one-seventh of that disengaged in the formation of the salts themselves. In other cases, results, at first view startling and apparently anomalous, will be found to be strictly in accordance with the general principles already laid down. In the formation of double salts there is no disengagement of heat—a principle announced in 1841, and which ought perhaps to be enunciated as a distinct law, although it is implicitly involved in Law 2. Again, if tribasic phosphoric acid or arsenic acid is added in fractional portions to a solution of potash till the subsalts are formed, the heat disengaged on each addition of acid corresponds to the amount of acid added; but after this point has been reached, the disengagement of heat follows a different law. The pyrophosphoric acid, on the other hand, behaves in the same way as the nitric and most other acids, when added in successive portions to solutions of potash or soda; equal increments of heat being evolved for equal additions of acid, till the pyrophosphate of potash or soda is formed.*

Appendix.

In the following tables I have given the results described in this communication and those of 1841 in a form which admits of comparison with one another, and with those of MM. Favre and Silbermann. I have also added a few determinations recently made by M. Thomsen of Copenhagen.† It will be seen that the original experiments of 1841 exhibit, on the whole, a fair agreement with those now communicated From the small scale on which they were performed (the whole weight of the solutions after mixture being less than 30 grammes), the imperfect form of the apparatus, and the uncertainty of the thermometric indications, I have indeed been surprised to find them so near the truth. The results of MM. Favre and Silbermann do not exhibit the precision which might have been expected from the high character of those experimentalists, and from the accuracy of other parts of their great work. The mercurial calorimeter employed by them appears to have been little adapted to its purpose; but after making due allowance for its imperfections, I am at a loss to account for the serious errors into which they have fallen. M. Thomsen's experiments have evidently been made with care, and his results agree comparatively with my own; but the absolute amount of heat obtained by him falls far short of what I have found. It is indeed much easier to obtain results relatively than absolutely correct. The numbers given in this paper will, I believe, be found rarely to differ *relatively* more than 1-200th from the truth, but they may hereafter require a small correction in respect to their *absolute* value. That correction can, however, be scarcely more than $\frac{1}{50}$th of the whole amount; and I have little doubt that the number, for example, given by Thomsen to express the heat disengaged in the combination of soda with nitric acid will prove to be as far below the true number as that given by MM. Favre and Silbermann is above it.

Table I.—*Potash.*

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.
Sulphuric ..	16,330 ..	16,083 ..	16,701
Nitric	15,076 ..	15,510 ..	14,800
Hydrochloric .	14,634 ..	15,656 ..	14,940
Oxalic	14,771 ..	14,156 ..	15,124
Acetic	14,257 ..	13,973 ..	13,805
Tartaric.. ..	13,612 ..	13,425 ..	13,508

* *Transactions of the Royal Irish Academy*, vol. xix. pp. 245—248. The observations of Graham confirm the statement that no heat is evolved in the formation of any double salt. *Memoirs of the Chemical Society*, vol. i. p. 83.

† *Poggendorff's Annalen*, cxxxviii. p. 78.

TABLE II.—*Soda.*

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.	Thomsen.
Sulphuric ..	16,483 ..	15,810 ..	16,580 ..	15,689
Nitric	14,288 ..	15,283 ..	14,480 ..	13,617
Hydrochloric	14,926 ..	15,128 ..	14,744 ..	13,740
Oxalic.. ..	14,796 ..	13,752 ..	15,032 ..	—
Acetic.. ..	14,046 ..	13,600 ..	14,000 ..	—
Tartaric ..	13,135 ..	13,651 ..	13,400 ..	—

TABLE III.—*Ammonia.*

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.
Sulphuric ..	14,135 ..	14,690 ..	14,710
Nitric	12,440 ..	13,676 ..	12,683
Hydrochloric .	12,440 ..	13,536 ..	12,964
Oxalic	12,684 ..	— ..	13,088
Acetic	12,195 ..	12,649 ..	12,316
Tartaric.. ..	11,400 ..	— ..	11,744

ANALYSIS OF A NEW MINERAL FROM BURMAH.

By D. WALDIE.

DURING the period extending from November, 1863, to the end of 1864, I had various samples of metallic ores sent to me for analysis by Mr. O'Riley, the Deputy Commissioner of Martaban, Burmah. They were mostly samples of galena, but one of a different kind particularly attracted my attention as of rather unusual composition, so that I suggested to him that it might be desirable to publish it. To this proposal he assented, suggesting that it should be presented to the journal of the Asiatic Society. Circumstances at the time prevented me from carrying my proposal into effect, but recently I resumed the investigation which had been lying long incomplete.

The analysis of the sample first sent by him on the 24th of July having been unsatisfactory on one point, and the specimen having been exhausted, I wrote to Mr. O'Riley for another sample, in order to settle this point. In reply he said that he had only a small specimen left, but sent me another small piece from the same range of hills, bearing a strong resemblance to the first, which he thought might probably be the same. I have no information of the locality whence they were got. Mr. O'Riley's letters were all dated from Shoaygyeen, except one in February, 1864, from the Karen country. In a subsequent letter, he mentioned that the samples referred to were from the same range of hills as a sample of ore he was then sending me, which turned out to be a double sulphide of copper and iron. This is all the information I can give of their source, as some time afterwards Mr. O'Riley died.

The following is the result of my analysis of the first sample sent on July 24th.

Copper	17·000
Silver	0·096
Iron	36·470
Antimony	1·150
Arsenic	32·700
Sulphur	1·360
Deficiency and loss	10·624
Earthy matter	0·560
Total	100·000

The silver is equal to 31¼ ounces, troy, per ton.

The unsatisfactory point which I wished to clear up was the deficiency of 10·624, which I supposed might be oxygen combined with the metals. But this did not appear a very probable solution of the difficulty, and it might rather be owing to errors in analysis. The determinations had all been carefully made according to the usual methods. The arsenic and antimony were separated from the other metals by hydrosulphate of soda, and the arsenic determined as arsenate of magnesia and ammonia, and there was no reason to doubt the correctness of the process. But I had some fear that arsenic might have been lost during the operations preparatory to its separation from the other metals, and an experiment made on the second sample by conducting the analysis in the same way gave support to this view, as by this plan only 31·5 per cent of arsenic was obtained, instead of the 37 per cent indicated below by another process. Probably arsenic had been volatilised as chloride.

The second sample sent by Mr. O'Riley, October 11th, was similar in appearance to the first, but differed somewhat in composition, as will be seen presently. No particular note had been taken of the physical properties of the first sample. The second one was in the form of a flattened piece about ⅛th of an inch (or 1·2 centimetres) thick, with a dull, blackish, earthy looking surface. When broken it presented an uneven fracture, of a laminated structure, somewhat cellular, of a steel grey colour with a purplish tint and metallic lustre. In general appearance it is like mispickel, but of a redder shade. Minute specks of brownish green matter could be seen here and there on the surface, particularly between the lamellæ, when these presented themselves to view edgewise. It gives no streak on paper, but a dark grey one on unglazed porcelain. Hardness, 5·5.

Specific gravity at 81° F. (27° C.).

In small pieces	7·343
In powder	7·428

The pieces were boiled in the bottle, but no doubt still retained air in some interior cells.

It is easily soluble in nitric and nitro-hydrochloric acids, with evolution of nitrous fumes. One portion was dissolved slowly by diluted nitric acid containing 3 per cent its volume of nitric acid of 1400, and the solution completed somewhat more rapidly by a solution containing 5 per cent its volume. Hydrochloric acid at atmospheric temperature dissolved it partially by standing some time (two or three days), to the extent of about 10 or 11 per cent, and by repeated boiling about 13 per cent more, but there appeared no definite limit to the action. Acetic acid dissolves a portion, evidently oxidised matter.

Ignited in a platinum crucible it caked together, lost its metallic lustre, and became of a brownish colour, but whitish at the edges where it adhered to the crucible and was removed with some difficulty, having slightly attacked the platinum. By this ignition it increased nearly 2 per cent in weight. Ignited in a small glass tube by the blowpipe till the glass softened, it did not appear to yield any arsenic.

In the analysis of this sample, the arsenic (with a little antimony) was separated from the other metals by fusing with nitrate of potash, and carbonate of soda (potassium nitrate and sodium carbonate), or by passing chlorine into the mineral mixed with solution of potash. As in this case, however, the action was very slow, the mineral was first oxidised by a little nitric acid, then mixed with solution of potash in excess and chlorine passed through it. This plan answered very well. The results of two analyses for the three principal constituents, agreeing very well, were as follows:—

Copper	13·28
Iron	43·88
Arsenic	37·03

A complete analysis was made by digesting a portion for about twelve hours with diluted hydrochloric acid, and thus removing the oxidised matters. The results were as follows:—

Soluble in dilute hydrochloric acid.

Oxide of copper	1·21
Protoxide of iron	1·97
Oxide of lead	1·89
Arsenious acid	1·12
	6·19

Insoluble.		
Copper	12·13	
Iron	42·12	
Arsenic	38·45	
Antimony	0·54	
Earthy matters	0·12	
		93·36
		99·55
Loss		0·45
		100·00

In one small piece I found 2·67 per cent of matters insoluble in nitro-muriatic acid, but generally it was very small.

It will be observed that this sample differs from the first in the smaller proportion of what may be considered accidental constituents, and is a purer specimen of the essential constituents, arsenic, iron, and copper. The inside pieces contained no sulphur; the outside crust yielded a trace probably in the state of earthy sulphate. And while the first sample contained a notable quantity of silver, this did not appear to contain any, or at least so little that I could not detect it in the amount of material at my disposal. The quantity of antimony was also less than half that of the first sample.

I have not been able to find in any book on mineralogy I have had access to a description of such a mineral. The nearest are arsenical iron pyrites (mispickel) and axotomous arsenical iron. But it differs from the former in the total absence of sulphur, and from both in the presence of a considerable quantity of copper, as well as in the larger proportion of iron, and it differs still more in the proportion of the two basic metals together to the arsenic, the latter being small in proportion to the former.

The constituents approximate, though not very closely, to two equivalents of arsenic, six of iron and one of copper; rather more than six of iron and less than one of copper. This can scarcely be reduced to any probable atomic formula; but if the proper metallic nature of arsenic be admitted it may be considered as an alloy, and alloys are not limited in their composition to definite formula. The excess of basic metals in its composition gives it a fixity under the action of heat not very usual in arsenides or unoxidised arsenical compounds.

I would venture to propose for this mineral the name of O'Rileyite, in honour of the gentleman who sent it to me, whose services have unfortunately been lost to the Indian Government by an untimely death. This notice may perhaps lead explorers of these districts to discover additional specimens of this or analogous minerals.

ON THE

EXAMINATION OF THE BESSEMER FLAME WITH COLOURED GLASSES AND WITH THE SPECTROSCOPE.*

By J. M SILLIMAN, M.E.,
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(Concluded from vol. xxii., p. 314).

II. Examination With the Spectroscope.

THE science of spectrum analysis is yet in its infancy, and there has been no scientific investigation, perhaps, which has been more contradictory in its results than that of the Bessemer flame. The first application of the spectroscope to the analysis of the Bessemer flame was made in 1862 by Dr. Roscoe at the works of Messrs. John Brown and Co., in Sheffield. Soon after this it was in constant use in Brown's works for controlling the process. It was next introduced at Crewe, and from there said to have been taken to Seraing, in Belgium, in 1865.

Roscoe's account of the general appearance of the spectrum has not altogether been verified by subsequent observers. His not having seen any line beyond 80° indicates an imperfection in his instrument. He, also, is the only one who claims to have seen the sodium line as an absorption-band, or who professes to have detected the lines of nitrogen and hydrogen in the Bessemer spectrum. His spectroscope was so arranged that the spectrum of the Bessemer flame was seen in the upper half of the field of view, while the spectrum with which it was to be compared was seen immediately below. The spectrum of the flame was thus compared with the following spectra:—

1. Spectrum of electric discharge in carbonic oxide vacuum.
2. Spectrum of strong spark between silver poles in air.
3. Spectrum of strong spark between iron poles in air.
4. Spectrum of strong spark between iron poles in hydrogen.
5. Solar spectrum.
6. Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were very few, and totally failed to explain the value of the Bessemer spectrum. The lines of the well-known carbon spectrum did not occur at all, either as bright lines or absorption-bands, nor was any coincidence observed between the lines of the Bessemer spectrum and those of the carbonic oxide vacuum tube. The lines of lithium, sodium, and potassium were strongly marked and identified with certainty. He found that three fine bright lines between E and *b*, shown on the plate at 66½°, 67°, 67½°, coincided with those of iron; and in place of the red hydrogen line C, he discovered a black band, which he considered an absorption-band, and states that it is better defined in wet than in dry weather.

In Austria, Professor Lielegg followed up this subject with great perseverance, and gave more extended accounts of the varying character of the Bessemer spectrum during the different stages of the process. His experiments were made at Gratz, where the spectroscope was afterwards used with great success in controlling the Bessemer process; but at Königshütte, where dark gray manganiferous iron was used, it was found that the indications which in other works so plainly determined the moment of decarbonisation were unreliable. In this case, the lines whose disappearance is to indicate the exact point of time for ending the process, disappear too soon. During the period in which the spectrum is brightest, among the glowing vapours and gases that stream from the converter, carbonic oxide, next to nitrogen, is most abundant, and it is for this reason that the first investigator, Roscoe, expressed himself as confident that the numerous lines of the spectrum were caused by this gas, although he could obtain no coincidence.

Brunner* states that "no part of the Bessemer spectrum is ever visible in the flame when the converter is heated for the first time after being re-lined, but that when the lining is not new, Lielegg's group of green lines ($CO\gamma$) appears in the spectrum, which then contains also the lines of potassium, sodium, and lithium." From which he concludes that this spectrum is not to be identified with carbonic oxide, but must be produced by other constituents of pig-iron. Others state that the Bessemer spectrum is sometimes visible while the converter is being heated after a blow. I made an observation of the flame from the converter while it was being heated the first time after being re-lined, and obtained with great distinctness the potassium, lithium, and sodium lines, but have not under any circumstances detected any other lines while the converter was being re-heated.

* Read at the Troy Meeting of the American Association for the dvancement of Science.

* Van Nostrand's *Eclectic Eng. Mag.*, vol. i., page 508.

Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum, found the lines in the blue and green fields to completely harmonise in the two spectra. The violet manganese line which had been seen by some he could not detect in either of the spectra. I have never observed it, but Dr. Wedding, who has summed up the observations of others, states that he has repeatedly seen it. Its position is at 135½°.

The instrument used in my investigations was constructed by Alvan Clark, of Cambridge, and consists of an equiangular flint-glass prism, in a metallic box, into the sides of which at the requisite angles are screwed an inverting telescope with a magnifying power of six, and a tube containing the adjustable slit and lens for rendering the rays parallel; also a tube with a scale, which is placed at such an angle that it is reflected from the surface of the prism through the telescope to the eye; it can be so adjusted as to appear along the upper edge of the spectrum. I was provided with Bunsen's plates of spectra on a large scale, and in order to adapt them to the scale in my instrument, I took the spectrum of the sun and obtained Fraunhofer's lines with great distinctness. Two characteristic lines in the solar spectrum were then noted, one of which appeared at 37° and the other at 117°, and a space measured equal to their distance apart as given on Bunsen's scale. This was divided into eighty equal parts, and the division extended in both directions. By the application of this scale to Bunsen's, I found that the remainder of Fraunhöfer's lines in my instrument exactly coincided with their position on his plates. The correctness of the new scale was also proved by other coincidences. By moving the prism, Fraunhöfer's lines will vary slightly in their relative distances apart, but in no possible position in which I could place the prism could I obtain the sun-spectrum as given by Wedding in connection with the Bessemer spectrum; if the spectrum given by him was obtained by the use of bisulphide of carbon in his prism, that substance causes a greater variation than I had supposed.

I have recorded the results of twenty-five observations on the Bessemer flame, most of which were taken at a distance of about thirty feet from the flame, though I have stationed myself at intermediate points between that and the flame; at one time sitting so close as to be almost scorched. Nearly all my observations were made at night and the lines obtained much better defined than when seen in diffused sunlight.

The record of my observations was kept as follows:—Five columns were ruled, headed—

Degree.	Colour.	Brightness.	Time.	Remarks.

Note was made of the dark bands as well as the bright ones, both of which were classed according to their distinctness, as very bright, bright, faint, and very faint. In the time column was noted the number of minutes after the commencement of the blow at which the lines appear.

At the first two or three observations I attempted to make a thorough note of the changes as they occurred throughout the whole spectrum, but afterward abandoned it as utterly impossible, as at the beginning of the second period, the lines come in so fast and the changes are so rapid that they cannot be accurately noted at the exact moment of their occurrence. I therefore confined myself to a few degrees at each observation, and by this method was enabled to note accurately, and at the exact moment of their occurrence, slight changes which otherwise might have escaped notice. Note was also taken of the changes in the general appearance of the whole spectrum during the successive stages of the process. After having made half a dozen observations, while viewing the spectrum of the flame from the converter while it was being heated for another charge, it was discovered that a movement of the eye before the eye-glass occasioned a similar movement of the lines of the spectrum along the scale, on which their position could thus be made to differ more than half a degree. I have seen no notice of this in the statements of others, and it may account for some of the apparent discrepancies. Thereafter, when taking the readings of any of the lines, the position of the eye was so adjusted as to bring the sodium line exactly at 50°. Owing to the extreme brilliancy of the flame the aperture may be made exceedingly narrow, and thus the many lines of the spectrum, which with a duller light and broader gauge would be blended together, may be separated.

At the beginning of the blow, the spectrum is continuous and very faint, and generally extends from 35° to 120°, covering about three-fourths of the length attained in the second period. This increases slightly in extent and brightness until the appearance of the sodium line. This line appears at the end of the first period at the beginning of a more decided flame. It comes flashing through from one extremity to the other for an instant, and then disappears only to return the next instant in brighter flashes, which are continued for about a minute, by which time the line becomes permanently established. On one occasion the sodium line, instead of flashing and disappearing as usual, continued visible after a few seconds, and expanded and contracted in width almost isochronously until it became permanently established. The appearance of this line indicates the termination of the first period. This period I have found to vary in extent from three to seventeen minutes in blows lasting from thirteen to twenty-seven minutes. None of the other lines make their appearance in vivid flashes as does the sodium. The lithium line becomes visible three or four minutes after the first flash of the sodium. It is very faint at first but soon becomes quite distinct, and lasts through the blow. The vivid flashing of the sodium line may be accounted for by the exceedingly small amount of sodium required to produce its spectrum—an amount not exceeding 1-180,000,000th of a grain. The slightest momentary combustion taking place in the stream of gas from the converter, would at that instant render glowing a sufficient amount of the vaporised sodium to produce its spectrum, and thus occasion the flashes so characteristic in the first appearance of that line. Lithium exists in a much smaller quantity, and requires 1-6,000,000th of a grain, or thirty times that given for sodium. By the time the lithium line is established the red potassium line at 23½°, and occasionally the violet line at 135°, appear, and the blue and green fields become divided into bands, which are so rapidly resolved into bright and dark lines, that it is difficult to note the exact time of the appearance of each. The spectrum increases to a dazzling brightness, and extends itself in both directions until it reaches from 23½° to 140°.

During the third period the spectrum becomes more brilliant, and the lines more distinct. Several new lines make their appearance in different parts of the spectrum, of which the ones at 51½°, 57°, and 67° are well defined, while others are faint and not always visible; some of them appearing only toward the close of the last period. In viewing the lines in the most refracted part of the spectrum, it has been repeatedly observed, both by myself and others, that these lines were more strongly marked when entering the eye at an angle than when viewed directly. That this was not imagination is proved by repeated identification of lines at the same point on the scale.

At the termination of the blow the lines are rapidly swept away, sometimes in the inverse order of their appearance, but more generally they disappear within the space of two or three seconds, leaving a continuous spectrum as at first, though somewhat brighter. Sometimes the sodium and lithium lines are swept away with the others, and at other times they remain visible. In either case the change is very decided, and does not generally occupy more than three seconds. In the course of my observations, thirty-three lines have been detected, as given in the table below.

Some of the lines given by Lielegg I have failed to find, but have detected others not given by him.

1st Period, 23½, 35, 50, 135.
2nd Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 52, 53, 56, 56½, 61½, 62, 62½, 63, 65, 66½, 67½, 70, 72, 120, 135.
3rd Period, 23½, 35, 43, 44, 44½, 45½, 46, 47½, 48½, 50, 51½, 52, 53, 56, 56½, 57, 61½, 62, 62½, 63, 65, 66½, 67½, 70, 72, 100, 102, 103, 105, 108, 135.

Among the dark bands detected, the most intense occurred at 44—46, 51—55, 56—58, 62—64½; others were found at 33—34½, 36½, 37½, 38½, 40, 68—72.

Many of the dark bands were crossed by bright lines.

I have repeatedly observed the dark band considered by Roscoe to be a hydrogen absorption line, but have not noticed that its intensity varied with the dampness of the weather. Whether it is an absorption-band or not can be determined by a series of observations continued through wet and dry weather. If this proves to be a hydrogen line, the Bessemer spectrum will be found more complicated than is generally supposed. It has been thought by some that the dark bands in the spectrum are absorption lines due to the cooling of the outer sheath of flame, but it is more probable, that although the pellets of iron and slag tend to produce a faint continuous spectrum; yet in contrast with the very brilliant lines it appears discontinuous, the dark bands being merely intervals between the bright ones. The iron spectrum has not been satisfactorily identified. It has been suggested that the brightness and size of the lines of the Bessemer spectrum do not allow the iron lines to appear. In comparing the Bessemer spectrum with Bunsen's spectra of nickel, cobalt and calcium, no coincidences were observed except two or three in the latter spectrum. The brightest calcium line, however, was not visible in the Bessemer spectrum. The Bessemer spectrum contains yet many mysteries to be solved, among which is the cause of the non-appearance of the lines of the spectrum at the beginning and termination of the blow.

This was readily solved when the numerous lines of the spectrum were attributed to carbon, but in proving them to be caused principally by manganese, their disappearance is not so readily accounted for.

One theory to account for it is that the luminous power of the flame is too small at the beginning and end of the process to produce a spectrum. In regard to this it may readily be shown that the brilliancy of the spectra of incandescent metallic vapours does not depend upon the illuminating power of a flame, but upon the heat of the flame into which they are introduced. For instance, the spectra are more distinct in the non-luminous flame of a Bunsen lamp than in the ordinary luminous gas-flame. If we take the theory as referring to the feebleness of light given off by those substances in the flame which produce the spectrum, it will resolve itself into the one of change of temperature, notwithstanding the fact that the illuminating power of flames of the same temperature varies with the composition of the gas, because there is evidently enough sodium in the flame to give its characteristic line; hence, whatever might be the illuminating power of the flame, if the heat is sufficiently intense the sodium line *will* show itself.

Dr. Wedding adopts the theory that the absence of the spectrum at the beginning and termination of the blow is because the absolute quantity of the bodies volatilised producing the spectrum is at these times too small. His reasons for holding this view are as follows:—"A trace of sodium will give its characteristic line, but, according to Simmler, a much larger quantity of manganese is needed to obtain a recognisable reaction than that which can be detected by the well-known blowpipe reaction with carbonate of soda. Consequently, spectrum analysis does not depend alone upon the *presence* of a body, but also upon the presence of a certain quantity. And although manganese is always left in the iron, it may not be left in sufficient quantity at the termination of the blow to produce the spectrum, and for this reason the lines disappear."

To this theory there are some strong objections. 1st. If we take the manganese in sufficient quantity, and hold it in a flame the spectrum will increase in brightness until a uniform temperature is attained; but when the amount of manganese vapourised begins to diminish, its spectrum will gradually decrease in brightness until it disappears. Now, if the disappearance of the manganese lines in the Bessemer spectrum is owing to the diminution of the quantity of manganese, we should infer that these lines would gradually grow more indistinct and then fade away; but, on the contrary, the manganese spectrum increases in brilliancy from its first appearance, and is more intense just before being swept away than at any other time. The analysis of the smoke, which appears when the flame ceases, proves that a considerable quantity is still volatilised, and it is notable that in manganiferous iron this quantity increases towards the close of the blow. 2nd. It would be more difficult to account by this theory for the non-appearance of the sodium line at the beginning of the blow, as sodium then in all probability exists in the issuing gas in sufficient quantity to produce its spectrum at a high temperature, as it is only by special precaution that we can keep it out from any flame. 3rd. A still greater difficulty would arise in applying this theory to the spectra of sodium and lithium at the close of the blow. As has before been stated, these lines sometimes disappear at the moment of complete decarbonisation, and sometimes remain. In the former case, to say that our friend sodium had given out would be doing great injustice to that element, as it has never given us reason for bringing so grave a charge against it. Dr. Wedding, in attempting to demonstrate that the non-appearance of the manganese lines is owing to the lack of sufficient quantity volatilised to produce its spectrum, makes the following statements:—

From analyses made by Brunner we find that the manganese contained in the iron falls from 3·460 per cent in the raw material, to 1·645, 0·429, and finally to 0·113 per cent in the decarbonised product; and that the protoxide of manganese in the slag first increases from 37·00 per cent to 37·90 per cent, and then sinks to 32·23 per cent, and furthermore, that a certain quantity of manganese is to be found in the smoke. How much manganese is really lost by volatilisation cannot be determined, since data are wanting as to the absolute quantity of slag and iron; consequently we cannot determine how much manganese has been lost by means of the eruptions.

But since the manganese contained in the pig-iron decreases constantly, and that contained in the slag after the termination of the boiling period also decreases, a considerable volatilisation of this body is probable just at the time when the spectrum is best developed. Comparing with this the experiments that can be made in the laboratory we arrive at the hypothesis, that the oxidised manganese which has entered into the slag is not volatilised but is retained by the slag; it can, therefore, get into the flame only in the shape of solid or fluid combinations.

In the above statements the results of the analysis prove that some of the manganese in the slag is volatilised. We cannot consider the manganese spectrum during the entire process as due wholly to the volatilisation of the manganese directly from the iron, for while the amount eliminated from the iron grows continually less, the manganese spectrum grows brighter. Owing to the intimate mixture by the blast of the iron and slag, the manganese oxide contained in the latter is brought in contact with the melted iron and vapourised. This mixing of the slag and iron would cease at the termination of the process, and this would account for the sudden diminution of smoke.

If there was a sufficient carbonic oxide flame to render the escaping gases glowing it is evident they would not issue from the converter as dark smoke, but as incandescent vapour having its characteristic spectrum. The lack of sufficient flame may, therefore, account for the disappearance of the manganese spectrum. The Besse-

mer flame presents other problems, and opens an intensely interesting field for scientific investigation; and by the use of more delicate instruments than have yet been employed for this purpose, discoveries may be made which will throw new light upon the subject of spectrum analysis.—*American Journal of Science.*

ELECTRO-MOTIVE FORCE.

By the Rev. H. HIGHTON, M.A.

The subject of electro-motive force is so important, and the prevalent idea, that only a definite amount of work can be got out of a definite amount of chemical changes, has so completely blocked the way against a true consideration of the subject, that I think it may be useful if I make a few remarks on the nature of this force.

Let me first contrast it with the force of attraction of gravitation.

There is then, *first*,—this wonderful difference; that the force of gravitation between two bodies varies inversely as the square of the distance between them, whereas electro-motive force is equal at all distances. The electro-motive force, developed between a plate of zinc and one of copper, 1-10th of an inch apart, is exactly the same, neither more nor less, than if one is in America, and the other in England; and if one were on the earth, and the other in the sun, or in the most distant fixed star, there is no reason to suppose it would be any less. This is a fact which will, of course, be acknowledged by every electrician, but I may as well say that I have tried a plate of zinc, placed at 10 miles distance from a plate of copper, and found the electro-motive force exactly the same as when they are 1-10th of an inch apart. If we could cut the earth into two hemispheres by an insulating plate we could transmit any amount of force, or any telegraph message, from any point in one hemisphere to any point of the other without the slightest difficulty. The resistance in that case would be next to *nil*; and as the magnetic and chemical forces transmitted by a circuit are only limited by the resistance of the circuit, almost any amount of power could be transmitted. As we cannot thus cut the earth in two by an insulating plate we are obliged in transmitting magnetic or other power from England to America to use the sphere of the earth for one-half of the circuit, and an insulated wire for the other half; and it is the resistance of this wire which limits the amount of force transmitted. Similarly, on the supposition of being able to form a circuit by cutting into two insulated hemispheres the sphere of stellar space, we could transmit any amount of magnetic force to the furthest star. Nor is this case wholly an imaginary one. As a matter of fact, we have in nature an instance of two enormous conductors with an insulating band between them. There is the earth and sea, which form one conductor; and at a certain distance from them there is a second conductor, consisting of the upper and rarefied strata of the atmosphere, with an insulating band of denser air between them. Now, when, from unknown causes, the electric equilibrium between a pole and the zenith is disturbed, a current passes from the zenith to the pole through these rarer strata of the air, and a corresponding current passes through the earth and sea in an opposite direction. This, of course, is the well-known phenomenon of the aurora with its corresponding earth currents which are so annoying in the electric-telegraph. Whether the circuit is completed through vertical strata of the atmosphere at the equator and the pole, rendered conducting by vapours or other means; or whether there are simply two electric discharges from the zenith to the pole, one in the upper regions of the air, the other in the earth and sea, acting inductively on one another, we cannot be sure. It may be right to add that in reality the force of gravity is the same at all distances; but inasmuch as it is a force radiating from a centre, the portion which acts on any given surface varies inversely as the square of the distance, because the whole force extends itself over a space varying as the square of the distance, and, therefore, a given surface receives so much less of it at a greater distance; but the force itself is equal at all distances.

Secondly. There is this important difference between gravity and electro-motive force; that the one acts only in straight lines, whereas the other appears to be wholly indifferent how tortuous its course may be. Provided only there are two channels of communication between two metals; one forming a liquid communication, and the other a metallic or gaseous conductor; the straightness of the two channels makes very little difference.

Now observe the enormous practical importance of this point. Suppose we want to avail ourselves of the force of gravity as a motive power to do work. The work produced is as the mass on which it acts multiplied by the space (or square of the time) through which it acts. Thus, if we use it to move clockwork, and want it to make the clock go for a very long time, we must raise a given weight to a proportionate height; and the more power we want to get from it the higher we must raise the weight in the first instance. Then, the force of gravity, acting through a longer space and longer time, will produce so much more work; and the work done is in proportion to the perpendicular height to which the weight is raised, measured in a straight line. But if we want to increase the work we can get out of the electro-motive force of a pound of zinc, and for this purpose increase the time and space through which it acts, we may place it as near as we like to a plate of copper (indeed, the nearer the better), and increase the space and time through which the electro-motive force acts by convoluting in any way the wire which connects the two metals. In this way, by increasing the length of the conductor, and so increasing the space and time through which the electro-motive force acts, we can increase, to any extent, the work obtained. And it is in this way, and owing to this peculiar property of electro-motive force, that we can get any amount of work whatever even from a single grain of zinc. It is strange, but it is true. Scientific men have hitherto (generally, though by no means universally), failed in recognising this wonderful fact, in consequence of that most fallacious idea of a definite mechanical equivalent of heat. It is surprising how it has grown into a common axiom with scarcely the slightest foundation in fact. Not a few scientific men have asserted, more or less directly, that the power to be got from electro-motive force is without limit; but of late years they seem to have been driven out of their position by the assumption of a definite mechanical equivalent for heat and chemical change. I have entered more fully into this question in the January number of the *Quarterly Journal of Science*, but let me here give a single fact which wholly overthrows the supposed action—at least in a practical point of view.

Take three cylinders and pistons; one filled with atmospheric air, another with vapour of water (or steam), and the third with vapour of turpentine. Impart to each a definite number of heat-units or calorics. This amount of heat will produce, say, one unit of work, or will raise an unit of weight one unit of height by means of the third cylinder and piston; but in the first cylinder and piston it will raise the same weight ten times as high, and in the second 6·6 times the same height. Of course, it is easy to answer that in the one case it has also expanded vapour of turpentine, whereas in the other cases it has only expanded steam or air, and that this is to be considered as work. But the answer is simple; that if this be work, it is not work in the ordinary and practical acceptation of the term, namely, units of weight raised units of height.

I will show at another time how the work done by a given quantity of heat may be increased indefinitely.

Putney, December 18, 1870.

EXTRACTION OF POTASH FROM THE "SUINT" OR POTASSIC SUDORATE IN SHEEP'S WOOL.*

By J. LAWRENCE SMITH.

This source of potash is not known in this country, and hardly anywhere else than in a certain part of France. Potash from this source formed an article of exhibition in 1867, as well as in 1862, and as the source is a curious one, and the process of obtaining it is not generally known, there is sufficient reason for describing it to the American technologist somewhat in detail. The nature of this source of potash was reported on in 1862, for the London Exhibition, by Hofmann, and free use has been made of this description.

It is well known that sheep draw from the land on which they graze a considerable quantity of potash, which, after circulating in their blood, is excreted from the skin with the sweat, in combination with which it is deposited in the wool. Chevreul pointed out that this peculiar compound, by the French called "suint," forms no less than a third of the weight of raw merino wool, from which it may be readily dissolved out by simple immersion in cold water. In coarser wools it is less abundant, and, according to MM. Maumené, and Rogelet, the potassic sudorate or suint of ordinary wools forms, on the average, about 15 per cent of the weight of the raw fleece.

This compound was formerly regarded as a soap; doubtless because wool contains beside the suint a considerable proportion (about 8½ per cent) of greasy matter (Chevreul). This grease, however, is, in fact, combined with earthy matter, chiefly lime, as an insoluble soap. The soluble sudorate is, according to MM. Maumené and Rogelet, a neutral salt, resulting from the combination of potash with a peculiar animal acid, of which little is known beyond the fact that it contains nitrogen.

At the great seats of the woollen manufacture in France, as at Rheims, Elbœuf, and Fourmies, the new industry of MM. Maumené and Rogelet is either established or in course of establishment. Their plan is to buy of the woollen manufacturers the solutions of suint obtained by the immersion of their raw fleeces in cold water; paying higher, of course, for those liquors in proportion as they are stronger; thus, for example, for the suint from a ton of wool they pay five francs, if diffused through 27 hectolitres of water (sp. gr. 1·030); whereas, for the same quantity of suint they can afford to give no less than 18 francs, if it be concentrated in 3 hectolitres of water (sp. gr. 1·250); and so in like proportion for the suint liquors of intermediate strength.

An ordinary fleece, weighing 4 kilogrammes, contains, according to MM. Maumené and Rogelet, about 600 grms. of sudorate of potassium or suint. This, according to their analysis, should contain 33 per cent of its weight, *i.e*, 198 grms. of pure potash. Of this, according to another estimate (showing the nitre it would produce), they appear to reckon on about 173 grms. as being practically recoverable.

The wool manufacturers of Rheims wash annually 10,000,000 kilos. of fleeces, those of Elbœuf 15,000,000 kilos., and those of Fourmies 2,000,000 kilos.—total 27,000,000 kilogrammes, the produce of 6,750,000 sheep. From this quantity, were it all subject to MM. Maumené and Rogelet's treatment, 1,167,750 kilos. of pure potash would, according to the above ratio, be recoverable. The value of the potash, as carbonate, reckoned at the average price of American potash, would range between 400,000 dols. and 450,000 dols. The wash-water yielding it, if paid for at MM. Maumené's and Rogelet's minimum price, would cost about 100,000 dols. Hence it appears that the process of MM. Maumené and Rogelet may be worked on a large scale and with very ample profit. MM. Maumené and Rogelet compute that there are 47,000,000 sheep in France—nearly seven times as many as those above calculated on. And they point out that if the fleeces of these were all subjected to the new treatment France would derive from her own soil all the potash she requires; enough, they observe, to make 12,000,000 kilos. of commercial carbonate of potash, convertible into 17,500,000 kilos. (about 17,500 tons) of saltpetre; with which, as they characteristically add, 1,870,000,000 cartridges could be charged. The difficulty of collecting the wash-waters of fleeces, scoured in small numbers by the farmers all over the country, is a great bar to such an extension of the process.

* From "The Progress and Condition of Several Departments of Industrial Chemistry," by J. Lawrence Smith.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from vol. xxii., p. 317.)

Lecture III.

In referring, at our last meeting, to the place in nature which ought to be assigned to these little organisms of which we have been speaking—the ferments—I stated one ground which appeared to me conclusive, or very nearly so, in favour of placing them in the animal and not in the vegetable kingdom. That ground was a chemical one, viz., that these organisms assimilate, or to use a homely phrase, they feed upon very complex substances, and they give off, during their vital functions, less complex substances. That circumstance appears chemically conclusive in favour of their being rather animals than plants, for plants build up complex substances, and animals assimilate the products which plants have formed, and break them up into simpler ones. There are, however, two other considerations which I think are of such importauce that it would be undesirable to pass them over, which tend in the same direction, and are striking confirmations of the conclusion to which we then came. The one is that, whereas plants require for their growth the light of the sun—in fact, their very growth is a process of absorption of heat by their leaves from the rays of the sun—and plants by doing so render heat latent, as we sometimes express it, that is, they cause an apparent disappearance of heat, and lower the temperature of the surrounding space; animals, on the contrary, give off heat during the exercise of their vital functions, and do not need to be exposed to heat or to continuous light for their growth. Now, in both these respects, as in the other respects, these little cells, the ferments, appear to be distinctly animals. I do not know of one case of a ferment requiring or using for its vital processes the light of the sun; they usually grow, and they seem to thrive quite well, in the dark. Again, there are well-known cases in which, during their vital functions, they evolve or give off heat, so that I think these are very overwhelming reasons for not considering them as vegetables in their functions, but rather as animals or animal atoms. I have on the table here three or four liquids, which are in states of fermentation, of which I have already had occasion to speak several times. This first carboy contains an extract of malt, to which common cane sugar has been added, and some brisk, thriving yeast was then introduced. Effervescence is now rapidly going on, as you may hear by the gas—carbonic acid—which is escaping through the bent tube into the vessel containing lime water. This liquid contains little soft, nearly round particles, which I was just speaking of as animals, though they certainly do no look like animals. The second flask contains another substance, of which I also spoke the other evening. There is here what I might call gastric juice—it is a mixture made for the purpose of getting lactic acid from sugar. Some pepsine was made to digest a certain quantity of white of egg,

* The Cantor Lectures. Delivered before the Society of Arts.

and to that mixture, whilst still acid, I mixed with some common cane sugar, and put into it some alcoholic ferment, or common yeast. A good deal of the yeast was digested; it disappeared, and was dissolved. I thereupon put in more and more, until there was an excess of it left in the flask. It was then kept for upwards of a week in a box, which I have been using for the purpose of these fermentations—a metallic box, which is kept, by means of a regulated gas-burner, at a temperature of about 30° centigrade, or a little above blood heat. During that time the substance has been gradually undergoing a change or fermentation. It became strongly acid, and I then added a base, at one time potash, and afterwards powdered marble or carbonate of lime, which was dissolved by the acid, and thus a quantity of lactic acid was formed. Here also there are little cells, which, under the microscope, can be seen to be different from those in the first mixture. They are smaller in their dimensions, but yet they present no very marked individual characteristics by which they can be identified. Indeed, the chief, or, I may almost say, the only thing by which we can certainly identify any one of these organisms is by setting it to work, and by seeing what work it performs. In the third carboy I have a mixture which had gone through the phase I have just been speaking of. It contained some sugar with lactic ferment, but when all the sugar had disappeared, and was transformed into lactic acid, I left the carboy in the same warm chamber, and another fermentation has set in, and there is already a considerable quantity of the substance called butyric acid present, and the greater part, if not the whole, of the lactic acid has already passed over into this butyric acid. Here, in this glass dish, there is another ferment still, although, unfortunately, it has got disturbed in coming here. It contained a decoction of yeast, with which was put about two per cent of pure vinegar, and about four per cent of alcohol, and I then touched the surface of the liquid, which was perfectly clear, with a glass rod which had been in contact with the vinegar-plant, and left some little particles floating on the surface of the liquid. These little particles, in the course of a day or two, spread over the liquid, and when this vessel came from University College this morning, it was covered wifh a perfectly uniform film, consisting of little cells, different from each of the others to which I have called your attention, and quite distinguishable under the microscope. I should state that, after the mixture was first made, and after the vinegar cells were put into it and allowed to grow on it, I supplied them with some additional food twice. On one occasion I added a somewhat larger quantity of alcohol than was intended, and the effect was that the cells were most injuriously affected. They constituted a dense, smooth, white film, and this seemed almost to disappear, and on examination under the microscope it was found that they had shrunk—in fact, they had been killed by a too strong dose of alcohol. This was then allowed to evaporate, and the vinegar cells very soon again spread over the liquid. I will now commence, in another dish, a similar experiment. I have in this bottle a mixture of yeast-water and alcohol, with a few drops of acetic acid in it. I will pour this into the glass dish, and then put on to the surface some of these little ferments, which I have here, and I have no doubt that, if we allow this mixture to stand, we shall find, by our next meeting, that it will be covered over with a smooth film, consisting of vinegar cells, which will be transforming the alcohol into acetic acid. I may show you the strength of the acid in this last instance by putting into it a slip of blue test-paper, which you see is immediately coloured a deep red.

With regard to the process by which these cells are propagated, some exceedingly interesting experiments have been made under the microscope. Professor Mitcherlich and various others, Pasteur among them, have put little alcohol cells under the microscope, putting them first into a liquid upon which they could feed, and they have noticed that the cells, or some of them, gradually swelled out at one side—that a little wart, if I may use the expression, made its appearance on one side; that this increased in size until it became as large as the original cell, and then it became detached. The propagation of the alcohol cells, the wine ferment, has been seen by several observers to take place by a process of budding. I will show you the growing cells, by throwing on the screen, by means of an oxy-hydrogen lantern, a photograph of the wine ferment, some of which will, I believe, show a little excrescence at the side, and the general arrangement of the cells will be easily detected. This is a photograph from a plate of M. Pasteur's, and conveys an exact representation of the appearance which the alcohol cells ordinarily present. I will now show you the photograph of the acetic ferments, and the difference in the general appearance is very striking. When examined carefully, it will be found that these little vinegar cells are in couples; little masses about twice as long as they are broad, and by degrees they become strangulated at the waist, and ultimately separate. With a considerable magnifying power it has been found that the wine cells contain granulated particles, but exceedingly little is yet known of their structure. Certainly one of the most promising directions for investigation in the phenomena of life is presented by the spacing study of these various little organisms, which we have so completely under our control.

With regard to the processes by which these cells are propagated, I have mentioned already that when certain liquids, capable of undergoing decomposition, are exposed to the air, that little cells gradually make their appearance in what was at first quite an unaccountable manner. It was long supposed, and on very good authority, that the oxygen of the air was the active agent in transforming a fermentible substance into these little cells; and Gay-Lussac, one of the ablest of French chemists, who died some little time, made some very careful experiments with a view to decide that point. They led him to the conclusion that oxygen was all that was needed in order to initiate the process of fermentation in the juice of grapes, which by itself does not ferment. It is worth while to state, in general terms, the nature of these experiments. He put into a glass vessel, closed by mercury, a small quantity of grape-juice, which was expressed under mercury, so that it did not come in contact with the air on its way into the glass jar intended to receive it. This was then kept closed for some time without change. He then introduced oxygen, sometimes from the atmosphere—I am now giving you an account partly of what was done by Gay-Lussac, and partly what was done by others—and sometimes the oxygen was derived from potassic chlorate. Air was used which had been passed through red-hot tubes, so that any vital organisms in it must have been destroyed before reaching the grape-juice; and it was found that, in these cases, the access of the air to the substance did induce the formation of yeast cells and did induce a process of alcoholic fermentation in the liquid by their growth. The conclusion, therefore, appeared to be established that oxygen was all that was needed for the process. Since that time, however, other experiments have been made, with precautions which were not observed by Gay-Lussac; and I must especially quote a truly masterly investigator, Pasteur, whose extraordinary researches in this subject have certainly constituted an important era in our knowledge of it. Pasteur has made a great number of experiments, partly such as those which had been made before, and partly fresh ones, of which I will describe a few characteristic samples. For instance, he took little glass bulbs, with a long neck bent in several places, like the one I hold in my hand. This little bulb contains some yeast-water, and also about 10 per cent of sugar, a mixture which is peculiarly susceptible of undergoing fermentation and decompositions of various kinds. When this was introduced into such a bulb, Pasteur boiled the liquid for some time, so that any

little living particles which might have entered the bulb with the liquid, by being exposed to the temperature of boiling water, might be killed, and also that any particles which might be lodged in the neck of the flask would be similarly treated and killed. Some of these bulbs he closed, sealing up the tubes whilst still full of steam, and he then put them by in a warm chamber, similar to that which I just now alluded to as being of the temperature of 30° centigrade, so that they should be under the conditions most favourable to the development of any little living organisms, if such could develop themselves. He so kept them for days, weeks, and months, and I am not sure that he did not keep some for years, and at the end of the whole time he found that in no case was there the production of these vital organisms. I told you that when the tube was closed the vessel was full of steam; of course that steam was condensed on cooling, and left a partial vacuum above the liquid, and when Pasteur opened the tube by breaking off the point, the air rushed in violently to fill the vacant space. He found that in almost every case, although not in all, after this air had rushed in, a process of decomposition commenced, and in some cases he found little animalcules, and various kinds of mould in others, and he has described a considerable number of different organisms which he got in different bulbs in that manner. It so happened, also, that in one case the tube, I think accidentally, at first remained unsealed, that it was not kept from contact with the air as the others were; still, to his amazement, Pasteur found that even in this one which remained open there were no organisms, that it remained as unchanged as those which were sealed up. Finding this, he repeated the experiment many times, making a great number of bulbs similar to the first, putting some of that same liquid into them, and boiling the liquid for some time, so as to destroy any organisms; but, when they had been killed, he left the bulbs open, and he found that the contents were as effectually protected by the conditions there present as if the tubes had been sealed. He submitted the results to several members of the French Academy; the experiment was repeated by other persons, and the results showed—if there were any exceptions I do not remember hearing of them—that no organisms were produced. You will notice that the liquid which had been boiled was separated by a long, thin tube from the outer air, and the air only had access to it through this long, narrow, tortuous passage, which, moreover, was at first wet inside, because of the condensed steam. Pasteur then cut off some of the tubes, so as to allow free access of air to the contents, without having to pass through this long narrow tube, and soon after that was done the process of decomposition set in, and he got various organisms formed in his mixture, which developed themselves in the way yeast, mould, and such-like organisms generally do.

I will now leave these experiments for the present, in order that I may tell you of some other discoveries, which will afford a key to them.

(To be continued).

NOTICES OF BOOKS.

Louisville Academy of Science.—The Journal of Science. Published under the auspices of the Louisville Academy of Science. Louisville, Ky. U.S.: L. Bell and Co. 1870.

UNDER the above titles, we have received two small pamphlets, the first of which is essentially the prospectus of a new scientific institution just established in the city of Louisville, Kentucky, a place of over 100,000 inhabitants. The object of this institution is to afford a pleasant resort for those of the citizens who are willing to encourage scientific investigations. The intention is to provide a course of lectures on the subjects of pure and applied chemistry, natural philosophy, astronomy, geology, botany, natural history, anatomy, physiology, hygiene, mental and moral philosophy, history, literature, and language. We learn also that a museum and library are to be formed, and the main portion of the first of the above-named pamphlets is occupied with a tabulated review of the course of lectures to be given during the session of of 1870-71. The second pamphlet is not as might be inferred from its title the first regular number of the "*Louisville Journal of Science*," but simply a small specimen number, exhibiting more the style of the work than what the work itself will be, because this will be regularly published every month, beginning with the 1st of December 1870, and will contain the lectures and papers read before the Louisville Academy of Science, with the latest important transactions of similar institutions. From the excellent mode in which this specimen number has been got up, we think the Louisville Academy of Science may, as has been the case with many similar institutions in the Great Transatlantic Republic, become an excellent means of imparting sound scientific as well as generally useful knowledge; for this is the great desideratum for the masses of the people.

CORRESPONDENCE.

EXAMINATIONS IN CHEMISTRY.

To the Editor of the Chemical News.

SIR,—As you have kindly allowed the interchange of opinions on examinations in practical chemistry I desired in my former letter to take place through the CHEMICAL NEWS, it devolves upon me to commence the interchange.

It will not be denied that practical examinations in purely experimental sciences, *if they are given at all*, play a very subordinate part in the examinations in these sciences, and this at a time when examinations are so extensively employed as tests of proficiency. Hence, compared with the written examinations, very little thought or attention has been devoted to them; consequently they are not in unison with the present state of these sciences. In chemistry very little has been done beyond giving examinations in qualitative analysis, and these examinations are frequently so trivial that they are limited to one or two bases, and the like number of acids.

I recently conducted in this college some examinations in practical chemistry. I gave to the junior students a complex mixture, to be examined qualitatively; to the more advanced students one in quantitative analysis, and one in assaying. The remarks which follow apply to the two latter examinations. In both cases the students were limited as to *time*, and there was *variety* as regards the substances to be estimated. I intended to make these examinations, as regards conditions, &c.—and I think I succeeded in making them—more severe than those a young chemist would encounter if he was engaged as an assistant to an analytical chemist, or if he was appointed chemist in some chemical manufactory. And in this point of view I have not seen any examinations equal to them; not even the practical examinations for the degree of Doctor of Science in the London University, which are the most advanced I have seen, although I think far inferior to what they ought to be for such a degree examination. It is in the extent and conditions of the examinations I think an interchange of opinions would be so useful. I send a copy of the printed papers as being the most satisfactory mode of proceeding. In the instructions it will be seen that six hours were allowed for both papers, but I may add that they were completed by the students in five hours, and most satisfactorily.

Subject — Quantitative Analysis—Practical Examination.

General Instructions.—The same value is attached to each determination. Six hours are allowed for the

analyses. The percentage amounts of the substances to be determined to be given in each case.

(1). Find the amount of chlorine in the sample of chloride of lime given you, by means of a standard solution of arsenious acid.

(2). Determine by means of the polariscope the amount of sugar in the sample of cane sugar given you.

(3). Estimate the amount of soluble phosphate in the sample of superphosphate given you.

(4). Find the "original gravity" of the *porter* given you for examination.

(5). Determine the amount of *peroxide of manganese* in the sample of manganese ore given you.

Subject—Assaying—Practical Examination.

General Instructions.—The same value is given for each assay. Six hours are allowed for the assays. The percentage amounts of the substances to be determined to be given in each case, No. 4 excepted.

(1). Determine by wet assay the quantity of iron in the sample of iron ore given you.

(2). Determine by dry assay the quantity of lead in the sample of lead ore given you.

(3). Determine by cupellation the quantity of silver in the button of lead obtained in the lead assay.

(4). Determine by Lewis Thompson's method the absolute heating power of the sample of coal given you, and state how much water at 212° F. will be converted into steam by the coal.

(5). Determine by wet assay the quantity of copper in the sample of copper ore given you.—I am, &c.,

ROBERT GALLOWAY.

Royal College of Science, Dublin,
December 27, 1870.

THE SEWAGE QUESTION.

To the Editor of the Chemical News.

SIR,—Will you kindly give me space to point out some mistakes into which Mr. Stanford has fallen with regard to my "Digest of Facts" relating to the sewage question?

It is *not* "the Report of the British Association Committee," nor is it true that it anywhere "pretends to be a synopsis of its views." It does not (*i.e.*, the first edition did not) contain the results of the work of the committee, which will be published shortly in their Report, but is a "Digest" of the most important ascertained facts, prepared at the suggestion of the committee, to save persons who wish to study the subject the trouble of wading through the numerous blue-books, &c., in which the necessary information is contained.

As to the implication that it merely contains "in abstract what has been already published *in extenso* in a Government blue-book" at a cheaper price, Mr. Stanford's own subsequent remarks, and the fact that the work has been sufficiently appreciated by the towns to necessitate the immediate preparation of a second edition, sufficiently refute this.

I am not aware that I have anywhere stated that "our sewers must be porous to the drainage water above and impervious below." What I have stated is, that "it is certain that towns must be provided with *pervious drains*, and it is equally certain that they must be provided with *impervious sewers*." ("Digest," p. 132).

I do not wish to enter into any special pleading for or against any system, as my object has been, and still is, to collect the substantiated evidence from all sides, and if the balance of evidence should at any time turn in favour of a "dry" system, though I certainly see no probability of its doing so, I shall be the first to acknowledge it, as it makes not the smallest difference to me which system is ultimately adopted, so that it be the one which offers the greatest sanitary advantages.—I am, &c.,

W. H. CORFIELD, M.A., M.B.,
Professor of Hygiene and Public Health at University College.

Jan. 3, 1871.

MISCELLANEOUS.

The Royal Polytechnic Institution—Professor Pepper and his co-Directors have, as is their usual custom at this festive season, provided a long and excellent programme for their visitors. This programme, which includes the lecture on the Continental war, in which the applications of modern chemical and mechanical science to military purposes are explained, will, we are sure, continue to attract large audiences.

Quantitative Estimation of the Non-Saponified Neutral Fatty Matter Present in Soap.—The soap, previously converted into thin shavings, is first thoroughly dried at 100°, and next boiled for a considerable time with petroleum naphtha, so-called benzoline of commerce in this country; but this fluid should be, previous to use, submitted to fractional distillation, and only the liquid which comes over at 86° applied for the purpose just alluded to. The ebullition of the weighed-off sample of soap has to be so arranged that the evaporating liquid runs back, after condensation, into the retort or flask; the use of the petroleum naphtha for this purpose is based on the fact that the fatty matter of soap saponified and combined with alkali is completely, or, at least, for all practical purposes, completely insoluble in this naphtha as instances the following examples:—11·3 grms. of Marseilles soap (previously dried at 100°) were boiled for several hours with benzol (pure) after the evaporation of the previously filtered liquid, there remained a quantity of 0·145 grm. = 1·2 per cent, the ash of which amounted to 0·002 grm. equal to 0·015 grm, or 0·13 per cent of soap, the rest being non-saponified fat; 8·197 grms. of soap treated with petroleum naphtha yielded 0·15 per cent, containing no ash at all. The use of ether, even if anhydrous, and of rectified sulphide of carbon are not suited for this purpose, because the alkaline stearates are, to some appreciable extent, soluble in these liquids.

The Glasgow Gas.—The following statistics regarding the illuminating power and purity of the Glasgow gas for the quarter ending the 31st of December, 1870, have been supplied to us by Dr. Wallace, F.R.S.E., Gas Examiner for the City.

	Townhead works.	Dalmarnock works.
Illuminating power of 5 cub. ft. per hour in standard candles, average	28·74	28·23
do. maximum	32·28	31·04
do. minimum	26·40	25·51
Sulphur, grains in 100 cubic feet of gas, average	12·68	12·25
do. maximum	18·32	17·88
do. minimum	6·28	5·23
Ammonia, grains in 100 cubic feet of gas, average	5·42	4·88
do. maximum	8·93	8·94
do. minimum	3·17	1·56

The two kinds of gas are tested daily (about 6 p.m.) at the same station, which is about a quarter of a mile from the Townhead works, and a mile and a half from the Dalmarnock works. The same mixture of coals is used in both works, and hydrated oxide of iron as well as lime is extensively employed in the purification of the gas. Not the slightest trace of sulphuretted hydrogen was observed during the quarter, except on one occasion, when a quantity of unpurified gas escaped accidentally into one of the holders, rendering the gas impure for several days. The gas-works of Glasgow belong to the City, having been acquired from the companies by whom they were erected about a year and a half ago. The illuminating power stipulated in the Glasgow Gas Act is 25 candles, but there is no limit defined as regards sulphur and ammonia. The price is 4s. 2d. per 1000 cubic feet.

THE CHEMICAL NEWS.

Vol. XXIII. No. 581.

ON FLUORIDE OF SILVER.*

By GEORGE GORE, F.R.S.

This paper contains an exhaustive account of the behaviour of argentic fluoride in vessels of platinum, carbon, and various fluorides in contact with chlorine, bromine, and iodine at various temperatures. When argentic fluoride is completely decomposed by chlorine in platinum vessels at a red heat, the reaction agrees with the following equation :—

$$4AgF + 4Cl + Pt = 4AgCl, PtF_4.$$

Vessels of cryolite and of fluor-spar were found incapable of retaining argentic fluoride in a melted state. Other vessels were also made by melting and casting various mixtures of earthy fluorides at a high temperature; and although forming beautiful products, probably capable of technical uses, they were not capable of retaining silver fluoride in a state of fusion. Numerous vessels were also made of seventeen different fluorides by moulding them in the state of clay and baking them at suitable temperatures; these also were found incapable of holding melted fluoride of silver. Argentic fluoride was only superficially decomposed by chlorine at 60° F. during thirty-eight days. When heated to 230° F. during fifteen days in a platinum vessel in chlorine, it was very little decomposed. Chloride of silver heated to fusion in a platinum vessel in chlorine corroded the vessel and formed a platinum-salt, as when fluoride of silver was employed.

An aqueous solution of argentic fluoride agitated with chlorine evolved heat and set free oxygen, in accordance with the following equation :—

$$8AgF + 8Cl + H_2O = 5AgCl + 3AgClO + 8HF + O,$$

or—

$$7AgCl + AgClO_3 + 8HF + O.$$

Dry hydrochloric acid gas completely decomposed argentic fluoride in a melted state, but only acted upon it superficially at 60° F. A saturated aqueous solution of argentine fluoride was not precipitated by chloric acid.

Perfectly anhydrous fluoride of silver was only superficially decomposed by contact with bromine in a platinum vessel during thirty-six days at 60° F., or during two days at 200° F. At a low red heat, in vessels of platinum, argentic fluoride was completely decomposed by a current of bromine vapour, a portion of its fluorine being expelled and a portion corroding the platinum and forming an insoluble compound of fluoride of platinum and bromide of silver. In carbon boats at the same temperature the whole of the silver-salt was converted into bromide, the boat being corroded and the fluoride escaping in chemical union with the carbon. The action of bromine on an aqueous solution of argentic fluoride was similar to the action of chlorine. A solution of argentine fluoride yielded copious precipitates both with hydrobromic and bromic acids.

Under the influence of a temperature of 200° to 600° F. in closed platinum vessels, iodine very slowly and incompletely decomposes argentic fluoride without corroding the vessels, and produces a feeble compound of argentic iodide, fluorine, and iodine, from which the two latter substances are expelled at a red heat. At a red heat in platinum vessels, iodine produces argentic iodide, and in the presence of free argentic fluoride corrodes the vessels in consequence of formation of platinic fluoride; iodine and fluorine pass away together during the reaction. In vessels of carbon at the same temperature argentic iodide is formed, the vessels are corroded, and a gaseous compound of fluorine and carbon is produced. By treating an aqueous solution of argentic fluoride with iodine, similar results are produced as with bromine and chlorine; a similar solution yields copious precipitates both with hydriodic and iodic acids.

A mode of analysis of iodine is also fully described in the paper. A known weight of iodine was dissolved in absolute alcohol, a strong solution of argentic nitrate of known strength added to it in portions at a time, with stirring until the colour of iodine exactly disappeared. The mixture was evaporated, the free nitric acid expelled by careful heat, and the residue weighed. The residue was then heated to fusion, to convert the iodate of silver into iodide, and again weighed. Two experiments of this kind yielded accurate results, and the process was easy and expeditious.

* Abstract of a paper read before the Royal Society.

ON SOME SOURCES OF ERROR IN VOLUMETRIC ANALYSIS.*

By ROBERT R. TATLOCK, F.R.S.E., F.C.S.

The object of this note is to point out a few of the many sources of error in volumetric analysis, and to indicate the best means of avoiding them. The accurate and observant experimenter will find little that is new in what I have to bring forward, but at the same time I intend to refer mainly to such circumstances as are but too frequently overlooked by the ordinary analyst.

In the first place, I would seek to direct attention to a source of error which, so far as I can ascertain, has hitherto been overlooked by chemists. It arises from the fact that saline solutions contract, as a rule, when mixed with water, the extent of the decrease in volume depending on the nature of the saline substance dissolved, as well as on the strength of the solution. This fact, although it does not seem to have been recognised in principle till within a comparatively recent period, appears to have been discerned in practice for a very long time back, as otherwise there would have been no necessity for the elaborate tables showing the proportions of salts, of acids, and of alkalies in solutions of different densities, with which many chemical works, even of old date, are amply provided. Much misapprehension prevails, even at the present time, regarding the nature of solution, but more particularly in connection with its effect on volume. In the Chemical News of July 15th, 1870, we find a paper by S. Beswick, of Boston, U.S., calling attention to the circumstance that Swedenborg was the first to announce that when saline substances were dissolved in water there was *no increase in bulk*, the volume of the solution being exactly the same as that of the water employed, a doctrine which was afterwards promulgated by Dalton. The author of that paper seems to take the doctrine for granted, and claims for Swedenborg the honour or credit of the discovery. A very superficial glance, however, will show that the hypothesis is altogether a mistake, for, if it were true, the density or sp. gr. of a saline solution would at once indicate the proportion of solid matter it contained, which we know is very far from being the case, On the contrary, it has been shown by Persoz that the volume of a saturated saline solution is just the volume of the water plus the volume of the salt. It is also certain that, when the saturated solution is mixed with water, a reduction in volume takes place. The sum of the volumes of the saline fluid and the water being appreciably more than that of the product after diffusion has occurred. This fact may be made apparent in various ways. For

* Abstract of a paper read to the Chemical Section of the Glasgow Philosophical Society, on Monday evening, Dec. 19.

example, if we mix together equal volumes of a salt solution of any given gravity and of water, and test the gravity of the resulting mixture, we find it higher than that of the mean, which of itself demonstrates that contraction has taken place, and we have thus a most refined method of determining the *amount* of condensation by accurately taking the gravities of the solutions at different strengths. The actual shrinking may be seen by half filling a large bulb of known capacity, provided with a long, narrow graduated stem, with saturated solution of common salt, and carefully filling completely to one of the marks on the narrow neck with water, taking care that the liquids do not diffuse. The level of the water having been accurately observed, by swaying the flask to and fro for some time, in order to mix the two liquids, the fluid is seen to run down the stem to such an extent that, with a bulb capable of holding one litre, and having a stem of one centimetre internal diameter, a difference of level of from ten to fifteen centimetres may be observed. Nor is this diminution in volume due to the slight reduction in temperature which is usually observed to accompany the diffusion, as that is generally too small to account for any appreciable alteration in volume.

I have made very many trials of various saline solutions in this direct way, but the following example, that of sulphate of ammonia, which is selected on account of its giving unusually marked results, will serve to give some idea of the extent of these contractions :—

Vol. of sulph. amm. sol. used.	Vol. of water used.	Sp. gr. of sulph. amm. sol.	Solid matter, per cent, by weight.	Solid matter, per cent, by vol.	Contraction, per cent, of total vol.
1	1	1·2500	43·5	54·3	1·165
1	1	1·1394	24·5	27·9	0·361
1	1	1·0756	13·5	14·5	0·115
3	1	1·2500	43·5	54·3	0·787

The diminution in bulk which takes place when various fluids are diffused in water is well known.

The practical distiller, in making down a high strength of spirit to a lower, expects to find a smaller volume of product than that of the sum of the two fluids used. It is also quite understood that strong sulphuric acid when mixed with water gives contraction, but I was not prepared for the result of some experiments, which showed an immense contraction even while the fluid remained at 100° C.

Only fifteen years ago Michel and Krafft denied that any diminution in volume takes place when saline solutions are mixed with water. How they could come to that conclusion in the face of the fact that a mixture of given bulks of saline solution and of water is not a fluid whose density is the mean of the two, it is difficult to conceive. Later experiments by Kremers, Schiff, and Persoz, show that contraction takes place as a rule, and Schiff mentions the curious circumstance of non-contraction in the case of ammonium chloride. More minute experiments are required, however, before the latter exception can be considered established.

The circumstances attendant upon these effects are very complicated, and in the present state of science the results cannot altogether be explained. In the case of saline substances the shrinking is usually accompanied by a slight reduction in temperature, whereas we naturally expect an elevation, as in the examples of alcohol and of sulphuric acid. According to observations by John Tatlock, sugar solutions and glycerine solutions contract and give heat on diffusion with water.

What the analytical chemist has chiefly to do with, however, is the effect of these contractions on the accuracy of his results when that has to depend to any extent upon the solution of a salt and the accurate division of the solution. In the ordinary analysis of a mixture of saline substances, such as a commercial potash salt, the method usually, and very properly, followed, is to dissolve the substance in water and filter into a measuring flask. The filter having been washed till the flask is half full or more, the solution is made up, accurately, to the mark with water, and is then thoroughly mixed. It is obvious that contraction must here take place, and that if we do not take this into account, we shall, when we proceed to divide the solution by pipettes, actually draw off a somewhat stronger solution than if no contraction had occurred. It is hardly necessary to observe that this error may to a great extent be obviated (1) by using as weak solutions as possible, as it is manifest from the example given that strong solutions contract far more for a given weight of solid substance than weak ones, and (2) by mixing the saline solution thoroughly with the water before making quite up to the required bulk.

Another source of error, and one I believe not often taken into account, arises from the fact that graduated vessels made to *deliver* fixed volumes of water do not deliver exactly corresponding volumes of saline fluids. Hence we do not obtain the exact amount of solid substance which we expect if we base upon the water-delivering capacity of the pipette or other vessel.

The following tabulated statement of the results of a few trials with solutions of common substances, will show that the adhesion of a strong solution to the glass surface is very great, and quite sufficient to disturb the accuracy of results in most ordinary cases.

The vessel used was a pipette, which delivered 1000 grs. of water at 60° F., by draining for ten seconds, and then touching the delivered water twice with the extreme point :—

Solution used.	Sp. gr.	Solid matter, per cent, by weight.	Pipette used. Vol. grs.	Weight of solution delivered.
Sulphate of ammonia	1·2500	43·5	1000	1247·4
Chloride of potassium	1·1812	24·5	1000	1179·3
Chloride of sodium ..	1·2072	26·4	1000	1204·4
Sulphate of zinc ..	1·4264	53·0	1000	1419·6
Chloride of ammonium	1·0724	26·0	1000	1071·9

It is obvious from these results that a pipette, delivering 1000 grs. of water, does not, as we should expect, deliver a quantity of fluid corresponding with the sp. gr., but sensibly less, and hence a pipette must be graduated to deliver, not water, but the particular saline solution to be employed; that is, the volume of the fluid delivered must be such that the weight will be identical with the gravity. As in the error caused by diffusion—contraction—the difference is here much greater in strong solutions, for a given weight of solid salt, than in weak ones, but a reference to the above example of chloride of ammonium will show that the difference depends upon the sp. gr. of the solution rather than on the amount of solid matter which it contains. Weak solutions are therefore to be preferred in all cases.

Another source of error, but one more generally recognised, arises from the fact that saline solutions expand more than water for equal increments of heat, and, as a necessary consequence, contract more than water by reduction of temperature. The expansion increases very rapidly with the strength of the solution, so that we should either work as near to the standard temperature as possible, or, where that is not admissible, with weak liquors.

The author next referred to the defects of graduated vessels as usually sold, and exhibited and described an apparatus devised by Mr. James Chalmers, of Iquique, Peru, by which a tube, although of unequal calibre throughout, could be graduated, so that each division delivered exactly equal volumes of the same fluid.

NOTES ON THE PROGRESS OF FOREIGN ANALYSIS.*

By Dr. LUNGE.

MOHR (vol. viii., p. 13) proposes to protect solutions of bichloride of tin, sulphuretted hydrogen, caustic baryta, sulphurous acid, and the like, against the action of atmospheric oxygen and carbonic acid, by covering them with a layer of petroleum.

Reichardt (p. 116) recovers uranium oxide from the residues accumulating from estimations of phosphoric acid by dissolving the uranium phosphate in hydrochloric or nitric acid, adding an excess of ferric chloride and sodium acetate, diluting and boiling for some time. The phosphoric acid is completely precipitated along with basic acetate of iron, and all uranium remains in solution. A cheaper method is to precipitate the acid solution of uranium phosphate after adding an excess of ferric chloride with sodium carbonate. The excess of the precipitant retains the uranium oxide completely in solution; the filtered solution is acidified with hydrochloric acid, heated till all carbonic acid is driven off, and the uranium oxide precipitated by ammonia.

Kubel (p. 125) draws attention to the fact that the precipitate caused by ammoniacal solution of magnesia in the solution of a phosphate contains more magnesia than corresponds to the calculated formula, and that this precipitate must therefore be re-dissolved and re-precipitated to obtain accurate results.

Cossa (p. 145) gives the results of careful experiments upon the solubility of calcium carbonate, dolomite, &c., in water saturated with carbonic acid.

P. 174, Bunsen's paper on his filter pump.

Gerlach (p. 245 to 298) gives a number of exceedingly valuable tables of solubility, embracing, perhaps, all published determinations of this character in a synoptic form. This part can be purchased in a special reprint.

Trommsdorff (pp. 330 to 370) gives a complete course of instructions for water analysis, which also deserves to be published separately. There are copious additions to it in vol. ix., pp. 157 to 176.

Schönn (p. 380) describes a method of decomposing many refractory substances by means of sodium or potassium in a steel crucible.

Emmerling (p. 434) describes the action of boiling liquids, water, acids, alkalies, &c., on glass and porcelain vessels. As Fresenius has done before, he finds that hydrochloric and nitric acid act on glass less than boiling water, but the addition of sulphuric acid, and especially of caustic or carbonated alkalies, acts in the opposite way. He, therefore, strongly advises to avoid evaporating solutions ever so slightly alkaline; not to use quite new vessels, which are far more acted upon than when they have been sometime in use; and to diminish the bulk of the washings, which can be easily done by the use of Bunsen's filter pump. Especially he recommends the use of porcelain vessels in preference to glass, wherever practicable, as water and acids have next to no action upon them, and alkalies much less than upon glass.

Böttger (p. 449) recommends test-paper stained with an alcoholic solution of alkanine. The smallest trace of alkalies, especially of ammonia, changes its deep red colour into blue.

Stein (p. 450) describes the way to prepare ultramarine paper for the purpose of testing for free acid in commercial sulphate of alumina, and other salts, which even in the completely neutral state act upon the ordinary test-papers like free acids, but not upon ultramarine, which is only discoloured by really free acid.

Poleck (p. 451) asserts that the formation of carbonic oxide, which has been observed during the action of potassium pyrogallate on oxygen, or mixtures containing it, and which has been believed to render this analytical method useless, takes place only when the proportion of oxygen in the mixture is greater than in atmospheric air. In the latter, or in mixtures containing less oxygen, no carbonic oxide at all is formed by this method.

Löwe (vol. ix., p. 20) proposes to substitute for the usual test solution of alkaline copper tartrate, another alkaline solution of copper, produced by the help of glycerine. It can be made more quickly, does not incline to the formation of fungi, and is much more stable, even in diffused daylight. To a solution of 16 grammes pure crystallised copper sulphate in 64 grammes water, there is added in the cold 80 cc. caustic soda solution of 1·34 specific gravity, and the mixture is shaken up with 6 to 8 grammes pure glycerine till all is dissolved. If this solution shows the least turbidity on diluting with two-thirds of its bulk of water, or on heating, a little more caustic soda and glycerine must be added. If it is desirable to have a solution free from sulphates, freshly precipitated copper hydrate is dissolved in soda solution, with the help of glycerine.

Reinige (p. 39) describes a very interesting method for the estimation of iodine, viz., by means of potassium permanganate. Two equivalents of the latter, and one equivalent of potassium iodide, produce one of potassium iodate, two of free potassa, and four of manganese peroxide. The decomposition is assisted by boiling the solution, and if the latter is very dilute, a little potassium carbonate is added to induce the commencement of the reaction. As bromine and chlorine are not in the least acted upon under the same circumstances, this reaction is, perhaps, the most convenient for the estimation of iodine. The latter must be combined with potassium, and the solution rendered nearly neutral but still slightly alkaline. It is heated till it boils gently, and a solution of 2·5 grammes potassium permanganate in 497·5 grammes water is gradually added, removing the beaker from the lamp each time for a few moments, to allow the precipitated peroxide to deposit, but heating it again to the boiling-point before adding another portion of permanganate. When the solution, after the precipitate has subsided, shows a distinctly reddish tint, all potassium iodide is decomposed. The small excess of permanganate is estimated by sodium hyposulphite, and the remainder shows exactly two milligrammes of iodine for each cc. of the permanganate solution used. The results are very accurate.

Fresenius (p. 52) describes a number of very important experiments concerning the influence of various acids and salts on the precipitation of barium sulphate, and the admixtures of foreign salts with the precipitate. His conclusions are:—(1). Barium sulphate does not require 43,000 parts of water for solution as usually stated, but upwards of 400,000 parts, even in presence of hydrochloric acid, which slightly raises its solubility. (2). Sodium chloride, potassium chloride, and barium nitrate do not increase its solubility very perceptibly, but the alkaline nitrates do so. (3). Hydrochloric acid does the same, and it must, therefore, be evaporated or partially neutralised before the precipitation. (4). Sodium chloride does not produce any foreign admixture in the precipitate. (5). Potassium chlorate does cause such admixture in a high degree. The precipitate can, however, be purified by digesting with hot hydrochloric acid, evaporating the same, and washing with water. (6). Sodium, potassium, and barium nitrate cause a very impure precipitate, which cannot be purified by washing with hydrochloric acid, but only by fusion with sodium carbonate, and estimating the sulphates in the aqueous solution of the fused mass.

Schorer (p. 213) describes and figures an extremely simple thermostat. The principle is, that the air in an empty test-tube inside the air-bath acts by its expansion or contraction on the mercury in one leg of a U-tube, into the other leg of which the gas delivery tube dips. The latter has a long narrow slit, which is lengthened or

* From *Fresenius's Zeitschrift*. Read before the Newcastle-upon-Tyne Chemical Society.

shortened as the mercury falls or rises, and thereby increases or decreases the quantity of gas delivered. A set screw on the top allows of adjusting the dip for any temperature required.

Lowe (p. 216) gives some excellent hints on the ultimate analysis of organic bodies.

Bunsen (p. 283) describes his method of analysing the ashes left by the combustion of vegetable or animal bodies. It is prccisely the same as he has taught for many years past to those working in his laboratory.

In the discussion which ensued,

Mr. R. C. Clapham said, with reference to Emmerling's paper, that the glass floats and windows in vitriol chambers were very often much corroded, and that this effect, which he attributed to hydrofluoric acid, varied very much with different kinds of glass.

The President (Mr. John Glover) had seen fluor spar in cargoes of pyrites.

Mr. W. H. Richardson, with regard to Stein's test-paper, said he had never found commercial sulphate of alumina which did not more or less affect ultramarine.

Dr. Wright suggested, as a delicate test for free acid, a mixture obtained by precipitating Prussian blue in the ordinary way, adding slowly weak caustic potash till the colour is just discharged, and diluting.

The President, with regard to Fresenius's paper, drew attention to the great discrepancy between results obtained here and in London from the same samples of pyrites, and promised a communication on this point.

ON THE STEARIC ACID INDUSTRY.*

By J. LAWRENCE SMITH.

Manufacture of Candles.

The name stearine does not represent the true character of the substance (it being stearic acid), but as it is one accepted by the industrial world, it is proper to retain it. This substance, the result of certain chemical processes, is now manufactured in all parts of the world.

The quality of the product does not vary materially. There seems to be rather a greater range in the qualities manufactured in some countries than in those manufactured in others. In France the demand for quality appears more uniform than in England, for in England a large number of softer candles are in general use, while the universal demand in France is for a hard, white, smooth candle. The principal difference noticed in candle stuffs at the Exposition is that some are a little more mottled than others. This is attributable in part to the difference of the methods of saponification—that made by the sulphuric acid saponification being more mottled than when treated by lime; but my observation convinces me that this defect is principally due to the method and care in cooling the melted stearine before moulding. In the character of the stearine products, there is no advance on the Exposition in London in 1862, this being due to the fact that there was hardly anything left to be desired under this head.

If we are enabled to make such beautiful candles out of almost any description of fatty matter, it is entirely due to the application of some of the most interesting chemical results on record; and it is with just pride that France can claim the development of the whole subject from its incipiency to the present perfection of the industrial processes. These labours commenced with M. Braconnot, of Nancy, in separating oleine and stearine, and were extended through the remarkable scientific researches of M. Chevreul. That these labours deserve to be ranked so high, arises from the fact that they were made at the birth of organic chemistry and on a most difficult class of bodies about the nature of which there was no correct conception and when the analyses of organic substances had been but just commenced. Still more, M. Chevreul did not give to the world the results of his labours as a mass of isolated facts, but he systematised and classified new acids, new bases, and left to us the chemical history of facts almost as fully made out as they are at the present time; and these have contributed as much, if not more, than any class of researches to give direction and growth to organic chemistry. The decomposition of the fats and formation of the fatty acids developed the fact that, when melted and allowed to cool slowly, the more solid acids crystallised out of the mixed mass in such manner as to allow of easy separation of the solid from the liquid part, which fact soon suggested a practical application. Without, however, going into details of all the known processes, so fully and clearly described by M. Stas in the report of the Exposition of 1855, a tolerably comprehensive review of the subject will be given.

In 1823 a complete account of the labours of Chevreul was published; at this period fruitless efforts were made to manufacture stearine, but without success. It was in 1831 that Adolph de Milly overcame the practical difficulties of manufacturing stearine on a large scale, and established this branch of industry on certain principles, some of which have remained unaltered, while others have been materially changed in later years by the same chemist, as will be seen a little further on.

The first countries in which stearine candles were manufactured were France, England, Belgium, Russia, Austria, and Sweden. This manufacture gradually found its way into other parts of the world, and it is now carried on in the remotest places, as Calcutta and Sydney (Australia).

Importance of the Stearic Acid Industry.

It is difficult to render an exact account of the importance of the stearic acid industry; nevertheless it can be stated with certainty that the annual production for France is 25,000,000 of kilos., and approximately for the remainder of Europe is 100,000,000 of kilos., and for America 10,000,000 of kilos.

The largest candle factory in the world is that known as the Price Company, having its principal establishments at Liverpool and London. Its principal products are what are called the composition candles, of a good quality, but a little greasy to the touch. These candles are used in England by all classes, but would not be received in other countries, where the requirements of luxury are more exacting. The establishments in Holland and Belgium are relatively of great importance, arising from the fact that apparatus, coal, and labour are cheap; the consequence being that these candles are manufactured cheaply and sold at corresponding prices, and are enabled to be exported more than they are used. As previously stated, from 1831 to 1846, saponification in open vessels by lime was altogether employed; in 1846 another process was introduced, viz., saponification by sulphuric acid followed by distillation. These two processes had their advantages and disadvantages. The saponification by lime gave a small relative yield of candle stuff, but the candles were of the finest quality, having a high melting-point; the yield of oleic acid was greater and of a superior quality. On the contrary, the sulphuric acid followed by distillation yielded a larger amount of candle stuff, but of second quality, and a smaller yield of liquid matter of inferior quality. The industrial world employs one or other of these other methods, according to the requirements and circumstances governing the demand and the nature of raw material employed. In some countries smooth, white, dry candles, of a light melting-point and of the first quality, are required, while in other countries, attracted by the low price, a candle is used that is yellowish, soft, and greasy to the touch, with a low fusing-point. For this reason, France, Germany, Russia, Italy, Spain, and Portugal,

* From "The Progress and Condition of Several Departments of Industrial Chemistry," by J. Lawrence Smith.

still employ the lime saponification, and Belgium, Holland, England, and America adopt the sulphuric acid to a great extent. As regards the profits of the two methods they are about balanced, because one class of manufacturers sell at a higher price for products of a better quality but smaller yield, while the other class sell an article at a lower price but of an inferior quality. But it is an observed fact that those who manufacture by sulphuric acid and distillation do not meet with the same commercial success as those manufacturing by lime.

There were forty or fifty factories that exposed products arising from the treatment of fats. Of these about thirty were French. Nineteen of these employed the old lime saponifiation with fourteen per cent of lime; three by the closed autoclave under pressure, with a diminished quantity of lime; one by the sulphuric acid saponification; two by water decomposition under pressure; and five by different methods of distillation. The more modern method of saponifying under pressure is gradually coming into use, and Messrs. Perré and Son, of Elbeuf, exhibited beautiful products made by that process with the apparatus invented by Renner.

Account of the Stearine Industry.

The first persons who thought of applying the fatty acids for the purposes of illumination were Gay-Lussac and Chevreul. For this purpose they took out a patent in January, 1825, which patent specified "that they reserved to themselves the exclusive right of preparing fatty acids for illumination, both liquid and solid, such as are obtained by saponification of the fats with *potash, soda, and the other bases, by the acids or by any other means.*

"We saponify the fats either at the ordinary boiling point with the pressure of a single atmosphere, or at a more elevated temperature with the pressure of several atmospheres. We have recognised that the saponification conducted in this way has great advantages over that conducted in the ordinary way under a single atmospheric pressure, 'the saponification being accomplished with the smallest possible quantity of alkali. We separate the stearic and margaric acid from oleic acid by the following processes.'"

This is done by several processes detailed in the patent, but the one essentially practical is by decomposing the soap with acids, and, by pressure, separating the solid from the liquid acids. It will be seen that this process is deduced directly from the theoretical researches of Chevreul, with the important exception in relation to saponifying under high pressure.

The process of Chevreul and Gay Lussac was not considered at the time capable of being brought into practice in the arts, from their using potash and soda, thus making the product a very expensive one.

Besides the above difficulty in the original development of stearic industry, another arose in the very commencement, viz., that when candles were made with the ordinary wick they burnt very imperfectly, and the inventors above referred to devised wicks of peculiar description that answered the purpose more or less perfectly. But prior to them, J. L. Cambacérés, devised similar ones, and subsequently improved upon them, and finally settled upon the plaited wick now in common use in all stearic acid factories.

The next important step in rendering the stearic acid industry a success was also made by M. Cambacérés, viz., the separation of the oleic acid by powerful pressure, first on the mixed acids cold, and subsequently warmed; and he established a factory in Paris to carry out his process, but this soon failed, from the inferior nature of candle-stock produced and the expense of its production, potash being employed by him as the agent for decomposing the fats.

For several years this industry was abandoned as being a difficult and unprofitable one, when, in 1829, two young physicians, De Milly and Motard, took the subject up, and, after two years of laborious and persistent study of it, accomplished the problem of the successful manufacture of candles from the fatty acids. It is only simple justice to say that the names of Chevreul and De Milly go side by side in this industry, the first in his theoretical discoveries, and the latter in his ingenious and successful devices in the accomplishment of great practical results. Nor is it too much to rank De Milly nearly as high as Leblanc for endowing chemical industry with an art so thoroughly perfected by him and leading to such important economical results in the every-day wants of the civilised world. France may well be proud of having given to the world the art of making soda by Leblanc, and that of making stearic acid candles by De Milly.

The success in making these candles depended first on the discovery of an economical means of saponification. This means consisted in the employment of lime to replace potash or soda, which method had been indicated in the original patent of Gay-Lussac, but no one had solved the problem successfully until it was done by MM. De Milly and Motard, in 1831, in a factory established by them near the *Barriere de l'Etoile* at Paris, and it is from the locality of this first factory that the almost universal name of "star candles" has been derived.

In the original method of saponifying by lime, De Milly and Motard conducted the operation in a closed vessel, under pressure, but from the difficulty of proper manipulation during the operation, it was abandoned, and afterwards carried on in an open vessel at the temperature of 100° C. We do not intend to go into any detail as regards the process already so well-known and described in works on technical chemistry, but simply to give a rapid sketch of its history and development.

The De Milly and Motard Process of Saponification.

After the lime soap is formed, it is decomposed by sulphuric acid, and the resulting fatty acids are cooled in shallow tin or copper pans, in rooms of about 60° F., so as to allow of the proper crystallisation of the solid acids. The next step is to press these acid cakes, which is first done in a cold press, then in a hot press, first suggested by Cambacérés, but successfully carried out by the horizontal presses invented by De Milly and Motard, and now universally employed in candle factories.

The difficulties, however, of these indefatigable manufacturers were far from being overcome; the pans coloured the fats with iron, the last trace of lime was not removed, and the candle made had a crystalline structure. By the alternate application of steam and dilute sulphuric acid, and white of egg and oxalic acid, they overcame the first difficulty. Various means were tried to prevent the crystalline structure, commencing with the objectionable use of arsenious acid, then the more expensive one of wax, which also had a tendency to colour the candle; they at last triumphed over this difficulty by cooling the acids to a temperature near their point of solidification before introducing them into the moulds that were first raised to the temperature of the cooling fats. During the cooling of the mass it is constantly agitated, and a pasty liquid is produced, which congeals in the moulds without crystallisation. Three years had now elapsed since the first establishment of their factory, the commercial results of which had been a failure. M. De Milly now came into the possession of the factory alone, his faith in its success remaining firm, and two years later, in June, 1836, he gave the perfecting touch to the successful manufacture of star candles. The wick has always been a source of great annoyance in the burning of the candles, for the small amount of mineral matter in the candle stuff would accumulate in the wick, and interfere with the free flowing of the melted fat. After numerous trials by saturating the wick with different substances, the required result was successfully accomplished by impregnating the wick with a certain quantity of boracic acid, dissolved in water containing one-thousandth of its weight of sulphuric acid. The boracic acid, as the combustion of the

candle progressed, united with the lime and the ashes of the wick, forming a very fusible salt, which accumulated on the end of the wick, forming a small drop.

With this last application in 1836 is to be dated the complete success of the stearic acid industry. The civilised world recognised it as such, and factories sprang into existence in various parts of Europe and America. Since then there have been some minor perfections in the above process, known as the lime saponification process. There have been, however, other processes put in practice for bringing about the same results that will be passed in review, viz :—

(1). Saponification by sulphuric acid and subsequent distillation.

(2). Saponification by water combined with distillation of glycerine and fatty acids.

(3). Saponification by water under pressure.

(4). Saponification by water under pressure with a very minute quantity of lime.

(5). Saponification by sulphuric acid without subsequent distillation.

ON THE EXAMINATION OF THE BARK OF COPROSMA GRANDIFOLIA, FOR ALKALOIDS.

By W. SKEY,
Analyst to the Geological Survey of New ealand.

The sample I tested was named by Mr. Buchanan, at the time of collecting; it has a bright yellow colour on its inner surface, is very bitter, with a slightly hot pungent flavour. It is decidedly the bitterest of any of the barks of this family which were pointed out to me, and for this reason I made choice of it for experiment.

The following is a brief summary of the results obtained :—It shows by an easy, simple, and I think a reliable process, that alkaloids, generally, and those of the Quina group in particular, are either entirely absent, or present only in so minute a quantity that the bark is quite worthless as a drug, on this account at least.

A decoction of 200 grammes of the pulverised bark, in weak hydrochloric acid, was slowly evaporated to a bulk of half-an-ounce, then filtered; the filtrate did not give any precipitate with the following reagents:—

Sulphocyanide of mercury.
Sulphocyanide of zinc.
Tannic acid.

These substances are capital tests for the alkaloids generally, giving dense precipitates in a very weak decoction, even of the common Gray bark.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from p. 11.)

One of the most important observations was made, at an early stage of the investigations, on the subject of ferments, by Dr. Schwann. He passed air through a red-hot tube, and he asserted, as the result of his operations, that air which had been so heated was incapable of producing the effects, which I mentioned just now as having been noticed by other observers, as produced by common air; that, whereas ordinary air starts fermentation, air which has been passed through a red-hot tube does not. That was what he said, and in some of his observations he was quite correct, but in some others he must have been misled. Shortly after his observations, another German philosopher thought of using cotton-wool as a strainer. He passed air through a glass tube fitted up somewhat in the same manner as the one I have here, with a tolerably compact plug of cotton-wool, which allowed the air to pass through it, but at the same time acted as a strainer, and collected a quantity of dirt at the side where the air entered it; and he found that the air which had been thus strained was no longer capable of producing the phenomena of decomposition, which air in the unstrained or unheated state does. Since, then, Pasteur has done the same thing in a more accurate and more decisive manner; and he has repeated the experiment with heated air, with precautions which leave nothing to be desired. One novelty in Pasteur's process is the use of a kind of cotton which is soluble—cotton which has been in contact with strong nitric acid, which is called gun-cotton. It retains the structure and appearance of ordinary cotton, but has this peculiarity, that it dissolves easily in a mixture of alcohol and ether. He put into a tube a plug of this gun-cotton, and then, by means of an aspirator, he drew air through this strainer for a long time, until he had collected quite a quantity of dust. He then took this gun-cotton with the dust upon it, and put it into a tube, where he poured alcohol and ether upon it, until the cotton was dissolved, and nothing left but the dust. In that manner he got the lightest portions of the dust and the heavier portions by themselves, as they very soon subsided in the liquid in which the gun-cotton had been dissolved. He then poured fresh alcohol upon them, so as to thoroughly cleanse them, and then put them under the microscope, in order to examine the particles of which they consisted. He found in this dust a great many particles of sand, calcic carbonate, and other mineral particles, as would naturally be expected, and also a great quantity of organic matter—little particles of cotton, wool, wood, and so on, and mixed with these he found some little spherical or oblong particles of very different sizes, and of some considerable varieties of shape. Some of these little round particles he found consisted of mere starch, and many of you are, no doubt, aware that starch consists of little spheroidal masses of different sizes. These he got rid of by a solvent, and others were then left, which resembled in their appearance so closely the germs of various fungi and organisms of those kinds and eggs of animalcules, that they were, to outward appearance, undistinguishable from them. He then took a liquid which had been boiled, but which was capable of decomposing—such a one as I mentioned here—by the action of any of these substances, and he put it into a flask, with precautions which I will not detain you by mentioning now more than to say that he slid into this liquid, which had not got anything present to induce the formation of organisms, some of the gun-cotton with the dust yet in it. It was the same thing as I mentioned before, only that the substance had got some of this dust from the air added to it; and he found that he also got the formation of organisms very readily and abundantly. He found that these little particles, which were to the eye undistinguishable from germs and spore, behave towards liquids of this kind just as if they were so. In various other ways the same form of experiment has been repeated, and uniformly with the same result, viz., that when little particles collected from the air, particles of extreme tenuity, are put into a liquid susceptible of undergoing decomposition, a great variety of organisms will make their appearance, just as if their seed had been sown in the liquid. This circumstance is one which, I think, will justify us in going back to what I told you of Pasteur's previous observations. I told you that he opened a number of the little bulbs which had contained yeast-water and sugar, so as to allow the air to rush into them. He found that, in some cases, he got one kind of organism produced, and in others another; in fact, he got a great variety. But if, instead of allowing the air to go into these bulbs in this way, he poured the liquid out into an open vessel, he always got the same sort of organisms;

* The Cantor Lectures. Delivered before the Society of Arts.

there was no variety. The appearance of the particles which resembled germs is, as I said, exceedingly various, and there are many reasons to suppose that if there are the germs of the organisms in the air, there must be an immense variety of them, a variety so great that we could not even venture to guess at its extent at present. When the liquid had free access to all of them, it is found, for reasons which would easily suggest themselves, on reflection, to anybody, that some of them, those which can thrive best upon the particular substance, develop themselves to the exclusion of the rest. I will give you one or two examples of the influence of food upon the development of ferments, instances which are well known, and are of some importance, as serving to prove the point which I have just mentioned. You are aware that the mixture which I have been speaking of, yeast-water with sugar, can be made to undergo alcoholic fermentation. I have already referred to it repeatedly in that point of view. We can make it undergo alcoholic fermentation if we put some alcoholic ferment into it, and keep it at a proper temperature; but if, instead of putting some number of cells—and even a few grains weight consist of an enormous number of cells—if, instead of that, we were merely to leave some of this liquid in contact with the air, we should have no alcoholic fermentation set up in it. That particular mixture of yeast-water and sugar does not, when exposed to all these germs, get yeast-cells developed in it, at all events, not to any perceptible extent. Instead of that, it gets cells formed which are similar to those in the second bottle I showed you, which is forming lactic acid; that is to say, the lactic fermentation will set in. The fact is, that the liquid is unwholesome for these particular cells, and does not agree with the alcohol cells, or yeast cells, so that if a great number of various germs are thrown into these particular substances, those which can thrive better, which are the lactic acid cells, develop themselves, and the alcohol cells do not. Again, if instead of taking this decoction of yeast and sugar, you were to take some grape juice, you would have alcoholic fermentation at once. That is the way it is done. If I were to leave a decoction of malt in contact with the air, in the same manner you would get the same thing set in as a rule. Again, if some of the liquid which I have in the glass dish here—some of the yeast-water with a little alcohol and acetic acid—be left in an open vessel, it gets an organism formed upon it; in fact, that is a process which Pasteur recommends for getting vinegar cells, if you want any. He says the air will, if you give it time, and supply the requisite conditions, start these cells in that mixture, but no alcohol cells, nor lactic acid cells, can be grown in it. It does not suit them; it is a substance which suits vinegar cells, and them only. Whatever may be the variety of the cells present in the air, it only developes those of that particular kind.

(To be continued.)

THE SUCRATE OF THE HYDROCARBONATE OF LIME APPLIED TO THE PURIFICATION OF THE SUGAR-CANE JUICE.

By MM. BOIVIN and LOISEAU.

What the authors call sucrate of hydrocarbonate of lime is a substance containing sugar, lime, and carbonate of lime, and in order to apply this material to the purification of saccharine liquids, a series of operations is required, of which the following is an outline:—

Production of the Lime and Carbonic Acid.—Limestone suitable for being burned is placed in a kiln, and the carbonic acid, evolved on being heated, is withdrawn by a pump; the caustic lime thus obtained constitutes, after having been slaked with the least possible quantity of water, the lime alluded to above.

Preparation of the Sucro-calcareous Solution.—A previously measured quantity of the saccharine liquid, one which it is desired to purify, is intimately mixed with lime so as to obtain the solution of that base; this liquid is next run into a large reservoir, but passes, previous to entering it, a sieve to retain undissolved particles.

Formation of Sucrate of Hydrocarbonate of Lime.—By the partial saturation, by means of carbonic acid, of the lime, contained and kept in solution by the sugar as just stated, the sucrate of hydrocarbonate of lime is obtained. This fluid is next boiled under pressure, next filtered, while the succeeding operations are, first, the withdrawal of the lime by excess of carbonic acid, boiling of the resulting liquid to expel the gas in excess, and cause precipitation of carbonate of lime; another filtration succeeds, after which the liquor (thick syrup) is filtered through animal charcoal, and lastly evaporated in vacuum pans. The deposited carbonate of lime is well-washed to extract any sugar it might contain, and the weak saccharine liquid thus obtained is used for the cleansing of the charcoal filters. The authors claim a great many advantages resulting from the process here briefly described; it is, however, undoubtedly lengthy, and one which, as has been observed, may lead to serious losses of sugar, especially in not thoroughly skilled hands.

ON THE OCCURRENCE OF SUPPOSED NATIVE COPPER.*

By HENRY BOWMAN.

During the months of March, April, and May last, we were burning at Washington a cargo of copper pyrites from the Tharsis mines in Spain. What appeared to be a block of pyrites broke the stone-breaking machine, being itself broken into three pieces. On being examined it showed marks of being much crushed, and turned out to be apparently a piece of native copper. The pieces exhibited weigh 21½ lbs., but as some smaller fragments may have been lost, the original mass was probably somewhat heavier. The colour varies in different parts from copper-red to almost white with a hackly fracture. The interior is full of cavities about shot size. The outer surface has no adherent matrix whatever. The specific gravity is 8·4. An analysis, for which I am indebted to Mr. Wm. Moody, showed the following composition:—

	Per cent.
Copper	84·80
Iron	5·54
Zinc	trace
Nickel	0·28
Cobalt	0·14
Antimony	0·50
Arsenic	7·10
Sulphur	0·73
Oxygen	0·41
	99·50

The sample for analysis was taken by breaking pieces off at different places, for in three samples the iron varied from 4·8 to 5·4 and 8·1.

With regard to its mode of deposition, I am inclined, without hazarding a positive opinion, to think that it has been reduced "in the dry way."

I am led to this conclusion by the appearances exhibited on the surfaces of fracture, which appear quite compatible with its having been reduced from a molten, but imperfectly fluid mass of heterogeneous composition, and this would appear to agree with the information afforded by analysis. The appearance of its outer surface seems, also, scarcely consistent with its having been reduced by any wet action from a mass of pyrites.

* Read before the Newcastle-upon-Tyne Chemical Society, October 27th, 1870.

I should add, that not hearing of the accident until two days after its occurrence, I have been unable to obtain a very satisfactory history.

In the discussion which ensued the specimen was generally considered as doubtfully authenticated.

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 27th, 1870.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

"*Observation of the Eclipse of the Sun, December 22nd, 1870,*" by J. B. Dancer, F.R.A.S.

The eclipse of the sun on Thursday, the 22nd of December, was favourably observed at Ardwick. Although a slight fog prevailed, all the details of the phenomenon were distinct and tolerably well defined. A number of spots were visible on the sun's surface, two of which were of some magnitude. The nuclei of these spots were linked together by maculæ, and surrounded by a penumbra which extended to a considerable distance. Faculæ also were very numerous and distinct. The approximate times of contact taken by a chronometer corrected by the standard clock at the Town Hall were as follows:—

	h.	m.	s.
First contact of the moon's limb with the sun	11	5	49
Contact of moon's limb with nucleus of the first large spot	11	31	36
With the nucleus of the second large spot	11	37	20
Last contact of moon's limb with the sun, Greenwich mean time	1	37	3

The temperature during the progress of the eclipse was taken at intervals by a mercurial thermometer with a black bulb *in vacuo*, exposed to the sun at the height of four feet from the ground.

Time.				Temp.
h.	m.	s.		Degrees.
11	10	0	.. ..	31·50
—	35	0	.. ..	30·25
—	45	0	.. ..	29·75
—	50	0	.. ..	29·25
12	22	0	.. ..	27·20
—	35	0	.. ..	28·50
1	37	0	.. ..	29·00

I had an impression that the moon's edge could be traced a short distance from the edge of the sun at the upper and lower points of contact, but this might be imagination.

The black surface of the moon appeared very uniform in colour. I tried with powers of 80 and 180 to distinguish the moon's disc, but did not succeed. Light clouds were passing over the sun's disc at this time. The diminution in light was quite perceptible at the time of the greatest phase.

Mr. Baxendell said that he observed the commencement of the eclipse at Cheetham Hill. The first contact took place at 11h. 5m. 46·2s. Greenwich mean time, or 24·2 seconds later than the time calculated by Mr. Dickinson and Mr. Hind. The definition of the limbs of the sun and moon, and of the spots on the solar disc was remarkably good, and he did not think his observation of the time of first contact could be in error to the extent of one second. The limb of the moon on the sun's disc appeared to be more sharply defined than the sun's limb. No distortion of the cusps was noticed. Unfortunately he was obliged to leave the observatory before the end of the eclipse, and therefore did not observe the time of last contact.

CORRESPONDENCE.

CHLORALUM.

To the Editor of the Chemical News.

Sir,—Permit me to protest in the most emphatic manner against Dr. Crace Calvert's system of experimenting on the comparative energy of antiseptics, and the conclusions published under his name in your journal. His tables prove that agents which are not antiseptics do not prevent putrefaction, and that antiseptics must be used in such a manner as to produce their effect or they are valueless. Dr. Calvert might have spared himself the trouble of placing "a known quantity" of chloralum beneath sound meat suspended by a thread in a wide-mouthed pint bottle. The chloride of aluminium is not volatile; nevertheless I have shown how, with a steam vapouriser, it is valuable in purifying the air, and may, under proper conditions, prevent taint in organic matters suspended in such air.

Dr. Calvert has not mentioned sulphurous acid, and yet I have for years kept whole carcasses of mutton and large joints and quarters of meat fresh for periods varying from one to nine months, after having suspended them for a few days in chambers through the atmosphere of which from one to three per cent of sulphurous acid had been diffused. Solutions of chloralum from 1005 to 1010 sp. gr., *i.e.* containing from 1 to 3 parts of chloride to 140 or to 70 parts of water, are sufficiently strong to keep fish and meat which have been simply dipped in them and then suspended in the dry air. The quantity of antiseptic used is but a small fraction of one per cent, and the preservation complete if the necessary precautions have been taken. According to my observations chloralum is more active than sulphurous acid.

Both sulphurous acid and chloralum are available when the carbolic and cresylic acids cannot be used owing to their smell, flavour, and causticity. I have employed the last-named agents very largely for several years, and they stand unrivalled for a limited number of medical and economic purposes, but chloralum covers a much larger field of useful applications, and we are not yet acquainted with a tithe of these. Ten years' experience with carbolic acid has proved that as a sick room disinfectant, and even in open alleys and stables, people refuse to use it. Condy's fluid, valuable in its way, has been supposed to act like carbolic acid and to disinfect, physicians prescribing it gladly, in the erroneous belief that it added to its deodorising properties that of destroying disease germs. Its success has been due to its freedom from odour. This is in a measure the secret of the great success attending the introduction of the harmless and odourless chloride of aluminium. The agent had never been thought of in therapeutics until last January, and the proof positive of its unequalled value is the extraordinary rapidity with which medical men have taken it up, and reported on its use in the treatment of wounds, arresting the fœtor of cancer, checking the throat lesions in diphtheria and scarlatina, preventing suppuration, absorbing from the air the odour of fresh paint, in a manner not yet explained, in addition to destroying many fœtid emanations which are simply masked by carbolic acid, and which are offensive, if not even actively poisonous.

We have lately succeeded in obtaining an odourless disinfecting powder of great strength, containing 30 per cent of chloride, which can compete in price with the 15 per cent carbolic acid powder. Its manufacture is delayed until suitable works can be erected, but any of your readers can obtain a small cask by applying direct to the Chloralum Company, and before the summer it is hoped that an adequate supply of this pleasant deodoriser and disinfectant may be available for dust-bins, earth-closets and many other purposes for which carbolic acid

powders or the more commonly used lime chloride cannot be employed.

Lastly—The London Cotton Mills, Limited, have undertaken the manufacture of wools and waddings containing a definite percentage of chloride of aluminium. Cotton wool thus treated is styptic and antiseptic. It may be used as a padding under bed sores, for the absorption of fœtid secretions, and suspended in the air as a filter of atmospheric currents. A curtain containing a layer of this wool, within a light porous covering suspended over the door of a sick chamber will very materially check the dispersion of fever poisons from a sick room into other parts of a house, and as usual with so useful an article, medical men will devise many other uses for this antiseptic chloralum wool, which have not yet occurred to your obedient servant,

JOHN GAMGEE.

1, Great Winchester Street Buildings, E.C.
London, January 4, 1871.

MAGNETIC POWER OF A BATTERY.

To the Editor of the Chemical News.

SIR,—Allow me a few lines in reference to Mr. Bottomley's letter in your last, in order to prevent any misconception. The problem he proposes is this. Given a certain number of cells, or given a certain area of platinum and zinc to cut up into plates; how shall the cells be arranged, or how shall the area of platinum and zinc be cut up so as to give a maximum of intensity? The answer to this Mr. Bottomley very correctly gives, namely, when the cells or plates are so arranged as to make the internal resistance equal to the external.

The problem I was speaking of was the following:—Given a certain battery arranged in a certain manner—by what external resistance shall we get most work out of it? Now in this case, curiously enough, we can get most *heat* when the external resistance equals the internal, but we do not also get the most magnetic power. And again, in Mr. Bottomley's problem, though he gets most intensity by his arrangement, he does not get most heat. It is the confusing together of the two problems and the two results, as is often done, that I meant to complain of in a former letter.—I am, &c.,

H. HIGHTON.

Putney, December 24, 1870.

MISCELLANEOUS.

Royal Institution of Great Britain.—The following are the probable arrangements for the Friday Evening Meetings before Easter, 1871, to which Members and their friends only are admitted:—Friday, Jan. 20th. Professor Tyndall, LL.D., F.R.S., M.R.I. Jan. 27th. Professor Odling, F.R.S. Feb. 3rd. W. Spottiswoode, Treas. R.S. and R.I., "Some Experiments on Successive Polarisation of Light made by Sir C. Wheatstone." Feb. 10th. E. J. Reed, C.B., "On Some Fallacies connected with Ships and Guns." Feb. 17. James N. Douglass, Engineer to the Trinity House, "On the Wolf-Rock Lighthouse." Feb. 24th. Dr. W. B. Carpenter, F.R.S., &c. "The Latest Scientific Researches in the Mediterranean and Straits of Gibraltar." March 3rd. Captain Noble, F.R.S., "On the Pressure of Fired Gunpowder." March 10th. W. Mattieu Williams, "On Rumford's Scientific Discoveries." March 17th. J. Norman Lockyer, F.R.S., M.R.I., "On the Eclipse." March 24th. Professor J. Clerk Maxwell, M.A. F.R.S., "On Colour." March 31st. Professsor Max Müller, LL.D., "On Solar Myths."

The Comparative Energy of Disinfectants.—We published last week a note on a series of experiments by Dr. Crace Calvert on this subject. Mr. Bollman Condy writes:—"Among the bodies enumerated was permanganate of potash (Condy's fluid). This circumstance has constrained me to venture to direct your attention to the fact, that that substance is not an antiseptic at all; nor, so far as I am aware, has it been patronised as such by those having an interest in saying most in its favour; at any rate, I can vouch for this being so in my own case. I have always been careful to point out that Condy's fluid is not an antiseptic or preserving agent, but a disinfectant, in the same sense wherein fresh air is one. You will, I think, agree with me that the latter agent namely, air, which is the one whereby ventilation operates as a disinfectant, is no antiseptic. One of the great aims in the preservation of food, for instance, is the exclusion of air. It would, therefore, seem to me that to class the permanganates with antiseptics, and to experiment with them as such, is much the same thing as to class in that category pure air and experiment with it in the apparent expectation that it might prove capable of preserving from decomposition organic bodies. I cannot see that any good would arise from proving that pure air has no antiseptic value; but, on the contrary, believe that only misapprehension as to the value of that essential to health would thereby be engendered in the public mind, which is not yet sufficiently instructed to distinguish between antiseptics and disinfectants. In the same way, experiments undertaken to prove that permanganates are destitute of antiseptic properties, which every one qualified to experiment already knows, instead of being of any service to sanitary science, must, on the contrary, when publised, be detrimental thereto, by erroneously leading popular opinion to conclude that they are, therefore, inefficient disinfectants, which is exactly the contrary of the truth. The disinfecting virtues of those agents depend precisely on the same principle as that whereon the efficacy for disinfection of ventilation hangs; namely, the essential element oxygen, which plays the main part in all true sanitation."—*British Medical Journal.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Polytechnisches Journal von Dingler, second number for November 1870.

This number contains the following original papers relating to chemistry and allied sciences:—

Method of Tinning Copper, Brass, and Iron at the Ordinary Temperature, and without the Intervention of any Apparatus.—F. Stolba.—The author states that all objects to be tinned should first be perfectly cleaned from oxide, as well as greasy matter; the cleaning may be effected either by mechanical or chemical means. The ingredients employed are finely-pulverised zinc and a carefully-made solution of protochloride of tin, containing from 5 to 10 per cent of that salt, to which solution about a small pinch of cream of tartar should be added. This solution is rubbed over the object to be tinned, and immediately after some of the finely-pulverised zinc is also rubbed on; a sponge, or a piece of cotton-waste free from grease, is the best for the purpose of rubbing on the solution of tin and the zinc powder. When the tinning is complete, the object is washed with water, and next polished with previously-washed and pulverised chalk.

Iodide of Silver in its Photographico-Chemical Relation.—Dr. J. Schnausz.—We learn from this essay that there exist two modifications of iodide of silver, one of which is actinic—that is to say, sensitive to light—while the other is not so. The author also points out that iodide of silver is very soluble (up to equal equivalents) in a solution of nitrate of silver, forming therewith a double salt, AgO,NO_5+AgI, which, if the solution of the nitrate is quite neutral, is far more sensitive to light than either of the two salts by themselves.

Memoir on the Specific Gravities which Agree with the Degrees of the Areometers in General Use.—Dr. G. Th. Gerlach.—This very valuable paper consists of a lengthy series of tabulated forms showing the number of degrees of the various areometers in general use, and the specific gravities corresponding thereto.

Conversion of Starch into Sugar by the Action of the Diastase of Malt.—Dr. A. Schwarzer.—This essay is not well suited for any useful abstraction, owing to the fact that it contains a series of tabulated forms exhibiting results of experiments. It appears, however, from the author's researches, that—(1) the conversion of starch is quickened by an increased quantity of diastase, as well as a higher temperature; (2) that when starch fails to be detected by iodine, the formation of sugar is nearly finished; (3) at all temperatures from 60° to 0° downwards, and the application of varying quantities of diastase, there are formed always from 50 to 53 per cent of sugar; (4) at temperatures above 60° less sugar is formed than at temperatures below that degree; (5) a temperature of 70° impairs, and in some instances destroys, the activity of diastase.

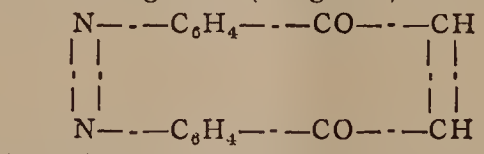
Smoke-Consuming Fire-Place suited for every kind of Fuel.—A. Wagner.—A description of a peculiarly-constructed kind of stove arranged for use either in kitchens or for heating apartments.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 17, 1870.

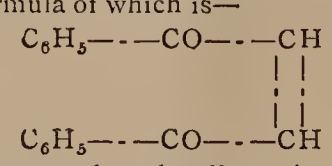
This number contains the following original papers and memoirs:—

Method of Water Analysis.—A. Müller.—The third part of this lengthy memoir, treating on the estimation of sulphuric acid.

Synthesis of Indigo-Blue (Indigotine).—A. Emmerling and C. Engler.—This very exhaustive memoir is divided into the following sections:—On the nitro-derivatives obtained from aceto-phenon (the methyl-keton of benzoic acid, C_8H_8O, obtained by the dry distillation of equal parts of benzoate and acetate of lime, a liquid boiling at 198°). Conversion of mononitro-aceto-phenon (in the state of thick syrup) into indigotine; this process consists in the withdrawal of water, and in a reduction. Constitution of indigo-blue and its derivatives. The structural formula of indigo-blue (indigotine) is—



According to the authors, the indigo-blue is the nitro-derivative of a peculiar keton, the formula of which is—



The memoir contains a very lengthy discussion, illustrated by a great number of formulæ, on a series of bodies belonging to the indigo derivatives.

Chloride of Iodine.—L. Henry.—The author has applied chloride of iodine for the purpose of decomposing some of the oxy-compounds of chlorine. In water kept very cold, and in which some iodine is suspended, a current of chlorine gas is passed until the iodine is dissolved; and to this solution is next added 1 molecule of chlorate of potassa—viz., 122·5 parts for every atom (127 parts) of iodine dissolved. On being next heated, the following reaction takes place, chlorine being evolved:—$KClO_3 + ICl = KIO_2 + ClCl$. On cooling, pure iodate of potassa crystallises from the liquid.

Aromatic Glycolic Acid.—W. Dittmar and A. Kekulé.—This paper treats, first, on the relation existing between a series of aromatic acids, and then on the body discovered by the authors—viz., oxymethyl-phenyl-formic acid—

$$C_6H_4\begin{cases}CH_2.OH\\CO.OH\end{cases}$$

This substance is a solid crystalline body, soluble in hot, and to some extent also in cold water; also in ether; its point of fusion is higher than that of toluylic acid. The oxymethyl-phenyl-formic acid is capable of being sublimed.

Synthesis of Substituted Guanidines.—E. Erlenmeyer.—Hydrochlorate of guanidine may be prepared synthetically by heating an alcoholic solution of chloride of ammonium with cyanamide. When hydrochlorates of aniline, toluidine, and methylamine were caused to act upon cyanamide in the same manner, the author obtained the corresponding hydrochlorates of phenyl, tolyl, and methyl-guanidine. The author's paper is further chiefly devoted to the crystallographical description, illustrated by diagrams, of the platinum double salt of methyl-uramine.

Acids which are formed by the Oxidation of the Fermentation Butyl-Alcohol.—E. Erlenmeyer.—After referring, at great length, to the researches made on this subject, by Dr. Michaelson, in the year 1864, the author states that he found that, among the products of the oxidation of the butyl-alcohol of fermentation, isobutyric acid is present, which, by further oxidation, is readily converted into water and acetic and carbonic acids. This process is illustrated by the following formula:—$C_4H_8O_2 + O_6 = (CO_2)_2 + (H_2O)_2 + C_2H_4O_2$. The author points out that, by means of this process, isobutyric acid may be distinguished from normal butyric acid, which, on being oxidised, yields propyl-butyric acid.

Valerianic Acids of Different Origin.—E. Erlenmeyer.—The chief points of interest in this paper are—Valerianic acid from valerian root is optically inactive and chemically identical with the valerianic acid obtained from inactive amyl alcohol, and from the cyanide of isobutyl, each of which three acids yields with baryta a readily crystallisable salt. The valerianic acid obtained from active amyl-alcohol, and also the acid obtained by the oxidation of leucine from albumen compounds, are optically active, have a somewhat higher specific gravity, a lower boiling-point than the inactive acid, while, moreover, the baryta salt of the former is an amorphous substance. The optically-active acid is converted into an inactive, by applying heat (200°) and a few drops of concentrated sulphuric acid to it.

New York Oxyhydrogen Gas Company.—H. Vogel.—This paper contains a description, illustrated by woodcuts, of the works of the company just named. The process of the manufacture of oxygen, as well as of hydrogen, is that invented by M. Tessié de Mothay. The author, while on a visit to New York, had an opportunity of viewing the works.

Naphthazarine.—C. Liebermann.—This lengthy paper contains the results of the author's researches on the pigment obtained in 1861, by M. Roussin, by heating to 200° binitro-naphthaline along with zinc and sulphuric acid. The author of this paper finds that the body alluded to is bioxy-naphthochinon, which stands in the same relation to naphthaline as alizarine stands to anthracen—that is to say, that naphthazarine—

$$C_{10}H_4\begin{cases}(OH)_2\\O_2\end{cases}$$

is to be considered as the alizarine of the naphthaline series.

Action of Chlorine upon Absolute Alcohol.—This very lengthy essay contains the exhaustive description of a series of experiments made by the author to determine what the final result is of the action of chlorine upon absolute alcohol. The body thereby obtained is termed chloralcoholate—

$$C_4H_7Cl_3O_2 = C_2HCl_3\begin{cases}OC_2H_5\\OH\end{cases}$$

It is a solid substance; crystalline; fuses at 46°; boils at 112·5°; is difficultly soluble in water, by which property this substance is distinguished from chloral hydrate. The theoretical vapour density of the chloralcoholate is 6·68; but it appears that this substance is split up at a higher temperature into chloral and alcohol. The memoir contains an exhaustive discussion, illustrated by a series of formulæ, on the mode of origin of the chloralcoholate from trichloracetate.

Electrolysis of some Chemical Combinations.—N. Bunge.—This paper contains the following sections:—Electrolysis of mercaptans; of (KHS); of sulpho-compounds; of trichlor-methyl-sulphate of potassa, (CCl_3SO_2OK). The author continues his extensive researches on this subject.

Pneumodensimetro Automatico.—A. de Negri.—The description, illustrated by woodcuts, of an ingenious modification of Dr. Bunsen's apparatus for the measurement of the density of gases by means of electricity—that is to say, by the aid of galvanic element and dial apparatus with clock-work, the instrument is made self-indicating.

Test for Detecting the Adulteration of the Colouring Matter of Red Wine.—MM. Cottini and Fantogini.—The authors take 50 c.c. of the red wine to be tested; add to it 6 c.c. of nitric acid, sp. gr. 1·412, and heat to about 90°—95°. Pure and unadulterated wine is not affected, even when heated so for one hour; while artificially-coloured wine is decolourised in five minutes.

Annalen der Chemie und Pharmacie, November, 1870.

This number contains the following original papers and memoirs:—

Allyl-Ether of Formic Acid.—B. Tollens, R. Weber, and Th. Kempf.—This paper is the first of a series forming an exhaustive essay on the allyl group. The following papers belonging to and forming part of this memoir are:—

Preparation of Allyl-Alcohol.—B. Tollens and A. Henninger.

Investigation of Mono-Allyline.—B. Tollens.

Haloid Ether of the Allyl-Alcohol.—B. Tollens.—This paper contains the following sections:—Allyl-bromide; allyl-chloride; allyl-iodide; allyl-sulphide; and allyl-sulpho-cyanide.

Experiments made with the View of Combining Allyl-Alcohol with Hydrogen.—B. Tollens.

Compounds of Allyl-Alcohol with Chlorine and Bromine.—B. Tollens.—This paper contains the following sections:—Chloride of allyl-alcohol; bromide of allyl-alcohol, $C_3H_6Br_2O$.

Synanthrose, a New Carbo-Hydrate of the Synantherеæ.—O. Popp.—The substance designated as synanthrose has been discovered by the author in the tubers of some plants belonging to the family of the *Compositæ*, or *Synantherеæ*. This new carbo-hydrate is a peculiar kind of sugar, which the author obtained from dahlia tubers (*Dahlia variabilis*) by a lengthy and complicated process of preparation. Pure synanthrose exhibits the following properties:—White-coloured, amorphous, solid body; very deliquescent; readily soluble in water and dilute alcohol; insoluble in absolute alcohol and in ether; chemically and optically inactive; does not reduce alkaline copper solution; incapable of entering directly into fermentation; dilute acids convert it into dextrose and levulose; not coloured brown by a cold solution of caustic potassa, but very readily acted upon by strong sulphuric acid; a cold solution of synanthrose is precipitated by nitrate of silver solution; on heating, the silver is reduced to the metallic state. The elementary composition of this body is $C_{12}H_{22}O_{11}$; that is to say, synanthrose is a metamer of cane sugar.

Inuloid, a Soluble Modification of Inuline.—O. Popp.—The substance which the author describes as inuloid is, in many of its properties, akin to inuline, but differs from it by being readily soluble

in water. The formula of this body is $C_{12}H_{10}O_5+H_2O$. Inuloid does not reduce alkaline copper solution; when boiled with water for some time, this substance is converted into levulose. Inuloid is contained in some not fully-ripened tubers and roots of some plants which, in ripe state, contain inuline.

Researches on the Isomerism of the Benzol Group: the Derivatives from Ethyl-Benzol.—(Thirteenth paper.)—F. Beilstein and A. Kuhlberg.—This memoir contains the following sections:—α and β nitro-ethyl-benzol; ethyl-phenol—

$$C_6H_4\begin{cases}HO\\C_2H_5\end{cases}$$

ethyl-paraoxybenzoic acid—

$$C_6H_4\begin{cases}O.C_2H_5\\CO_2H\end{cases}$$

Constitution of Xylylic Acid, Para-Xylylic Acid, and on a New Modification of Dimethyl-Benzol.—R. Fittig and P. Bieber.—This paper is the fourth portion of an exhaustive memoir on trimethyl-benzol.

Synthesis of Hydrocinnamic Acid.—R. Fittig and J. Kiesow.—This essay contains the following sections:—Phenyl-ethyl-chloride, $C_6H_5.C_2H_4Cl$; phenyl-bromide; preparation of hydrocinnamic acid. The formula of this body is $C_9H_{10}O_2$, while its calcium compound is $(C_9H_9O_2)_2Ca+1\frac{1}{2}H_2O$.

Ethyl-Phenol.—R. Fittig and J. Kiesow.—Ethyl-phenol—

$$C_8H_{10}O=C_6H_4\begin{cases}C_2H_5\\HO\end{cases}$$

is a crystalline solid body, fusing at about 48°, boiling at about 211°, somewhat soluble in water, and very readily so in alcohol and ether. Tetra-bromethyl-phenol—

$$C_8H_6Br_4O=C_6Br_4\begin{cases}C_2H_5\\HO\end{cases}$$

a solid body, crystallising in quadrilateral prisms, insoluble in water, and fusing at 106°; its calcium-salt is $(C_8H_5Br_4O)_2Ca$.

December, 1870.

This number contains the following original memoirs and papers:—

Researches on the Constitution of Twice (Zweifach) Substituted Benzols.—V. Meyer.—This very lengthy essay is divided into the following sections:—Introduction, containing a review of the labours and researches on substitution in general; new method of synthesis of aromatic acids—viz., benzoic, naphtoë, and isophthalic acids; regularity observable in the substitution of benzol derivatives; dibrom-benzol; substitution of aromatic amides; sulphanilic acid; formula of benzol; constitution of triderivatives of benzol; on the method of designating the benzol derivatives.

Researches on the Position of the Hydrogen Atoms of Benzol.—H. Hubner and J. Alsberg.—This memoir, akin to the foregoing, not well suited for any useful abstraction, is divided into the following chapters:—Introduction (containing the following brief definition of chemistry—"Chemistry is that portion of molecular physics which treats of the mutual attraction of atoms as units"); aniline from bromnitro-benzols; researches on acetanilide from pure nitrobenzol. The chief result deduced from their researches by the authors is, that the hydrogen atoms of benzol are mutually in the same position.

Journal für Praktische Chemie, No. 17, 1870.

This number contains the following original papers and memoirs:—

Oxidation of Albuminates to Urea.—O. Loew.—This paper contains an account of some experiments made by the author with the view of verifying the correctness of the various statements made by several experimenters, on the successful oxidation of albumen and various protein compounds, so as to obtain urea therefrom. The author's experiments lead to a negative result in this direction.

Some of the Causes by which the Variety of the Crystalline Form of Carbonate of Lime is brought about.—Dr. H. Credner.—This very lengthy essay, illustrated by a series of lithographs, is divided into the following sections:—Introductory observations; crystallo-genetic experiments made with carbonate of lime; experiments made with a cold solution of pure bicarbonate of lime; experiments made with the same solution, with the addition of silicate of potassa; with silicate of soda; with a mixture of the two solutions of silicate; experiments made with a solution of bicarbonate of lime and bicarbonate of strontia; experiments made with a solution of bicarbonate of lime to which a solution of gypsum was added; experiments made with a cold solution of bicarbonate of lime to which salts of lead were added. From the general results of the author's experiments we quote the following:—The addition of certain substances to the solutions of minerals affect the crystalline form and the number of planes of the resulting crystals. The addition of various substances to the solutions of a species of any mineral becomes an impulse to the formation of totally different mineral species. When carbonate of lime crystallises from not too dilute, but perfectly pure, solutions in the shape of calc spar, it assumes the shape of arragonite upon the addition of small quantities of carbonate of lead, sulphate of lime, or carbonate of strontia. The differences of temperature, and of the degree of concentration of the solutions of carbonate of lime, are not, therefore, the only causes of the dimorphismus of carbonate of lime.

Occurrence of Amorphous Native Sulphide of Mercury.—Dr. G. E. Moore.—This paper contains the account of the discovery and mineralogical description of a peculiar mercury ore met with in the State of California, U.S. The mineral referred to is amorphous, coloured like graphite, with a metallic lustre; sp. gr., 7·701 to 7·748; behaves, when heated in blowpipe-flame, like cinnabar; and sublimes when heated in a small flask. Composition, in 100 parts—Sulphur, 13·82; mercury, 85·79; iron, 0·39; quartz, 0·25. Formula, $HgS+FeS_2$. This mineral occurs in abundance in the Bedington mercury mine, Lake County, California.

Two New Compounds of Nicotine with the Chlorides of Zinc and Cadmium.—Dr. H. Vohl.—The double salts herein described exhibit the following properties:—The nicotine-zinc salt is a crystalline body, soluble in water, very difficulty soluble in ether and absolute alcohol, and slightly acid reaction. Formula—

$$(C_{10}H_7N+ClH)+(ZnCl)+4HO.$$

The cadmium-nicotine salt is also crystalline, and agrees in its general properties with the zinc salt. The formula of the cadmium salt is $(C_{10}H_7N+ClH)+(CdCl)+2HO$.

Presence of Amygdaline and a Substance Resembling Asparagine in the Seeds of Vicia Sativa (Vetch).—H. Ritthausen and Dr. U. Kreusler.—Only a small and incomplete portion of this paper is published; it will be continued in the next number of this periodical.

Journal für Gasbeleuchtung, November, 1870.

The original papers contained in this number bear chiefly on the water supply and waterworks of some towns in Germany.

NOTES AND QUERIES.

Solubility of Tin in Nitric Acid.—Neither Mr. Hay nor Mr. Scott has mentioned the fact, which I remember observing when I once had some tin dissolved in dilute nitric acid, viz., that the solution is accompanied, as might be expected, with an evolution of ammonia.—H. B. Gibbins.

Solubility of Tin in Nitric Acid.—Can any of your readers point out any paper in which it appears that the fact of the solubility of tin in nitric acid was published previously to my announcement of it in your journal of December 16th, 1870? I myself know of no such paper.—George Hay.

Emerald Green.—(Reply to "Emerald.")—There are various methods in use for making the green colour you refer to, among these we quote the following:—Take one kilo. of pure sulphate of copper free from iron, dissolve it in twelve litres of water, and add thereto 350 grs. of arsenious acid, and one kilo. of refined pearl-ash, dissolved together in eight litres of water; the latter solution should be added to the former, care being taken to stir the fluids. The ensuing precipitate should be washed with warm water and dried. For further information consult Gentele's "Lehrbuch der Farbenfabrikation," often quoted by us previously.

MEETINGS FOR THE WEEK.

Monday, Jan. 16th.—Medical, 8.
Tuesday, 17th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Zoological, 9.
Wednesday, 18th.—Meteorological, 7.
—— Society of Arts, 8.
Thursday, 19th.—London Institution, 7.30.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoverics."
—— Royal, 8.30.
—— Chemical, 8.
—— Royal Society Club, 6.
Friday, 20th.—Royal Institution, 9. Professor Tyndall.
Saturday, 21st.—Royal Institution, 3. Rev. W. H. Channing, "On Laws of Life Revealed in History."

TO CORRESPONDENTS.

W. Hamilton Reid.—Johnston's "Lectures on Agricultural Chemistry" is the best work. If it is out of print you can see it at the Library of the Commissioners of Patents.

J. D. M.—As far as we can make out your writing of the words, the translation would be "Nordhausen fuming double vitriol" but, in all probability, the words refer to fuming, or so-called Nordhausen, sulphuric acid.

S. E. Phillips.—Declined with thanks.

W. F. K. Stock.—The article would occupy a whole number of the Chemical News; it has already appeared. You will find full particulars in the 3rd edition of Mitchell's "Manual of Practical Assaying."

Delphinus.—Ure's Dictionary is published by Longmans and Co., price £4 14s. 6d.; it probably contains the information you require.

China.—There is no English translation. Messrs. Hachette and Co., Foreign Booksellers, Strand, will supply you; the price is about 17s.

BOOKS RECEIVED.

The Retrospect of Medicine, edited by W. Braithwaite, M.D. and James Braithwaite M.D. Lond. Vol. lxii. London: Simpkin, Marshall, and Co.

Lessons in Elementary Physics. By Balfour Stewart, LL.D., F.R.S. London; Macmillan and Co.

Beet-Root Distillation, containing a Report on Beet-Root Distillation, and the Savalle Stills. By Dr. Augustus Voelcker, F.R.S.

Popular Science Review.

Introduction to the Study of Inorganic Chemistry. By William Allen Miller, M.D., LL.D., F.R.S., &c. London: Longmans, Green, and Co.

A Laboratory Text-Book of Practical Chemistry. By W. J. Valentin, F.C.S. London: John Churchill and Sons.

Inaugural Lecture of the Industrial and Technological Museum.

Journal of the London Institution.

Henderson's Patent Processes for Refining Cast-Iron and for the Production of Steel and Wrought-Iron.

Essays on the Use and Limit of the Imagination in Science, By John Tyndall, LL.D., F.R.S. London: Longmans, Green, and Co.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 582.

ON THE

EXAMINATION OF THE BESSEMER FLAME WITH COLOURED GLASSES, AND WITH THE SPECTROSCOPE.

By J. SPEAR PARKER.

I have been engaged for some time in investigations as to the nature of the Bessemer flame at the Cyclops Steel and Iron Works, on behalf of Messrs. Charles Cammell and Co., and as the subject is one of considerable interest at present, I forward you the results I have obtained, with their kind permission.

The combinations of glasses used by Mr. Rowan and Professor Silliman for observing this flame are somewhat similar in their effect, the two light yellow in the one case being nearly equivalent to one dark yellow in the other. The combination I used was one cobalt-blue glass of rather light shade, and one amber-coloured; too deep a blue should be avoided, otherwise the flame merely shows varying shades of crimson. The appearances observed were similar to those recorded by Professor Silliman. On first turning on the blast, the flame appeared of a reddish orange; after the lapse of a few minutes, this gradually changed to crimson, first in flashes, afterwards continuously; the sodium line appearing in the spectroscope at exactly the same time, and corresponding also to the flashes; after a few more minutes had passed, the flame altered to yellow, and, at that time, the characteristic Bessemer spectrum appeared, flickering at first like the sodium line; when it became steady, the flame, when viewed through the coloured medium, appeared to consist of a sheath of a light yellow colour, the inner cone being rose-red, while the upper part of the flame was edged with crimson; this appearance was maintained throughout the rest of the decarbonisation, only varied by occasional flashes of a deeper red, or of a greenish hue. When the termination of the reaction was attained, the flame, after a few premonitory flashes, changed again to the crimson colour. The appearance of a crimson ring round the mouth of the converter I am inclined to attribute to irradiation.

With regard to the utility of this method, it is evident that, as the changes seen are owing to the same phenomena as those observed by the naked eye, it cannot be more accurate than the method now in general use, the experienced eye of the workman being able to detect the change with surprising precision; it has, however, the advantage that more inexperienced hands may thus find a safe guide, and it is especially of benefit in protecting the eyes of the observer from the bright glare of the naked flame, a light so intense that it must, I think, in the long run, prove very injurious to the sight.

The indications of the spectroscope, however, have a different basis. The instrument I have used in my investigations is of the direct vision form, containing five glass prisms, and, although for comparisons with other spectra and for scientific investigations, the ordinary form is, undoubtedly, best fitted, I think that, should the spectroscope be more generally introduced and placed in the hands of those unaccustomed to the use of scientific instruments, the direct vision is preferable, for after once focussing it may be used like a telescope, and is less liable to need re-adjustment.

The spectrum, as observed with this instrument, agrees very closely with the one represented on page 125 of the first edition of Roscoe's "Spectrum Analysis." Between the lithium (a) and sodium lines appears the first prominent group in the red, consisting of two bright lines in close juxtaposition, a band of bright light gradually fading towards the least refrangible end of the spectrum; shortly after this is a bright line in the orange-red; and three narrow and feebly luminous lines occur between this and the brilliant sodium lime, which is closely followed in the greenish yellow by two lines of moderate brightness. At the commencement of the green occurs another most characteristic group; it is very similar in general appearance to the one mentioned in the red, consisting of two broad very bright green lines in close proximity, with a band of light fading towards the yellow. In each of these cases, the edges of the lines are hazy; in the green group, however, a narrow line may be observed on each side of the two brightest; after a short gap in the spectrum, another group of five bright green lines occurs; most of these are narrower with well-defined edges. The apparent presence of an absorption line in this group is merely owing, I think, to the effects of contrast. Beyond this, occur two sharply-defined lines in the greenish blue. I have not succeeded in seeing with certainty any more refrangible lines in the Bessemer spectrum, but such would, doubtless, be visible with a more perfect instrument. The red potassium line was not very readily observed on account of its occurring on the extreme left of the field of view, and its comparatively feeble luminosity. The above mentioned are all I have been able to define, but, towards the close of the process, when the temperature becomes most intense, numerous others appear, those portions of the spectrum which previously seemed blank being filled with fine lines; this is especially observable in the considerable space extending from the lithium line to the first prominent red group.

With regard to the order of their appearance in the spectrum, after the sodium lime, I have always observed the red and the first green group mentioned above together with the intervening lines appear simultaneously; at first alternately brightening and disappearing, then continuously. The lithium line is rather variable, sometimes becoming visible before, sometimes after, the first appearance of these lines. The more refrangible green group and the lines in the geenish blue usually become visible rather later; probably, however, this is owing to their inferior brightness. When the time is reached for the termination of the blowing, the whole of the lines disappear almost at the same moment, except the two due to the presence of sodium and lithium, which remained whenever sufficient time was left to observe them.

Dr. Marshall Watts has called attention to the peculiar spectrum of the flame which is produced when spiegeleisen is added to the decarbonised iron; on most occasions the spectrum I have seen corresponds with his description: once or twice I have observed, in addition, a group of several well-detailed lines in the bluish violet, these being developed only when the flame is very strong, in consequence of the charge having been slightly overblown; while, sometimes, the ordinary spectrum of the Bessemer flame is alone produced. Although, at first sight, these two appear to differ considerably, it is evident as he has shown, that it is merely an alteration of the relative intensity of the various lines. The peculiar spectrum of this flame is especially marked by the great prominence and brightness of many of the lines in the green, and hence the flame appears of a greenish yellow when viewed through the coloured medium.

The indications of the spectroscope, and the appearance of the flame, as seen through the coloured glasses are, therefore, found to be in perfect accordance; the continuous spectrum without any bright lines corresponds to the reddish orange flame; when the spectrum with the sodium line is produced the flame appears crimson; as soon as the characteristic Bessemer spectrum becomes visible the flame alters to a light yellow; and at the termination of the decarbonisation, when the continuous spectrum with

the sodium line is again produced, the flame at the same moment alters also to crimson as before.

Professor Lielegg states that he has seen the Bessemer spectrum when the converter has been heated with coke merely; I have not been so fortunate, as I have never seen, under such circumstances, more than the red potassium, the sodium, and the lithium lines.

All the earlier observers concurred in attributing the variable lines to the spectrum of carbon in some form: the statement that they are due to the presence of manganese, therefore, will occasion considerable surprise. While it may be that some of the sharply-defined and more refrangible lines correspond with the spectrum of that element, it does not seem likely that the first green group for instance, or the prominent red group, which are so similar in their general appearance to the bands in the usual carbon spectrum, can be owing to that cause; nor does Mr. Huggins describe any lines as occurring in those portions of the manganese spectrum (*Philosophical Transactions*, 1864). Their appearance, at the time when decarbonisation commences, and their disappearance at the moment when it is completed, afford very strong presumptive evidence in favour of the carbon hypothesis, but an observation I was fortunate enough to make, places it, I think, almost beyond a doubt.

The peculiar spectrum of the flame which rushes out of the converter when the spiegeleisen is added has already been adverted to; this is also observed when the charge is poured out into the ladle. I was very much astonished, however, to see it produced from the stream of steel pouring into the ingot moulds, and, on looking for the cause, I observed a large flame rushing out of the mould; on repeating the observation I only saw the spectrum when this flame was produced, corresponding also with it in comparative brightness. It is caused by the *graphite* with which the moulds are lined to fill up any cracks or crevices, and thus facilitate their ready separation from the ingots, and must, therefore, be due to the combustion of carbon by the air carried down with the stream of metal at an intense white heat. The brightest lines only were observed, especially the two green groups, and they corresponded with the altered spectrum produced by the spiegel flame.

To ascertain the nature of the Bessemer spectrum with absolute certainty, observations are required with more powerful and delicate instruments than those hitherto used, but I think there is little doubt that the most characteristic portion of the spectrum will be found to be owing to the presence of carbon in some form.

AN ACCOUNT OF THE MANUFACTURE OF COPPER ON THE TYNE.*

By R. CALVERT CLAPHAM.

The manufacture of copper, as now extensively carried on, has introduced a new industry in this district. Up to the year 1850, no copper was produced on the Tyne; in 1851, J. and W. Allen commenced soda works at Wallsend, to work out Mr. Longmaid's patent, and they extracted as much copper as produced four tons sulphate of copper weekly; a few years later Mr. Gossage erected copper works at Wellington, Mr. Russell at Walker, and Mr. Mease at Jarrow, to work up the poor copper ores then chiefly obtained from Cornish and Irish pyrites. The regulus, or precipitate, was sent to Swansea, to be smelted for copper. H. L. Pattinson and Co. began to smelt copper ores, obtained from burnt pyrites, in 1858; the regulus also being sent to Wales. Up to this date the total copper regulus, &c., made on the Tyne, would not represent more than 400 tons of copper annually.

In 1865, copper works were erected at Hebburn, to carry out the so-called Henderson process, for extracting copper by the wet method, from ores obtained from Spanish pyrites. Since that date there has been a rapid increase of production in the older works, and recently others have been erected for a similar purpose by the Bede Metal Co., at Jarrow.

The total quantity of copper produced on the Tyne in 1869 was 4100 tons, and of this quantity about 280 tons consisted of sulphate of copper, and the value of the products was about £340,000. This production has been greatly extended in the present year. From the facilities now at hand for the disposal of the burnt copper pyrites produced in the district, instead of sending them all the way to Swansea, and the cheap rate at which copper pyrites can be supplied, it appears pretty certain that this ore will soon take the place of non-cupreous ores. The total consumption of pyrites, of all kinds, in the United Kingdom, for 1869, was nearly 400,000 tons, namely:—

	Tons.
Imported from Norway	63,091
,, Holland	13,983
,, Portugal	140,805
,, Spain	99,648
,, Other parts	2,420
	319,947
Produced in Ireland	56,291
,, Cornwall and other parts ..	19,658
	395,896

And of this quantity 265,453 tons consisted of copper pyrites, namely:—

	Tons.
Imported from Norway	25,000
,, Portugal	140,805
,, Spain	99,648
	265,453

Large as this importation is, it may be interesting to know that the importation of pyrites for this year will not be far short of 400,000 tons. The importance of this fact on the future development of the soda trade cannot be exaggerated, and, indeed, these manufactures—soda and copper—are every year becoming more dependent on each other. The erection of copper works in this district has already had a marvellous effect in increasing local importations, and at the same time reducing the cost of the sulphur to the soda maker, the price having fallen 40 per cent since 1865.

As the plan of manufacture generally conducted in the district is well known to most of the members, I need only briefly refer to it. The pyrites most suited for the purpose is obtained from Spain, and contains less silica and other impurities than most other pyrites.

In the first place the sulphur is nearly all extracted by the soda maker in producing his sulphuric acid; the ores are then sent to the copper works: at this stage the better kinds contain about 3 to 5 per cent copper, 4 per cent sulphur, 4 per cent silica, small percentages of silver and lead, and the balance is peroxide of iron. It is mixed with a certain weight of common salt, the quantity depending on the percentage of sulphur left after burning. The whole is ground to a fine powder, and is then placed in long reverberatory furnaces, which are kept at a moderate heat. During this operation the sulphur is converted into sulphuric acid, and the salt is at the same time decomposed; the hydrochloric acid acts upon the copper, and, provided the heat in the furnace is not kept too high, the whole of the copper is rendered soluble and can be washed out of the residue. A part of the chloride of copper is, however, driven off by the heat and passes off into the condensers along with the free hydrochloric acid, and is there condensed. The acid solutions from the towers is

* Read before the Newcastle-upon-Tyne Chemical Society.

of a distinct greenish blue colour, showing the presence of copper.

When the ground material in the furnace is complete, it is thrown into wooden tanks, and digested in water and the acid solution from the condensers, the acid assisting most materially in dissolving out the metals. The liquors run off from the tank are necessarily of a mixed nature, and consist of chlorides of copper, silver, and lead, and undecomposed common salt, also sulphates of soda and lead, &c.

To obtain the copper, the liquors are treated with old scrap iron or spongy metallic iron, which is made from the residue ores, by heating with coal in a muffle furnace. In both cases the copper is precipitated, but when spongy iron is used the action is much more rapid. The precipitate thus obtained is frequently washed in water to free it as much as possible from the salts of iron, soda, &c., after which it is allowed to drain and partially dry. In this state it is smelted in an ordinary reverberatory furnace, the first smelting yielding what is termed "pimple copper." This is again slowly melted in a similar furnace, with a free current of air passing over it for the purpose of oxidising the impurities; it is run out and forms what is termed "blister copper," which is refined into "cake or ingot copper" for the market.

The residue left in the tanks consists of nearly pure peroxide of iron, and is extensively employed for fettling in puddling furnaces and in blast furnaces in the place of hematite ores. The analysis may be roughly stated as under:—

	Tons.
Peroxide of iron	90·00
Silica	6·00
Water, &c.	4·00
	100·00

Most of the Spanish pyrites contain also notable quantities of both silver and lead, and in some cases gold; but until very recently no attempt has been made to separate these metals. M. F. Claudet has, however, patented a process to effect this object, which may be briefly described as under. The ore is treated as above described: the first two or three washings, M. Claudet states, contain the silver, the chloride of silver formed during the calcination being rendered soluble in the large excess of common salt which is used. The solutions to be treated for silver are run into large vats, where a soluble iodide is added to precipitate the silver. The precipitate thus collected is a mixture of iodide of silver and sulphate of lead, with salts of copper. The latter are dissolved out by weak acid, and the remaining precipitate is decomposed by metallic zinc, which reduces the iodide of silver. The zinc iodide which is formed is used to precipitate chloride of silver from other solutions. The precipitate also contains gold.

It will be seen from the above short statement, that all the metals in the ores can be extracted and made commercially useful, and that the only article produced in any quantity, and not hitherto utilised by the process, is sulphate of soda. Experiments upon a large scale have already been carried out by Dr. Merz and Mr. Napier, to recover it from the waste liquors; and it may reasonably be expected that in time it also will be obtained.

But if we leave our own locality and glance for a moment at the production of copper in the United Kingdom generally, we find that the native mines, which have been worked for centuries past, reached their maximum production in 1860, viz., 15,968 tons. As the importations from abroad increased, and the production of copper from burnt pyrites gradually assumed greater importance, the mining of native ores has been proportionately abandoned, and the weight raised in 1869 amounted to only 8291 tons, equal to a reduction of nearly 50 per cent.

The following figures represent the production, importation, exportation, and consumption of copper in 1869:—

	Tons.	Tons.
Copper made from native ores		8291
Viz., from Cornwall and Devon	6794	
,, Wales	346	
,, Isle of Man	30	
,, Ireland	1022	
,, sundry precipitates	99	
	8291	
Copper made from burnt pyrites ..		7600
Viz., from the Tyne	4100	
,, Lancashire and other places	3500	
	7600	
Imports (consisting of ores, regulus, bars, rods, &c., calculated as pure copper	63,097	
	78,988	
Viz., from Chili	42,000	
,, Australia	7500	
,, Cape of Good Hope, Norway, North America, and other places	13,597	
	63,097	
The exports of copper manufactured in England are	42,569	
Do. foreign copper ..	12,309	
	54,878	

The consumption of copper in England was as under:—

	Tons.
1864	20,120
1865	26,514
1866	28,326
1867	20,305
1868	17,400
1869	21,665

Or an average of 22,388 tons annually.

In conclusion, I have to thank the copper manufacturers for the prompt manner in which they have given me all the information required, and Mr. R. Hunt, F.R.S., for his kindness in overlooking my figures, and for valuable information.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from p. 11.)

I STATED that exposure to a red heat was found by Pasteur to act effectually in destroying the vitality of these little particles, and in every case in which he used air which had been subjected to that heat, he found that the air was incapable of sowing any of these organisms in liquids even the most favourable to them. There was, however, still one remarkable exception, which was presented by the experiment of Gay-Lussac, to which I alluded some time ago. He found that when he used a mercury trough, which he selected as giving him the best condition for the purpose, he got these little cells produced from the air which had been calcined. Now, Pasteur found that mercury exposed to the air, as it is in these operations, has adhering to it a number of these little germs, and that when no more than the ordinary precautions are taken for cleansing the mercury, that it has got with it a considerable variety of such little organisms, which,

* The Cantor Lectures. Delivered before the Society of Arts.

if placed in a suitable material, develop themselves and grow quite well. He proved this in various ways. For instance, some of the little bulbs which had been sealed up whilst full of fermentable liquor and steam, and which had been kept for some time in a warm chamber, so as to be certainly free from vital organisms, were opened under mercury, so as to allow the ends of the tubes to be filled with mercury. He then lifted it up, so that nothing came into contact with the liquid but mercury, and passed into them sometimes air which had been passed through a red-hot platinum tube, and sometimes oxygen gas given off from molten chlorate, where certainly there would be nothing of organic life present, and in almost all these cases he found that organisms developed themselves. He attributed this entirely to the mercury, because when that was absent the result was the opposite. In order to prove this point more decisively, he took a liquid which was capable of decomposing, kept it for some time in a quiescent state, and then allowed a drop of mercury, in the state in which he had been using it before, to flow into it, and put the mixture into his warm chamber. He soon found that the mercury had carried in the germs of these organisms, and that they developed themselves quite well in it. Certainly anyone unaccustomed to such accurate precautions could hardly have anticipated such a result as that, and a result which is, I think, most instructive, as showing what extraordinary precautions are needed, in order to prevent the entrance of these excessively small particles into the materials which we are working with. Side by side with this, I must mention another result of Pasteur's for it was, perhaps, hardly less startling, and that was, that when, instead of taking the liquid which I mentioned to you just now, yeast-water and sugar, he took common cow's milk, or at all events, the mixture which is sold by that name, and boiled it, with a view of destroying any organisms that might be in it, and then he sealed up the bulb while still full of steam, so that no air could get into it, and when he kept such sealed-up bulbs for some time in a warm chamber, he found clear evidences of decomposition; he found a turbidity in the substance, a curdling of the nitrogenised materials of the milk; and on taking out some of it, he found it was swarming with little animalculæ; and yet he had boiled the milk for a considerable time, and had closed the vessel whilst the ebullition was still going on, so that no air could have carried the germs into it before it was closed. Still, there were little organisms unmistakably present. He then modified his experiment in this manner: he boiled his milk at a higher temperature. I need hardly tell you how that can be done. You are, of course, aware that the temperatures at which water, or milk, or any liquid boils are different, according to the pressure which the air exerts upon it; that is to say, if you were to boil water here, and then if you were to carry it to the top of St. Paul's and notice the temperature in each case, you would find that at the greater height it would boil at a lower temperature. If, in like manner, you carried it down to the bottom of a deep mine, and boiled it there, you would find the temperature would be higher; the greater the pressure of the superincumbent air, the higher the temperature at which any liquid boils. Pasteur wanted to make his milk boil at a higher temperature, and for that purpose he resorted to a very simple device. He had a long tube attached to the vessel in which his milk was boiling, bent over at the top, and brought down into a glass jar containing mercury to the depth of fifteen inches or more. Of course, under these circumstances, the steam, which was being formed in the vessel, has to force its way up against the pressure of this mercury; the pressure of these fifteen inches of mercury was added to the pressure of air, and a total pressure was obtained, about half as much again as the pressure of the atmosphere amounted to. Of course, the milk had to boil at a higher temperature, corresponding to this higher pressure; and what did he find then? He proceeded, as before, with the experiment, closing the vessel while it was boiling, and not letting any air into it. He then kept it, and he found that no organisms appeared, even on keeping it a very long time; and he was, therefore, led to conclude that the milk must have contained in it some germs which could withstand the temperature at which the milk was boiling at first, but the vitality of which was destroyed by exposure to the higher temperature to which he exposed it in the subsequent experiment. He had reason for that, for other experiments had been made by himself, and by various other philosophers, which proved that many species of organisms can withstand a very high temperature without losing their vitality. In that respect, there are great differences amongst these little organisms which are remarkable and interesting, and will, no doubt, be of value to future investigations. To give you an idea of the great variety presented by them in their power of witshtanding heat, I may mention, that if I were to heat the contents of this carboy, in which the alcoholic fermentation is going on, to 60° C. (100° being boiling-point centigrade), which is rather more than half, the fermentation would be completely arrested, and the yeast cells would be killed. On the other hand, the particles in milk are capable of withstanding 100°. Pasteur connected that fact with the circumstance that milk is alkaline, whilst this liquid is acid, and, as a rule, acid liquids destroy the vitality of these organisms at a lower temperature than alkaline liquids. That is not all. There are in the particles themselves great differences in their power of withstanding heat. Amongst the experiments which are particularly remarkable in that point of view, I ought to mention some with regard to the little spores of mould, and such-like things; for instance, the *Pennicillium glaucum* and some others. M. Pasteur collected some of these; and after taking a little piece of asbestos, or mineral flax, as it is sometimes called, and heating it in a flame, so as to destroy anything adhering to it, he put it carefully into a vessel in which some of this mould was growing, and moved it about, so that a number of particles of the seed of the mould might adhere to it. He then heated the asbestos thus coated with dust to 120° C., a higher temperature than that to which the milk had been exposed; but after putting it into a liquid capable of feeding mould, he found that the mould made its appearance in considerable quantity, so that the germs of that particular organism were not destroyed by 120° of temperature. He even went higher, as far as 125°, and found that that was not enough, but a little over 125° killed them; 130° they cannot stand, so that, according to these observations, the limit appears to be between 125° and 130°.

In all the cases of which I have been speaking, the ferments (because all these organisms are in their nature and functions analogous to the common ferments) were removed from the substances which were employed before the air and such like materials carrying the germs, were brought in contact with them.

With regard to processes for arresting fermentations and decomposition in liquids in which they are taking place, a number of observations have been made which are of considerable practical as well as theoretical importance, in relation to the results which I have been stating. Of course, mere heating, carried to a sufficient intensity, will arrest any process of fermentation or putrefaction which may be going on in a substance, and the applications of that process are, of course, exceedingly numerous and important. The only thing is, that we do not know, and it would be most hazardous to suppose that, in any particular case, we can name beforehand the temperature requisite to destroy a particular organism. If any observer were to say that he has exposed a mixture to 100°, and, therefore, the organism must be destroyed, experience would refute him; if he said he had exposed it to 110°, or even 120°, experience again would refute him; but if he had exposed it to 150°, and asserted that he must have destroyed them, it is quite possible that experience might show that there are organisms which will resist even that temperature. It would have been almost impossible, some time ago, to admit, and we

could not have admitted, that these organisms would have withstood the temperature which they have been found to withstand; and, therefore, what temperature is sufficient to destroy the organism in any case must be found by experiment, and that alone. Amongst other conditions for arresting the process of decomposition or putrefaction, which are in their nature like those of fermentation, I ought to mention the process of drying. All the processes of fermentation which I have been speaking of, and all others which I could tell you of, are accompanied by moisture. Moisture is present, and is essential to them; in fact, these little organisms are exceedingly soft, wet things; moisture constitutes a great part of their substance, and in a dry medium they cannot live, or if the substance were dried, they would be destroyed by it. Applications, therefore, of a mere drying process are amongst the most important and interesting of this class of agencies. Many of them are well known; for instance, the ordinary process for preserving fruit by means of drying it. Germs of putrefaction or decomposition may be present in the fruit; but if you merely take away the greater part of the moisture, you render the substance incapable of decomposing, Among the agents which serve for that purpose, there are some which abstract the water, not in a state of vapour, but in the liquid state; for instance, common salt. If you put a piece of fresh meat in contact with salt, or rub it over with the salt, the salt gradually absorbs the water, and draws the water out of the meat. The action is truly a drying action upon the meat, and it is effectual by a perfectly similar process to that which would go on if you exposed the meat in a dry chamber to a current of warm air. In like manner, of course, it is known to many persons that sugar is used just as salt is, to remove water from substances containing it in any quantity. If you were to rub any fruit or animal substance with a sufficient quantity of dry sugar, you would get the sugar dissolved by the water, which would be removed from the materials; and amongst the observations which are made in common life, there are some which bear, in an interesting and instructive way, upon what I have been saying to you. For instance, I have heard it said that ordinary jam—fruit and sugar, which have been boiled together for some time—keep better if the pots into which it is poured are tied up whilst hot. The observation has been so frequently made that one was inclined to think that there must be some truth in it; and I think if we admit that the paper can act as a strainer in the same way as the cotton wool, you will see at once that it must be as people suppose. Take two cases. Suppose one pot of jam, allowed to cool before it is tied down—little germs will fall upon it from the air, and they will retain their vitality because they fall upon a cool substance; they will be shut in by the paper, and will soon fall to work decomposing the fruit. If you take another pot, perfectly similar, filled with a boiling-hot mixture, immediately cover it over, though, of course, some of the outside air must be shut in, any germs which are floating in it will be scalded, and, in all probability, destroyed, so that no decomposition can take place.

Amongst other materials which serve to arrest fermentation, there are several chemical agents of considerable energy, which are frequently employed for that purpose. Amongst the foremost, I ought to mention creosote, the active material of smoke; and I have no doubt that the antiseptic action which smoke is said to exert upon ourselves—because it is said that smoke is very wholesome, although I do not lean to that view myself—is due to the presence of this creosote or carbolic acid. Everyone is aware that one process for preserving meat, which has long been in use, is to suspend it in a chimney in which the smoke of wood is present. The smoke of wood, like that of coal, contains this substance, or one nearly allied to it, and amongst antiseptic agents it is one of the most energetic. A small quantity of this carbolic acid thrown into that fermenting liquid would completely kill the organisms. In the same way, if I were to introduce a little sulphurous acid into any of these mixtures, I should immediately kill the organisms and arrest the fermentation. Sulphurous acid is now largely used for this purpose, being employed, in combination with lime and water, to saturate the casks in which beer is to be stored, so that the wood being impregnated with it, any germs which might find their way from the atmosphere, and set up a process of decomposition, are arrested and destroyed. Another very powerful antiseptic agent is prussic acid, one of the most powerful of poisons to all animal organisms, and it is particularly powerful in stopping the action of these ferments. Another substance which I think is worthy of consideration, in the same point of view, is a mixture which is, to a great extent, of unknown composition. I refer to the poisonous matter which is given off in tobacco smoke. It must, I think, when present in the air, exerts a very powerful antiseptic action upon these organisms. It has been shown, by the experiments of Professor Tyndall, that in the lower vessel of the lungs there are considerable deposits of the dust which floats about in the air; and we are, of course, exposed in that manner to the action of a number of the seeds of these ferments, and, for aught we know, of diseases, because many malignant diseases are attributed to processes of decomposition analogous to those which we have been considering; and they may be, and, as some persons think, are carried by germs in the air, in the same way as those I have been mentioning. Now, any powerful substance which would kill these germs must, of course, exert a beneficial action, and when persons are exposed to the smoke of tobacco, there is no doubt that some of it enters the lungs with the air which is vitiated, and that some of the smoke must be deposited in the lower passages of the lungs with these little mischievous germs, and must certainly somewhat astonish them.

I have here several little apparatus, all alike in their general arrangement; each consist of two little tables, connected together in such a way that air may be made to pass through both of them in one direction, but not in the other. A tube goes from the top of one into the liquid in the second, and the tube from this second passes on into the air; and these bottles can, by means of an aspirator, be supplied with air which has been strained through cotton wool, and no other air can pass into them. The bottles contain the same mixture which I have been talking about so much, yeast-water and sugar, a liquid which decomposes in almost any way you like, for almost all these germs live in it more or less vigorously. After the liquid was put in, it was kept boiling for a considerable time, so that there is, I trust, in the bottles no living organism whatever; in fact, I have reason to believe that any organisms which may have been there have been destroyed by the high temperature to which they were exposed. I might draw hundreds of cubic feet of air through that apparatus, and it would remain entirely unchanged. Next Monday we will resume this again. We will also examine this particular apparatus, which is exactly the same, with this exception, that after the whole had been filled in the manner I have stated, a little mould was introduced by a separate tube into the first bottle. The apparatus will be taken back to University College, where it will be put into the warm chamber, where the organisms will be developed; and I have no doubt the liquid in the first bottle will be in a state of active decomposition before the day is over. Then next week, we will draw purified air, which, by itself, has no action on the liquid, and see whether it will carry any germs into the second bottle. I have no doubt that, by Monday next, there will be enough mould upon it to enable us to perform the experiment; and I shall then also have the pleasure of telling you of some applications which M. Pasteur has made of his theoretical results to practical purposes such as the preservation of wines and such-like matters.

(To be continued).

CHEMICAL TABLES ACCORDING TO THE THEORIES OF MODERN CHEMISTRY.*

By Professor ALBERT R. LEEDS.

(Continued from vol. xxii., p. 306.)

Table III.

Revised Atomic Weights according to the Ancient Chemistry. H = 1.

Element.	Atomic weight.	Authority.
Aluminum	13·740	Dumas.
Antimony	122·340	Dexter.
Arsenic	75·000	Pelouze.
Barium	68·510	Dumas.
Bismuth	210·340	,,
Boron	11·000	,,
Bromine	79·970	De Marignac.
Cadmium	56·000	C. v. Hauer.
Cæsium	133·000	Johnson and Allen.
Calcium	20·010	Dumas.
Carbon	6·000	,,
Cerium	45·644	C. Wolf.
Chlorine	35·500	Dumas.
Chromium	26·270	Berlin.
Cobalt	29·540	Dumas.
Columbium	49·225	Rammelsberg.
Copper	31·720	Erdmann and Marchand.
Didymium	47·920	De Marignac.
Erbium	56·300	Bahr and Bunsen.
Fluorine	19·000	Louyet.
Glucinum	9·290	Awdejew.
Gold	196·730	Berzelius.
Hydrogen	1·000	Dumas.
Indium	37·813	Winkler.
Iodine	127·000	Dumas.
Iridium	98·560	Berzelius.
Iron	28·000	Erdmann and Marchand.
Lanthanum	46·400	Mosander.
Lead	103·500	Dumas.
Lithium	7·000	Troost.
Magnesium	12·300	Dumas.
Manganese	27·480	,,
Mercury	100·100	Erdmann and Marchand.
Molybdenum	48·000	Dumas.
Nickel	29·510	,,
Nitrogen	14·000	,,
Osmium	99·400	Berzelius.
Oxygen	16·000	Dumas.
Palladium	53·280	Berzelius.
Phosphorus	31·000	Schrötter.
Platinum	98·940	Andrews.
Potassium	39·130	Stas.
Rhodium	52·160	Berzelius.
Rubidium	85·400	Bunsen.
Ruthenium	52·100	Claus.
Selenium	39·620	Berzelius.
Silicon	14·010	Dumas.
Silver	107·950	Stas.
Sodium	23·050	,,
Strontium	43·740	Dumas.
Sulphur	16·000	,,
Tantalum	91·000	Rammelsberg.
Tellurium	64·500	Dumas.
Terbium	37·680	Delafontaine.
Thallium	203·000	Crookes.
Thorium	50·500	Berzelius.
Tin	59·030	Dumas.
Titanium	25·170	Pierre.
Tungsten	92·000	Dumas.
Uranium	59·430	Ebelmen.
Vanadium	51·330	Roscoe.
Yttrium	30·850	Bahr and Bunsen.
Zinc	32·530	Erdmann.
Zirconium	33·600	Berzelius.

(To be continued).

* Communicated by the Editors of the *Journal of the Franklin Institute*. From advance-sheets.

THE ISOMERS OF AMYL.

By O. LOEW.

In the Chemical News, vol. xxii., p. 230, Mr. Herbert McLeod undertakes to prove that the number of the isomers of amyl have been over-estimated in the table given by me. It is not difficult to prove the contrary, and to show that the real cause of his differing opinion is that he did not pay sufficient attention to the relative importance of the atoms in different positions. We have to take into consideration not only the four-atomic value of carbon, but also the relative effectiveness. Let us, for example, compare the formulæ Nos. 4 and 9, which are, according to Mr. Herbert McLeod, identical.

No. 4.

$$\left.\begin{matrix} b\mathrm{H_3C}\gamma \\ b\mathrm{H_3C}\gamma \\ b\mathrm{H_3C}\gamma \end{matrix}\right\} \mathrm{C}\beta \Big\} \mathrm{C}a \qquad \text{or} \qquad b\mathrm{H_3}\equiv\mathrm{C}-\mathrm{C}\beta\begin{matrix}-\mathrm{C}\gamma\equiv\mathrm{H_3}b \\ | \\ \mathrm{C}a=\mathrm{H_2}a \\ |\end{matrix}$$

No. 9.

$$\left.\begin{matrix} b\mathrm{H_3C}\gamma \\ b\mathrm{H_3C}\gamma \end{matrix}\right\} \mathrm{C}\beta \begin{cases} a\mathrm{H_3} \\ a\mathrm{H_2} \end{cases} \mathrm{C}a$$

It will be seen at the first glance that the carbon atom a has a higher relative degree than the atom β, and this again another than the more subordinate carbon atoms, γ. When we now compare No. 4 with No. 9, we observe that in No. 4 there are three carbon atoms of the lowest order, γ, and one of the highest, a; while in No. 9 there are two of the lowest, and two of the highest order. In a similar manner, we have hydrogen atoms of a higher degree, a, and of a lower, b. Thus we have in No. 4 only two "typic" hydrogen atoms, while in No. 9 we have five of them.

The hydride of No. 4 has been prepared recently by M. Lwow, in St. Petersburg*; he calls it "tetra-methyl-formen." M. Lwow obtained this body by the action of iodide of "trimethyl-carbinol" upon zinc-methyl; he will probably obtain an isomeric body on treating chloride of aceton with zinc-methyl.

Kolbe, in his theory, pays special attention to the different intensities of the atoms in different positions, and, based upon this principle, predicted the existence of many combinations—as, for example, the isomeric cyanides, isomeric ureas, secondary, tertiary, and dicarbolic alcohols; the mere atomicity is not always sufficient. The normal alcohol, corresponding to a dicarbol, will be a bi-acid alcohol, different from so-called glycols.

Alcohol of No. 4.

$$\left.\begin{matrix} \mathrm{H_3C} \\ \mathrm{H_3C} \\ \mathrm{H_3C} \end{matrix}\right\} \mathrm{C} \Big\} \mathrm{C(OH)} \qquad \mathrm{H_2}$$

Normal alcohol of No. 9.

$$\left.\begin{matrix} \mathrm{H_3C} \\ \mathrm{H_3C} \end{matrix}\right\} \mathrm{C} \begin{cases} \mathrm{H_2} \\ \mathrm{H_2} \end{cases} \mathrm{C(OH)}$$

As regards the aromatic series, an alcohol of a dicarbol has already been prepared—viz., the phthalic alcohol, by Kolbe and Wischin.† The following formulæ will show the structure of this interesting combination:—

Kolbe's formula of benzol.

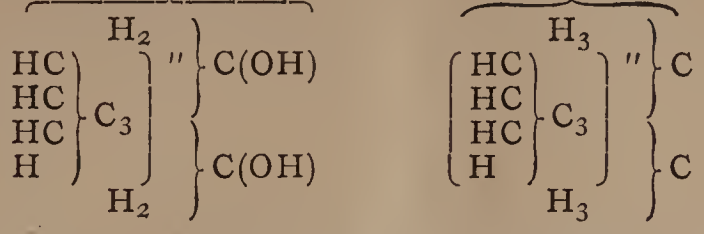
$$\left.\begin{matrix} \mathrm{HC} \\ \mathrm{HC} \\ \mathrm{HC} \\ \mathrm{H_3} \end{matrix}\right\} \mathrm{C_3}$$

Phthalic acid.

$$\left\{\left.\begin{matrix} \mathrm{HC} \\ \mathrm{HC} \\ \mathrm{HC} \\ \mathrm{H} \end{matrix}\right\} \mathrm{C_3} \right\}'' \begin{matrix} \mathrm{COOH} \\ \mathrm{COOH} \end{matrix}$$

* *Zeitschrift für Chemie*, November, 1870.
† *Zeitschrift für Chemie*, 1867.

Phthalic alcohol. Phthalic dicarbol.



I have not the slightest doubt that the existence of such dicarbolic alcohols will be proved for the fatty series also.

City College, New York, December, 1870.

ON THE STEARIC ACID INDUSTRY.*

By J. LAWRENCE SMITH.

Sulphuric Acid Saponification.

This method was discovered by Chevreul in his original researches on fatty bodies, and in the patent taken out by him in conjunction with Gay-Lussac in 1825, this method is specified, but certain portions of the fats were so altered as to discolour the product to such an extent as to render it of no use in practice. The acids, however, thus formed could be distilled more or less perfectly, as first shown by Chevreul, and subsequently practically executed by Dubrunfaut. But the first idea of combining the two operations, viz., sulphuric acid saponification followed by distillation, is due to Coley Jones and Wilson, of England, subsequently perfected by Gwinne and Jones.

The process as it is now carried out may be summed up as follows :—The fat is placed in large vessels that may be of wood or masonry lined with lead, and from six to fifteen per cent of concentrated sulphuric acid is added: the mixture is then heated by a steam coil to near the temperature of boiling water, and maintained at that temperature eighteen or twenty hours. Some, however, operate at a higher temperature, and consume less time in the reaction, with as much as thirty per cent of sulphuric acid and agitation, decomposing batches of two hundred pounds in four minutes. The fat is decomposed with an alteration of part of the glycerine and part of the fat, sulphurous acid and carbonic acid being evolved. The black mass resulting from the action of the sulphuric acid is now thoroughly washed by boiling water, until all the fatty acids are freed completely from sulphuric acid. The fatty acids are now introduced into large cast-iron stills, capable of holding over one ton of these acids. The stills are heated from below, so as to bring the contents to the temperature of 260° C., when a jet of superheated steam of 350° to 380° C. is made to traverse the charge, and in about twelve hours the matter is distilled over, leaving behind a pitchy substance, which may be used in the manufacture of gas, or for coating roofs and other purposes in the arts. The fatty acids when distilled may be cooled in pans, and submitted to the cold and hot pressure ; for some purposes it is submitted only to the cold pressure, and in England much is used without any pressure at all, when palm oil has been the fat acted upon.

This method contrasted with the lime saponification has its advantages and disadvantages ; among its advantages are the facility of using common and refuse fats, and in giving a larger yield of candle stuff; among its disadvantages are the inferior grade of candle stock produced, and the actual loss of about ten per cent of the fat by decomposition into gaseous and pitchy matters.

The comparative yield of lime and sulphuric acid saponifications in candle stock from tallow and palm oil is forty-seven per cent for the lime, and sixty-two per cent for the sulphuric acid.

Saponification by Water and Distillation.

The decomposition of fats by the increase of temperature and the subsequent distillation of the fat acids was originally observed by Chevreul, and different methods were devised by Moses Poole and by Gay-Lussac, as early as 1828, to obtain practical results from it, but so much acroleine was formed that it was abandoned as impracticable. Dubrunfaut revived the method, and was more successful ; but it was reserved for George Wilson, of the Price Candle Works, near London, to solve the problem in 1852, at least so far as palm oil was concerned, and in 1855 he had three stills in operation, each of a capacity of about 1600 gallons, from which, by one and the same operation, the fat acids and glycerine were distilled over uncombined.

To accomplish this twofold result, viz., to accomplish the watery saponification of the neutral fats, and the distillation of the fatty acids and the glycerine that result from the saponification, it is necessary to heat the fat in a still to a temperature between 290° and 315° C., and to pass through the heated fat a current of superheated steam, properly subdivided, having a temperature of 315° C. For proper success in the operation, the distillation must be conducted between 290° and 315° C., as otherwise the distillation is very slow, or acroleine is produced. This distillation goes on slower than when the fatty acids already formed are acted upon, the latter being accomplished in one-half or one-third the time. The glycerine distilled is of a good quality, and admits of further purification, thus forming the widely known and much appreciated Price's glycerine. It is well to state here that the employment of other than palm oil in this process gives unsatisfactory results, and consequently its use is limited.

Saponification by Water Under High Pressure.

The conception of this method, in common with all the methods of saponification of fatty bodies, is to be referred back to the author of the discovery of the true nature of fats, M. Chevreul, for in his original researches he pointed out the perfect analogy between the fats and the compound ethers, the latter class of bodies being decomposed with their two constituents in the presence of water heated in close vessels under pressure ; a reasonable deduction from which was that fats would undergo an analogous decomposition. This, however, was not undertaken at the time, but, by an accident, about the time of Chevreul's researches, it was first observed to take place by Faraday when his attention was drawn to some changes in oils used by Perkins in his curious steam-engine that employed very hot water. Faraday records the facts as observed by him in the *Quarterly Journal of Science, Literature, and the Arts*, published in London in 1823, vol. xvi., page 172, as follows :—

"*Change of Fats in Perkins's Engine by Water, Heat, and Pressure.*—Mr. Perkins uses in his steam cylinder a mixture of about equal parts of Russia tallow and olive oil to lubricate the piston and diminish friction. This mixture is consequently exposed to the action of steam at considerable pressure and temperature ; being carried in by the steam it is found in the water, giving rise to peculiar appearances.

"The original mixture is solid at common temperature, but fuses at about 85° F. When boiled in alcohol a small portion dissolves. The water as it issues from the end of the ejection pipe into the tub placed to receive it and from which it is pumped up again into the generator, appears white and translucent, after having been used, it sometimes very much resembles thin milk. A scum is found floating on it, which, when collected together, forms a solid mass, but when it has been long exposed to the action of steam, and at a high temperature, it has very nearly the consistency of wax. It is always black and dirty.

"A portion of this substance was digested in hot alcohol and the clear solution set aside ; floculi separated in abundance from it in cooling, which, when dried and collected and fused, gave a greyish substance, contracting and cracking as it cooled, with the lustre and appearance of wax, but rather more brittle. It does not melt in boil-

* From "The Progress and Condition of Several Departments of Industrial Chemistry," J. Lawrence Smith.

ing water, but at a higher temperature melts, and ultimately burns like wax. It is rather lighter than water; it dissolves readily in alkalies, more readily, I think, than fat, and in this respect resembles Chevreul's acids of fats as well as in its solubility in alcohol. The alkaline solution is turbid. It is not soluble in ether, or very slightly so; when burnt, it leaves an ash consisting principally of carbonate of lime. The cold alcoholic solution on evaporation left a substance similar in many respects, but much softer, even fluid. It burnt in the same manner, leaving a slight ash of carbonate of lime."

The above is sufficient to show that the practical decomposition of fats by water heated under pressure was accomplished as far back as 1823, as any chemist will conclude from Faraday's observation made at a time when we were much less endowed with facilities for research than we are now. No attempt was then made to draw any practical results from these observations, and we find no further notice taken of the subject until early in the year 1854, by R. A. Tilghmann, of Philadelphia, when patents were taken out by him for decomposing fats mixed with water and superheated in vessels of a certain description, the sum and substance of which method is embraced in the following portion of the specifications.

Tilghmann's Process.

He mixes the fat on which he operates with one-third or one-half its volume of water, and the mixture is placed in some convenient vessel in which it can be submitted to heat equal to that of melted lead, and kept at that temperature until the operation is complete. The vessel is to be closed so that the requisite pressure can be applied to prevent the water from being converted into steam. The result of the action is the splitting of the fat into fatty acids and glycerine, which can be readily separated, mechanically, and both prepared for market. The acids are cooled in the ordinary way and submitted to pressure to procure candle stuff.

The method of Tilghmann, as originally patented, was never introduced into practice; since then, with change in the manner of operating and in the nature of the boilers, it has been successfully conducted in many factories.

In the latter part of the same year that Tilghmann's process was patented, M. Melsens, of Belgium, took out a patent very analogous, using fats mixed with water in the proportion of twenty to one hundred per cent of the latter; the water might be acidulated with from one to ten per cent of sulphuric acid, or the addition of salt would suffice; the whole was heated from 180° to 200° C. for several hours. The success of Melsen's process was immediate, and it was put into operation on a large scale in Antwerp in vessels holding one ton of tallow, to which was added fifty per cent of water, and in six hours the decomposition was complete at a temperature of 180° C. (ten atmospheres). The fatty acids thus made were of a very satisfactory quality, quite as much so as those obtained by other methods of saponification.

PROCEEDINGS OF SOCIETIES.

ROYAL IRISH ACADEMY.

A GENERAL meeting of the members of the Royal Irish Academy was held on Monday evening, January 9th, 1871, the Rev. J. H. Jellet, President, in the chair. The attendance was not large.

New Members.—The following gentlemen were, after a ballot, elected members of the Academy:— Very Rev. Ulick J. Bourke, Tuam; George Woods Maunsell, Esq.; John Symons, Esq., Hull; and Ramsay H. Traquair, Esq., M.D.

Mr. CHARLES R. C. TICHBORNE, F.C.S., read two short papers, entitled "*Laboratory Notes.*" The first was on "*The Production of Acetic Acid by the Destructive Distillation of Resin.*" He said he found that, when resin was submitted to distillation, among other products was a strongly acid solution. The silver salt of this acid was formed, and on analysis it proved to be acetic. He remarked that it was rather surprising to see acetic acid produced in appreciable quantities from a substance so comparatively pure in oxygen. The amount of oxygen in colophony was 10·6 per cent, whilst in an acid-yielding substance, such as woody fibre, it was 49·4 per cent.

The subject of the second paper was "*The Formation of Ozone by Resin Oils.*" He said when light resin oils were submitted to the combined action of atmospheric oxygen and light, ozone was produced in abundance. ("Such oils, when poured upon a solution of iodide of potassium, instantly produce blue iodide of starch.") At the same time the boiling-point of the oil was raised. Ozone was probably the prime mover in the production of colophonic hydrate, a substance discovered about two years ago by himself. All the terebine oils on exposure to light produced this effect. That obtained from pine seeds was said to possess this property in a most marked manner; oils of lemon and bergamot in a slight degree. The resin oils possessed this property more decidedly than ordinary oil of turpentine. Mr. Tichborne, by experiments, showed the action of those ozonified oils upon iodide of potassium.

The papers were referred to the council for publication.

Mr. G. J. STONEY, F.R.S., next read an exhaustive paper "*On the Cause of Interrupted Spectra of Gases.*" This paper was also ordered to be printed.

ROYAL GEOLOGICAL SOCIETY OF IRELAND.

A GENERAL meeting of this Society was held on Wednesday, in the Museum Building, Trinity College. The chair was taken by the Rev. Maxwell Close, and the following papers were read:—

(1.) EDWARD HULL, Esq., F.G.S.—"*On the Geological Age of the Ballycastle Coal-field, and its Relations to the Carboniferous Rocks of the West of Scotland.*"

(2.) JOHN LEECH, Esq.—"*On the Moving Bog of Castlerea, Co. Roscommon.*"

NOTICES OF BOOKS.

Handy Book of Small Farm Management. By THOMAS BALDWIN, Superintendent of the Agricultural Department of National Education (Ireland). Dublin: Browne and Nolan.

THIS volume, dedicated to his Excellency the Viceroy of Ireland, Earl Spencer, K.G., &c., is a book which, although it is evidently written with special reference to the Emerald Isle, contains in 215 pages an exceedingly large amount of well-digested matter, interesting to all farmers, too many of whom are not so conversant with the subjects herein treated of as they ought to be. The main divisions of this work are:—Manures; Farm Implements; Crops; Live Stock; Works which Permanently Improve the Value of Land; the Management of a Five-and-a-half Acre Farm at Glasnevin. We have perused this little book with great pleasure; the author is a thorough master of his subject, and he is very successful in writing what he has to say in plain, clear, and intelligible language. We hope that all who take an interest in that unfortunate, although beautiful, portion of the British Dominions—Ireland—will contribute to disseminate the contents of this little book among the many small farmers of that island. We also recommend the perusal and study of this small work

to English farmers, who will find, even in the wood-engravings alone, many a useful hint worthy to be impressed on their mind and retained in their memory.

Handbook of the Telegraph; being a Manual of Telegraphy, Telegraph Clerk's Remembrancer, and Guide to Candidates for Employment in the Telegraph Service. By R. Bond. Third edition, revised and enlarged, with illustrations. London: Lockwood and Co., 1870.

This neat little volume, No. 138 of Weale's Rudimentary Series, contains the following chapters:—Introduction; Form of Application, Security, Secrecy, &c.; Officials; Scale of Charges; Delivery of Telegrams; the Double Needle Instrument; the Single Needle Instrument; the Morse Printing Instrument; Grouping of Letters of Different Alphabets; Hughes's Printing Instrument; the Bell Instrument; Wheatstone's Automatic System; Wheatstone's A B C Instrument; the Pneumatic Tube; Postal Telegraph Codes; Miscellaneous Regulations; Inland Telegrams; Forms; Miscellaneous Regulations; Abstracts, Abstract Books, Accounts, Returns; Railway Companies' Messages and Forms; Railway Train Telegraph; Offences; Useful Hints; Telegraph Statistics; Index. From this list our readers will perceive that the volume has been prepared especially with the view of being a guide to those who desire to enter the telegraph service, and for this purpose the work is very well adapted, being clearly, yet concisely written, and well illustrated by woodcuts. The fact that this little volume has already reached a third edition is the best evidence of its utility.

Abstracts of English and Colonial Patent Specifications Relating to the Preservation of Food, &c. Compiled from original documents, or their printed copies, lodged in the Patent Office attached to the Registrar-General's Department, Melbourne, by William Henry Archer, Registrar-General of Victoria. Melbourne: Printed and published by John Ferres, Government Printer; also published by H. T. Dwight, 232, Bourke Street East, 1870.

This pamphlet contains, compressed into the space of sixty-six pages, an unusually large amount of highly valuable matter. After a brief preface, the author, in an introductory chapter, sets forth the scientific principles of fermentation and putrefaction, and next treats briefly on the following subjects, under the headings of each of which respectively the abstracts of the patents have been classified, viz:—Reduction of Temperature; Deprivation of Moisture; Salting; Exclusion of Air; Antiseptic Agents. The contents of this pamphlet are too numerous to admit of any particular abstraction or quotation. The work is illustrated by a series of exceedingly well executed photo-lithographic plates, and is in this respect a decided improvement over several similar publications issued by our own Patent Office. All who are interested in the preservation of food and the numerous means devised to gain that end, will peruse this pamphlet with pleasure.

CORRESPONDENCE.

FUSIBILITY OF PLATINUM BY THE BLOWPIPE.

To the Editor of the Chemical News.

Sir,—Your issue of the 3rd ult. (Chemical News, vol. xxii., p. 268) contains a communication from Dr. W. Skey, of New Zealand, which opens with the following sentence:—

"The metal platinum has hitherto been supposed to be infusible, except at a temperature that is so high as to be incapable of being produced by the common blowpipe; at least, I have carefully searched for any statements to the contrary, without success."

If Dr. Skey will look in Plattner's "Probirkunst," p. 16 of last edition (p. 14 of 3rd edition), he will find the following statement:—

"When it is desired to test whether it is possible to produce a sufficiently strong oxidising flame, it is only required to try to fuse the end of a platinum wire of a thickness of 0·1 m.m. to a globule. The wire is bent for this purpose at a right angle, and the shorter bend is so held in the outer flame that the axis of the wire exactly coincides with the axis of the blowpipe-flame, care being at the same time taken that neither wire nor flame vibrate. With strong and really good flame a globule of molten platinum is very soon formed, and this globule will be the larger according to the greater strength of the flame."

A similar method of showing the fusibility of platinum by the blowpipe is given by Dr. H. O. Lenz, in his "Löthrohrschule" (Gotha, 1848); and Bruno Kerl, in his handy little "Lietfaden," refers also to the fusibility of fine platinum wire by the blowpipe-flame. Other published statements of this well-known fact might likewise be quoted.—I am, &c.,

E. J. Chapman.

Toronto, Canada,
December 26th, 1870.

ANTISEPTICS AND DISINFECTANTS.

To the Editor of the Chemical News.

Sir,—My friend Professor Gamgee, in a letter published in the last number of your journal (Chemical News, vol. xxiii., p. 20), "protests against Dr. Crace Calvert's system of experimenting on the comparative energy of antiseptics," and especially as regards the solution of chloride of aluminium. I do not write now to defend Dr. C. Calvert, but on my own account, to protest against Professor Gamgee's assertion as to carbolic acid, and to recommend caution in accepting "chloralum" as a *disinfectant*.

(1.) He says, "Two years experience with carbolic acid has proved that as a sick room disinfectant, and even in open alleys and stables, people refuse to use it," inferentially on account of its odour. He is in error here. I commenced the extensive use of carbolic acid for the purposes of "disinfection" at the time of the cattle plague, in 1865; I used it as my sole disinfectant very largely, and for all purposes—*i.e.*, for closets, drains, rooms, bedding, clothing, &c., during the cholera epidemic of 1866. I have used it since then constantly and habitually for similar purposes where fever, scarlet fever, and small-pox have prevailed, and I can assert positively that, when used thoroughly and every detail is attended to as it should be, it is a most efficient agent for destroying contagia. Moreover, my experience is that people do not refuse to use it except when the crude acid employed smells of sulphide of ammonium. That supplied by some manufacturers is very objectionable on this account, and I have refused to purchase such acid for the public service here, because people would certainly have rebelled. McDougall's acid is quite free from objection on this score; we use it for night-stools and similar utensils, and for floors in the sick room, and for evaporation there constantly. During the past dry summer I used it in the water-carts upon the roadway in our narrow streets, courts, and alleys, with positive satisfaction to the inhabitants; they said it "sweetened the air"—and it did. In empty rooms I prefer to fumigate with sulphurous acid.

(2.) I object to the inference that, because "chloralum" is an antiseptic, it is necessarily a disinfectant; and I protest against Professor Gamgee assuming its disinfecting power from its chemical properties as an antiseptic. The term "disinfection" has of late acquired greater precision of signification than it had a few years

ago. It means the destruction of the vitality—power of growth and reproduction—of those minute particles of matter which, given off from the sick, are capable of producing a like disease in the healthy. To prove anything to be a disinfectant, one or both of the following kinds of evidence must be forthcoming:—(1). It should be shown by *experiment* conducted by some such accomplished microscopist as Dr. Beale or Dr. Sanderson, that the substance in question, used in the strength at which it is proposed to use it as a disinfectant, has the power of destroying the vital manifestations of those minute amœbiform particles of matter which constitute the simplest form of living things. I can scarcely imagine a better service that could be rendered just now to preventive medicine than the publication of some reliable observations of this kind in respect of the reputed disinfectants. They would make a grand clearance of our foggy notions about disinfectants. (2). It should be shown on a large scale, and by repeated and prolonged observation, that the use of the reputed disinfectant actually has resulted in the arrest of the spread of contagious disease. Such observations are best made first upon the contagious diseases of animals. Positive inferences drawn from such observation should be capable of resisting fair logical criticism. I have the greatest personal respect for Professor Gamgee, and for the admirable work he has done—no one more; and I do not believe he would claim "disinfecting" powers for chloralum unless he was himself satisfied that it possessed them; but the proof which may be convincing to him may nevertheless be insufficient to establish his claim in a way satisfactory to others. He will, I am sure, pardon me for saying that, in my view of the matter, he is not warranted in propounding chloralum as a "disinfectant," and still less in recommending its use as such, without, at the same time, adducing the evidence on which his opinion and recommendation are grounded. The *onus probandi* lies with him: he cannot make the assertion, and then leave the profession to show that he is wrong, although there are plenty of bad precedents for such a course.

"Chloralum" may turn out to be the best disinfectant known; his discovery may possibly be one of the highest practical value, but he cannot be permitted to anticipate observation and experiment. He has as yet advanced nothing to satisfy me, or any one, that chloralum used in any way is capable of destroying the peculiar manifestations of a morbid contagion. That it is capable of checking the throat lesions in diphtheria and scarlatina is no more than has been said by the highest authorities of ordinary alum. That medical men should have taken it up with extraordinary rapidity goes for very little, since nothing can be more wild than the manner in which medical men often adopt this or that favourite or reputed disinfectant; and, moreover, Professor Gamgee himself tells us that one great secret of its success has been its freedom from odour. May it not be possible to retort, in the words he uses when speaking of Condy's fluid, which he says "has been supposed to act like carbolic acid, and to disinfect, physicians prescribing it gladly in the erroneous belief that it added to its deodorising properties that of destroying disease germs. Its success has been due to its freedom from odour."

One word more, and I have done. Professor Gamgee has asked me to use the chloralum, and has kindly supplied me with a quantity for this purpose. Small-pox and scarlet fever are now raging in my district, and thus I have abundant opportunities of doing so. Why, then, have I not tried it? This is a fair question to ask, and I will anticipate it. My reply is that I dare not assume the responsibility of its trial until *primâ facie* proof at least is afforded me that by using it I shall be using that which is capable of "destroying disease germs." I must have something which I can rely upon. I can rely upon the disinfectant agencies I employ now, the reliance depending upon empirical observation; I cannot rely upon chloralum, for neither experimental nor empirical observation has been produced in its support.—I am, &c.,

Edward Ballard, M.D.,
Medical Officer of Health for Islington.

7, Compton Terrace, N.,
January 13th, 1871.

MISCELLANEOUS.

Royal Institution of Great Britain.—We have received a corrected list of the arrangements for the Friday Evening Meetings before Easter, from which we learn that the subject of the discourse by Dr. Odling on Friday, January 27th, will be "Recent Improvements in the Production of Chlorine." The discourse on Friday, February 24th, will be by W. Mattieu Williams "On Rumford's Scientific Discoveries," and Dr. Carpenter's discourse on "The Latest Scientific Researches in the Mediterranean and Straits of Gibraltar," will be delivered on March 10th, instead of February 24th, as announced in our last week's issue.

Metropolitan Gas Supply.—Dr. Letheby, the Chief Gas Examiner appointed by the Board of Trade, has recently submitted to the Corporation and the Metropolitan Board of Works a report of the quality of the gas supplied to London by several of the gas companies during the last three months, and the following are the tabular results:—

1*st. With regard to Illuminating Power*—

Illuminating Power in Standard Sperm Candles.

		Maximum.	Minimum.	Average.
Common gas.	City of London Gas Company	18·4	16·2	17·20
	Chartered Gas Co. — Leadenhall St.	19·2	16·5	17·57
	Chartered Gas Co. — Gray's Inn Road	17·5	15·5	16·50
	Chartered Gas Co. — Arundell Street	18·7	16·4	17·37
	Great Central Gas Company	19·3	16·6	18·02
	Imperial Gas Co. — Camden Street	17·1	14·1	15·49
	Imperial Gas Co. — Oakley Square	17·8	15·2	16·50
	South Metropolitan Gas Co.	18·5	14·2	16·14
Cannel gas.	City of London Gas Company	30·8	24·3	26·96
	Chartered Gas Company	26·6	23·1	25·55

From which it appears that the average illuminating power of the common gas supplied by the several companies has ranged from 15·49 candles, in the case of the Imperial gas, at the Camden Street testing place, to 18·02 in that of the Great Central gas; and the Cannel gas has been from 25·5 to 26·9 candles.

2*nd. With respect to Purity*.—The gas of all the companies has been constantly free from sulphuretted hydrogen, but the amount of sulphur in other form has fluctuated to a large extent, as will be seen from the following table:—

Grains of Sulphur per 100 *cubic feet.*

		Maximum.	Minimum.	Average.
Common gas.	City of London Gas Company	30·3	17·8	23·54
	Chartered Gas Co. — Leadenhall St.	34·8	19·5	27·25
	Chartered Gas Co. — Gray's Inn Road	36·9	21·9	33·17
	Chartered Gas Co. — Arundell Street	33·8	22·7	28·02
	Great Central Gas Company	31·1	11·1	19·78
	Imperial Gas Co. — Camden Street	31·8	23·4	27·91
	Imperial Gas Co. — Oakley Square	38·4	27·0	32·37
	South Metropolitan Gas Co.	39·3	27·6	32·31
	City of London Gas Company	22·4	4·8	12·27
	Chartered Gas Company	27·9	19·7	24·39

So that, in the case of the common gas, the average proportion of sulphur has been from 19·78 grains per 100 cubic feet, in the Great Central gas, to 33·17 grains in that of the Chartered gas at Arundell Street; and the Cannel gas has ranged from 12·27 grs. per 100 cubic feet of the City companies' gas to 24·39 in the Chartered.

Ammonia has rarely been present in excessive quantity; but it exceeded 5 grs. per 100 cubic feet on five occasions in the Imperial Company's gas, at the testing place at Oakley Square, Chelsea, and on one occasion in the Chartered gas at Arundell Street; both of these were subjects of appeal to Dr. Letheby, who decided, after full enquiry into the matter, that the excess arose from accidental circumstances.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Polytechnisches Journal von Dingler, first number for December, 1870.

This number contains the following original papers relating to chemistry and allied sciences:—

Determination of the Value of Various Kinds of Kaolin by means of a Pyrometrical Method.—Dr. C. Bischof.—The author describes, in this exhaustive paper, a series of experiments made with porcelain clay obtained from different localities, the standard sample being the levigated porcelain clay from Zettlitz, near Carlsbad (Bohemia), which consists, in 100 parts, of—Alumina, 38·54; chemically-combined silica, 40·53; silica as sand, 5·15; magnesia, 0·38; lime, 0·08; peroxide of iron, 0·90; potassa, 0·66; loss by ignition, 13·0. This sample, pressed into the shape of small cylinders, was first heated to the temperature of the melting-point of platinum by itself, and the effect of this ignition carefully examined, and the experiment repeated with the addition of a flux from 1-10th to 2-10ths of the weight of the kaolin. In this manner have been tested—The kaolin from Halle; kaolin from Aue; kaolin from Seilitz, near Meiszen; kaolin from Sornzig; kaolin from St. Yrieix; kaolin des Colettes près Calizolle (Algeria); kaolin from Brittany; kaolin from Pilsen; kaolin from St. Austell.

New Method for the Quantitative Estimation of Copper by Volumetry.—F. Weil.—Reserved for full translation.

Researchcs on the Part which Saline Substances and Non-Crystallisable Sugar Play in the Formation of Molasses.—E. Feltz.—The author, manager of an extensive beet-root sugar manufactory, relates a series of experiments made with the view of elucidating the mode of action of non-crystallisable sugar upon the formation of molasses—that is to say, the conversion, by the presence of the substances alluded to, of crystallisable into non-crystallisable sugar.

Use of Centrifugal Machines for the Purpose of Obtaining Raw Sugar.—Dr. O. Tech.

Charcoal as a Disinfectant and Antidote.—Drs. H. Eulenberg and H. Vohl.—This exhaustive memoir contains the account of a series of experiments made with the view of investigating the disinfectant, deodorising, and antidote properties of charcoal, bone-black, and similar materials.

Compressed Leather.—Dr. Dingler.—The author states that offal of leather, cuttings, and scraps, are first cleansed from dirt and dust, then soaked in water containing 1 per cent of sulphuric acid, until the material becomes soft and plastic, next compressed into the shape of blocks, dried by steam, and lastly rolled out in mills. In order to soften the mass, 1 lb. of glycerine is added to 100 lbs. of material. The leather thus again obtained is applicable for the inner soles of boots, &c.

Annalen der Physik und Chemie, von Poggendorff, No. 11, 1870.

This number contains the following original memoirs and papers:—

New Optical Method of Determining the Vibrations of Columns of Air which Emit Sound.—Drs. Tœpler and Boltzmann.—This lengthy essay, illustrated by a series of lithographic plates, contains the account of experiments belonging to the subject of acoustics.

Temperature and Physical Constitution of the Sun.—F. Zöllner.—This essay, illustrated by a series of very beautifully executed chromo-lithographs, is, as regards its contents, entirely of an algebraical character. The author deduces, however, from his investigations the following condensed results:—(1). The absence of the lines of some element in the spectrum of a self-luminous body (star) does not prove the absence of this substance. (2). The layer in which the inversion of the spectrum takes place differs for every body, and that layer is placed the nearer to the centre of the star according to the greater vapour density and emissive power of the substance which yields the spectrum. (3). This layer is, *ceteris paribus*, nearer to the centre of different stars according as the intensity of the gravitation thereof is greater. (4). The distances of the layers of conversion of the simple bodies (emitting spectra) from each other, as well as from the centre of the stars, increase with an increase of temperature. (5). The spectra of different stars are, *ceteris paribus*, the richer in spectrum lines according to the lower temperature and the greater mass of the stars. (6). The great difference observed in the intensity of the dark lines of the solar spectrum and that of other fixed stars does not simply depend upon the difference of the power of absorption, but also upon the difference of depth wherein the inversion of the respective spectra takes place.

Transversal Vibrations of Sound-Emitting Liquids and Elastic Fluids.—Dr. L. Matthiessen.—A mathematical essay.

Attraction Exercised by a Magnetising Spiral upon a Movable Iron Cylinder contained Inside the Spiral.—Dr. A. von Waltenhofen.—A memoir illustrated by a series of diagrams and engravings absolutely required for it to be properly understood.

On Disaggregation, and on the True Caloric Capacity of Bodies.—Dr. E. Budde.

Observations on Amorphous Sulphur.—R. Weber.—This essay contains the account of a series of experiments made with sulphur obtained by precipitation (by means of acids) from hyposulphites, alkaline sulphurets, and the decomposition of sulphuretted hydrogen. Sulphur obtained from hyposulphite of soda by the addition of some hydrochloric acid to the solution of that salt, exhibits the appearance of an oily fluid, which remains liquid for a considerable time after having been washed by carefully-conducted decantation, and resembles, as regards colour and consistency, the yolk of eggs. The sp. gr. of this sulphur varies from 1·92 to 1·927; it is amorphous, but becomes, when completely solidified, crystalline, a phenomenon which is greatly accelerated by heat. The author found that the liquid sulphur contains small quantities of persulphide of hydrogen, but the origin of that substance could not be traced. The assertion often made, that crystalline sulphur by contact with acids is converted into amorphous sulphur, is not found to be correct when experiments were purposely instituted to test this.

New Locality where Meneghinite is Found.—A. Frenzel.—The author describes a locality in Germany where the mineral alluded to has been found by him. The mineral, on being examined, was found to possess a sp. gr. of 6·367. The chief constituents of this substance are lead, antimony, and sulphur; in 100 parts—Lead, 63·89; antimony, 18·82; sulphur, 17·29.

Galvanic Caloric Action (Wärmewirgung) at the Limit of the Surface of Electrolytes.—C. Schultz-Sellack.

Two Optical Methods of Observation.—C. Christiansen.—Algebraico-physical papers.

Observations on Kohlrausch's Experiments for the Determination of the Capacity of Heat of Gases.—L. Boltzmann.

A Pseudoscopic and Optometric Figure.—H. Emsmann.

Co-Efficient of Refraction of an Alcoholic Solution of Fuchsine.—C. Christiansen.—A series of tables.

Journal für Praktische Chemie, No. 18, 1870.

This number contains the following original papers:—

Presence of Amygdaline and a New Asparagine-Like Substance in Vetch (Vicia sativa).—H. Ritthausen and Dr. U. Kreusler.—The authors, while experimenting with vetch obtained from Attica (Greece), found, on treating the coarse powder of this seed with water, that it gave off the smell of hydrocyanic acid, which, on nearer investigation, was found to be due to an amorphous modification of amygdaline. As regards the presence of an asparagine-like substance, the authors found a crystalline body, which, on being submitted to elementary organic analysis, gave results leading to the formula, $C_8H_{16}N_8O_6$; this substance is difficultly soluble in water, more readily so in boiling dilute alcohol, and almost insoluble in boiling alcohol at 85 per cent.

Acids contained in the Seeds of Yellow Lupines (Lupinus Luteus).—H. Ritthausen.—This memoir contains an exhaustive account of the qualitative and quantitative methods of analysis employed by the author to detect and determine, in the seeds of the plant just named, malic and oxalic acids, present in the state of combination with bases.

Some Inorganic Periodides.—Dr. S. M. Jörgensen.—The following periodides are described:—Potassium-triodide: The aqueous solution of this salt is discoloured when shaken up with some mercury, the result being the formation of some proto-iodide of mercury; the alcoholic solution of the potassium-triodide is also decolourised when so treated, but no iodide of mercury is eliminated. Cupro-tetrammonium-mercuro-iodide, $4NH_3,CuI_2,2HgI_2$: In 100 parts—Ammonium, 5·26; copper, 4·91; mercury, 30·92; iodine, 58·91. Cupro-tetrammonium tetra-iodide, $4NH_3,CuI_2,I_2$. Cupro-tetrammonium mercuro-iodide, $4NH_3,CuI_2,HgI_2$. Cupro-tetrammonium hexaiodide, $4NH_3,CuI_2,I_4$. Mercuro hexaiodide.

Composition of Plumbostibe and of Embrithite.—A. Frenzel.—After referring to some older analysis of these minerals, the author states that, on making accurate estimations of the constituents, he found for plumbostibe, in 100 parts:—Lead, 60·59; antimony, 21·53; sulphur, 17·88. Sp. gr. of the mineral, 6·12. The formula of embrithite is exactly the same as that just mentioned, but the mineral was

found to contain, in 100 parts:—Lead, 59·30; copper, 0·80; antimony, 21·47; sulphur, 18·08;—total, 99·65.

Quantitative Estimation of Phosphorus present in Pig-Iron, Steel, and Wrought-Iron.—F. Kessler.—This lengthy memoir contains the account of a series of experiments instituted by the author with the view to test a method of estimation of phosphorus in the metals alluded to, whereby the metals, after having been dissolved in suitable acids, are completely precipitated by ferrocyanide of potassium, and the phosphorus estimated in the filtrate as pyrophosphate of magnesium. The author has compared this mode of operation with that of the molybdic acid method, and the results are reported to be in favour of the ferrocyanide of potassium plan.

NOTES AND QUERIES.

Solubility of Tin in Nitric Acid.—The solubility of tin in cold dilute nitric acid has long been known. Probably, the colder the acid the less water is needed. If Mr. Hay refers to Gmelin's "Chemistry" (Cavendish Society's edition), vol. v., p. 92, he will find it stated that "very dilute nitric acid, at ordinary temperatures, dissolves tin."—R. W.

Solubility of Tin in Nitric Acid.—Mr. Hay will find an account of the production of stannous nitrate by the solution of metallic tin in dilute nitric acid in Watts's "Dictionary," vol. iv., p. 107. I may also state that a solution of tin in dilute nitric acid has been used by dyers as a mordant in the West Riding of Yorkshire during the last forty years.—Geo. Jarmain, 80, Northgate, Huddersfield.

Solubility of Tin in Nitric Acid.—Among my lecture-notes which I have been using during the last six years, I find this statement:—"Tin, antimony, and tungsten, when heated with the moderately-strong acid, are converted into insoluble oxides; the others, and tin also when diluted acid is taken, are converted into nitrates, all of which are soluble." Of course, only such metals as are acted on by nitric acid are referred to. Never having myself experimented on the subject, this statement regarding the tin must have been published at the time, six years ago, and must, moreover, have already been a recognised fact. Where the original statement is to be found I do not now remember, but that it is to be found there can be no doubt whatever.—A. Dupre, Laboratory, Westminster Hospital, London, S.W.

Solubility of Tin in Nitric Acid.—In your impression of the 16th ult. (Chemical News, vol. xxii., p. 298) Mr. George Hay describes the solvent powers of nitric acid upon tin, dissolving the metal, and forming a yellow solution which is decomposed at the boiling-point, forming the hydrate of meta-stannic acid. In "Notes and Queries" of your impression of the 13th inst., he asks if the fact of the solubility of tin in nitric acid has been published previously to his announcement alluded to above. If he will kindly refer to Brande's "Manual of Chemistry," 1848, p. 783, he will find the solublility announced under the head of "protonitrate of tin." This, says Brande, "may be formed by acting upon the *metal* by dilute nitric acid. . . . . A yellow solution which will not crystallise is obtained; exposed to the air, it absorbs oxygen and peroxide of tin precipitates, or, according to Dumas, a hydrate of the protoxide. If evaporated, the oxide falls, and a portion of nitrate of ammonia remains. During the action of the dilute acid upon tin no gas is evolved; it is therefore evident that water, as well as part of the acid, are in this case decomposed." For the benefit of the student reader, I have not appended the formula of the decomposition from Brande, but have translated it into a modern formula:—$8Sn + 20NO_2HO = 8N_2O_4Sno'' + NO_2Amo + 6OH_2$. Ammonium nitrate being formed simultaneously, as might be expected (Does not Mr. H. B. Gibbins mean the above, and not "evolution of ammonia," in presence of an excess of acid?). I hope this information will suit Mr. G. Hay, as the *yellow liquid* is probably identical with his, seeing that on page 784 Brande says that, by treating the metal with concentrated acid, none remains in solution.—George E. Davis, Eton.

MEETINGS FOR THE WEEK.

Monday, Jan. 23rd.—Medical, 8.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology. (Educational Course.)
—— Royal Geographical, 8.30.
Tuesday, 24th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Ethnological, 8.
—— Civil Engineers, 8.
Wednesday, 25th.—Society of Arts, 8.
—— Geological, 8.
—— London Institution, 7. Conversazione. Prof. Tyndall, LL.D., F.R.S., "On Dust and Disease."
Thursday, 26th.—London Institution, 7.30. Mr. F. S. Barff, M.A., F.C.S., "On the Action, Nature, and Detection of Poisons."
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— Royal, 8.30.
Friday, 27th.—Royal Institution, 9. Dr. Odling, "On Recent Improvements in the Production of Chlorine."
—— Quekett Microscopical Club, 8.
Saturday, 28th.—Royal Institution, 3. Rev. W. H. Channing, "On Laws of Life Revealed in History."

TO CORRESPONDENTS.

*** Vol. XXII. of The Chemical News, containing a copious index is now ready, price 11s. 8d., by post, 12s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our office, or, if accompanied by a cloth case, for 1s. Subscribers wishing to complete their sets of volumes are requested to apply to the publisher, who will give them information respecting scarce numbers and volumes. Vol. xxiii. commenced on January 6th, and will be complete in twenty-six numbers. Reading Cases, price 1s. 6d. each post free, may also be obtained at the Office.

Errata.—In Mr. Highton's letter of last week the following sentence should have been omitted:—"And again, in Mr. Bottomley's problem, though he gets most intensity by his arrangement, he does not get most heat."

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THE CHEMICAL NEWS.

VOL. XXIII. No. 583.

SOME EXPERIMENTS ON THE DISCHARGE OF ELECTRICITY THROUGH RAREFIED MEDIA AND THE ATMOSPHERE.*

By CROMWELL FLEETWOOD VARLEY.

THE nature of the action inside the tube is at present involved in considerable mystery, but some light is thrown upon the subject by the following experiments.

The tube principally used in these experiments contains two aluminium wire rings, the one $\frac{4}{10}$th inch in diameter, the other $\frac{9}{10}$th inch, and separated $\frac{8}{10}$th inch, the tube 1¼ inch in diameter, 3¼ inches in length; it was one of Geissler's manufacture, was very well exhausted, and professed to contain hydrogen. A U-shaped glass tube containing glycerine and water was placed in circuit. Two aluminium wires inserted in this tube gave a ready means of reducing or augmenting the resistance at pleasure. Glycerine affords an easy means of producing very great resistances.

The battery used in this experiment was a Daniell's battery, each cell having a resistance of from 50 to 100 Ohms. The resistance of the glycerine-and-water tube was between 2 and 3 megohms; this latter resistance was made large, in order that the resistance of the tube and battery might be neglected without entailing error.

The following law was found to govern the passage of the current:—1st. Each tube requires a certain potential to leap across. 2nd. That having once established a passage for the current, a lower potential is sufficient to continue the current. 3rd. If the minimum potential, which will maintain a current through the tube, be P, and the power be varied to P+1, P+2, &c. to P+n, the current will vary in strength, as 1, 2, &c., n.

It appears that a certain amount of power is necessary to spring across the vacuum; after that it behaves as an ordinary conductor, excluding that portion of the battery whose potential is P, and which is used to balance the opposition of the tube. In these experiments P was 304 cells. The tube in question could not be persuaded to allow a current of less than 323 cells to pass; but when once the current had established a channel, on lowering the potential by *short circuiting* portions of the battery, so as not so break the circuit, the current would flow when the battery was reduced to 308 cells.

Nature of the Luminous Cloud.—Plücker has shown that when such an exhausted tube, with a current through it, is placed between the poles of an electro-magnet, a luminous arch is produced, which arch follows the course of the magnetic rays.

As the electro-magnet is magnetised, the tube, which before was full of a luminous cloud, is seen gradually to change; the magnet gathers up this diffused cloud, and builds up the arch.

Inasmuch as the electricity was passing in a continuous current from the battery, from wire to wire, it is evident the light is projected right and left into those parts of the tube where there is no electric current flowing.

To endeavour to ascertain the nature of this arch, a special tube was constructed. A piece of talc, bent into the form U, had a fibre of silk stretched across it; on this fibre of silk was cemented a thin strip of talc, 1 inch in length, $\frac{1}{10}$th inch broad, weighing about $\frac{1}{10}$th of a grain. The tube was sealed up and exhausted; carbonic acid and potash were used to get a high vacuum. When the magnet was not magnetised, the passage of the current from wire to wire did not affect the piece of talc. When the magnet was charged, and the luminous arch was made to play upon the lower portion of the talc, it repelled it, no matter which way the electric current was passing.

When the tube was shifted over the poles of the magnet so as to project the luminous arch against the upper part of the talc, the upper end of the talc was repelled in all instances; the arch, when projected against the lower part of the talc, being near the magnet, was more concentrated, and the angle of deviation of the talc was as much as 20°. When the upper part of the arch, which was much more diffused, was thrown upon the upper part of the talc, it was repelled about 5°.

This experiment, I think, indicates that this arch is composed of *attenuated particles of matter projected* from the negative pole by electricity in all directions, but that the magnet controls their course, and these particles seem to be thrown by momentum on each side of the negative pole, beyond the limit of the electric current.

This arch requires time for its formation, for when a charged condenser is discharged through the tube no arch is produced. The arch from the negative pole is a hollow cylinder; the little talc tell-tale against which the arch was projected cut out the light, and a corresponding dark space existed throughout the remainder of the course of the arch.

There was on the talc, at the spot where the arch struck it, a little bright luminous cloud, as though the attenuated luminous vapour was condensed by this material obstruction.

Great care had been taken not to let the arch strike the single filament of silk which suspended the talc. Having demonstrated that the talc was repelled as described, the arch was allowed to play against the silk fibre, which the author expected would have been instantly burnt; such, however, was not the case. Even when a powerful induction-coil replaced the battery, the fibre remained unhurt.

Comparison of the above Phenomena with Discharges between the Poles of a Holtz's Machine in air.

In the first part of this paper four different kinds of discharges were described *in vacuo*. With a "Holtz's" machine, which will give 11-inch sparks in the air, four well-marked different kinds of discharge have been obtained *in the air*; one of which, the author thinks, will explain the curious and rare phenomenon known as "ball lightning."

In the experiments hereafter referred to, the condensers were, in all cases, attached to the "Holtz's" machine. The first discharge is the long 11-inch zigzag spark or lightning-flash; the second is the well-known "brush," which is best obtained by connecting the negative pole of the "Holtz's" machine to the earth; the third kind of discharge is a hissing red flame, ½ inch in length, playing about the negative pole, the positive pole being scarcely luminous at all, and if luminous, at one or two points only; the fourth or most remarkable phenomenon is best obtained in the following manner (I should here remark that the brass balls on each of the poles are about an inch in diameter):—Tie to the negative pole a small thin strip or filament of wood, 3 inches in length, and bent so as to project on each side of the negative pole, and a little beyond it towards the positive. On rotating the machine, two bright spots are seen upon the positive pole. If the positive pole be made to rotate upon its axis, the luminous spots do not rotate with it; if, however, the *negative pole*, with its filament of wood, be rotated, the spots on the *positive pole obey it*, and rotate also. The insertion of a non-conductor, such as a strip of glass, in front of the projecting wooden end, obliterates the luminous spot on the positive pole. When the author first discovered this, he, seeing apparently pieces of dirt on the positive pole, wiped it clean with a silk handkerchief, but there they remained in spite of all wiping; he then examined the negative pole, and discovered a minute speck of dirt corresponding to the luminous spots on the positive pole.

* Abstract of a paper read before the Royal Society.

When the filament of wood is removed from the negative pole, there is sometimes a luminosity or glow over a large portion of the surface of the positive ball. If in this state three or four little pieces of wax, or even a drop or two of water, be placed upon the negative pole, corresponding non-luminous spots will be found upon the positive pole, which rotate with the former, but do not with the latter.

It is, therefore, evident that there are lines of force existing between the two poles, and by these means one is able to telegraph *from* the negative *to* the positive pole to a distance of 8 inches through the air, without any other conductor than that which the electrical machine has constructed for itself across the non-conducting gas.

The foregoing seems to the author to give a possible explanation of "ball-lightning;" if it be possible for there to be a negatively electrified cloud sufficiently charged to produce a flash from the earth to the cloud, a point in the cloud would correspond to the wood projection on the negative conductor; if such a cloud exist, a luminous spot would be seen moving about the surface of the earth, corresponding to the moving point of cloud over it, and thus present phenomena similar to those described by the privileged few who have witnessed this extraordinary natural phenomenon.

The author has been informed more than once by captains of vessels, that when men have been struck by lightning, a slight burn has been left upon the skin of the same shape as the object from which the discharge flew. In one instance he was informed that some brass numbers, attached to the rigging from which the discharge passed to the sailor, were imprinted upon his skin.

It is now seen that this is perfectly possible if the discharge be a negative one, that is, if the man be + to the brass number.

A TABLE FOR CALCULATING THE WEIGHT AND YIELD, PER RUNNING FATHOM, OF MINERAL VEINS.

By E. J. CHAPMAN, Ph.D.,
Professor of Mineralogy and Geology in University College, Toronto, and Consulting Mining Engineer.

THE weight in tons and average richness of mineral veins are usually given per running fathom—that is, per parallelogram measuring 6 feet in length, and 6 feet in depth, by the mean width of the vein, whatever the latter may be. The following table will enable these results to be calculated very rapidly, provided the specific gravity of the vein matter (mixed ore and gangue), and the average percentage of metal or mineral carried by the vein, be previously ascertained. The table has been calculated on the supposition that the specific gravity equals unity, and that the average yield in metal is equivalent to 1 per cent. The values given in columns III. and IV. must thus be multiplied by the specific gravity of the vein matter; and those given in column V. must be multiplied also by this quantity, and the resulting product must finally be multiplied by the average percentage of metal or mineral, as ascertained by estimate or by actual assay. The values in column V. correspond to both the British ton of 2240 lbs., and the American ton (chiefly used in Canada) of 2000 lbs. Where the width of the vein is in feet and inches, the values of the two, as given in the table, must of course be added together.

Example.—A vein averages 3 feet in width, with specific gravity equal to 3·8, and percentage of metal equal to 2·6. Required the weight in British and American tons, and the yield (exclusive of loss in mechanical and furnace treatment) per running fathom.

3 tons (see the Table) × 3·8 = 11½ British tons (nearly).
3·36 tons (see the Table) × 3·8 = 12¾ American tons.
67·30 lbs. (see the Table) × 3·8 × 2·6 = 665 lbs.

Each fathom, therefore, of a vein of this strength will contain 108 cubic feet; will weigh 11½ British, and 12¾ American tons; and will carry 665 lbs. of metal.

I. Width of vein. Feet.	I. Width of vein. Inches.	II. Contents in cubic feet per fathom.	III. Weight in English tons (2240 lbs.), per fathom.	IV. Weight in American tons (2000 lbs.), per fathom.	V. Amount of metal in lbs., average per fathom.
	1	3	0·0833	0·0933	1·87
	2	6	0·1666	0·1866	3·74
	3	9	0·2500	0·2800	5·61
	4	12	0·3333	0·3733	7·48
	5	15	0·4166	0·4666	9·35
	6	18	0·5000	0·5600	11·22
	7	21	0·5833	0·6533	13·09
	8	24	0·6666	0·7466	14·95
	9	27	0·7500	0·8400	16·83
	10	30	0·8333	0·9333	18·70
	11	33	0·9166	1·0266	20·57
1	—	36	1 ton.	1·1200	22·43
2	—	72	2 ,,	2·2400	44·87
3	—	108	3 ,,	3·3600	67·30
4	—	144	4 ,,	4·4800	89·74
5	—	180	5 ,,	5·6000	112·17
6	—	216	6 ,,	6·7300	134·61
7	—	252	7 ,,	7·8400	157·04
8	—	288	8 ,,	8·9600	179·48
9	—	324	9 ,,	10·0800	201·92
10	—	360	10 ,,	11·2000	224·35

ON THE OXIDATION PRODUCTS OF PICOLINE.*

By JAMES DEWAR, F.R.S.E,
Chemical Demonstrator in the University of Edinburgh, and Lecturer on Chemistry at the Edinburgh Veterinary College.

THE combined researches of Anderson and Williams on the basic compounds contained in coal-tar have led to the discovery of two well-defined series of organic bases, called respectively the pyridine and chinoline series, the members of both of which possess the properties of nitrile bases. The isomerism between the pyridine and the aniline series of bases excited considerable interest at the time of its discovery. The subsequent researches of Williams on the products of the distillation of chinchonine led to the discovery of bases having the same composition as the members of the pyridine and chinoline series of coal-tar. When first discovered they were supposed to be identical. Since that time, however, a careful examination and comparison of the tar series of bases with the chinchonine series has led Mr. Williams to the interesting discovery that the two lutidines, as also the two chinolines, are, in reality, not identical, but isomeric. This introduces a greater complexity into the study of the constitution of these compounds.

I began this investigation in the summer of 1867 under the able direction of Professor Aug. Kekulé, in the University of Ghent. At that time the whole of the then known facts regarding the properties of these bases had been accumulated by Anderson and Williams, and by Perkin, who had obtained pyridine from a naphthaline derivative. Save by this latter method the only process of preparation known was destructive distillation. All the attempts that had been made to elucidate the internal constitution and relationship of these bases had failed to yield positive results, their extreme stability in presence of the most powerful reagents presenting a barrier to investigation. In 1869 Professor Adolphe Baeyer made the brilliant synthesis of picoline through the action of tribrom-allyl and of acryl aldehyde, respectively, on ammonia;

* Read before the Royal Society of Edinburgh.

and through the action of higher aldehyde homologues has shown the reaction to be general. Thus, by the synthetical labours of Baeyer, we have acquired for the first time a definite knowledge regarding the mode of formation and constitution of this class of organic compounds.

Having formerly employed permanganate of potassium in the oxidation of phenol (see *Proceedings of the Royal Society of Edinburgh*, Session 1866-67, p. 82), I naturally attempted the oxidation of these bases by the same agent ; and I have since found that A. W. Hofmann had successfully employed it to oxidise chinoline into ammonia and oxalic acid. Finding the members of the pyridine series to be easily attacked by this reagent, I commenced a careful examination of the products of the oxidation of picoline, with the object of learning something regarding its internal structure. A preliminary note on the results obtained was communicated to the British Association at its Norwich meeting, 1868 (See Report Brit. Assoc., 1868).

I am indebted to my friend Dr. Ronalds, of Bonnington, for a liberal supply of a quantity of bases that he had carefully prepared, with the object of instituting an investigation into these compounds himself. The crude bases placed in my hands had been repeatedly fractionated on a large scale, and the individual fractions were thus tolerably pure to begin with. Finding they contained traces of pyrrol and hydrocarbons, I re-dissolved in acid the fraction boiling from 130° to 160° C., and subsequently treated it in the way recommended by Anderson and Williams to purify these bodies. The mixture of bases thus obtained was subjected to a series of careful fractional distillations. With the object of effecting the best possible separation, I employed the method recommended by Warren, and found it admirably suited to effect a comparatively easy separation, so far as fractional distillation can be made to yield a pure product. The bases were transferred to a retort connected with an ascending spiral of copper tube enclosed in a paraffine bath of large dimension, the temperature of which was continuously equalised by constant stirring. Five successive fractionations by this method gave a separation as complete as was necessary for the object I had in view. From the laborious researches of Anderson and Williams, we know that these bases for a large range of temperature have the same composition, and that perfectly pure products can be obtained only through the fractional precipitation of the platinum salts. Although I did not make an exhaustive separation by Warren's method, the fractionation was so effective that, by comparing the temperature of the boiling vapour in the retort with the temperature of the paraffine bath throughout the whole course of a distillation, a difference of 10° C. at starting gradually increased to 20°. No fraction that I obtained, separated in this process by a difference of 2° C. in the intermediate condenser, had a perfectly constant boiling-point. On different occasions fractions separated in this way, boiling between 130° and 140° C., have been employed in the following experiments. Analyses of the platinum salts of portions boiling between those temperatures showed the fractions to be substantially picoline, with a possible trace of lutidine.

Picoline, as is well known, resists oxidation by the most powerful agents adapted for this purpose, nitric and chromic acids being without visible action, even at high temperatures. Of all oxidising agents, permanganic acid, or its potassium salt, seems to be the most powerful. Having, on a former occasion, employed this reagent to effect the oxidation of phenol alcohol, I naturally investigated the action effected on these nitrile bases. I found that the whole of the members of this series of compounds could be readily oxidised by the use of this substance. The higher members of the series were oxidised more readily than the lower, but in all cases it was effected with ease and rapidity. The following is a description of the apparatus used in the experiments on the oxidation products of these substances and of the mode of conducting the operation. A flask, about three litres in capacity, was connected by a wide tube with a large reversed Liebig's condenser, so as to effect a rapid condensation of volatile products and their immediate return to the field of chemical action. The flask having been placed on a sand-bath, 150 grms. potassium manganate, 1½ litres water, and 25 grms picoline were introduced, and the whole heated to near the boiling-point. The reaction began suddenly, with great evolution of heat, necessitating the removal of the source of external heat. The reduction of the permanganate was completed in half an hour. After the contents of the flask had cooled, the oxide of manganese was separated by filtration from the strongly alkaline liquid, and washed repeatedly with boilng water. The alkaline liquid was then transferred to a flask, and the basic substances distilled off. The residual alkaline liquid was then concentrated by evaporation to 200 c.c. and 300 c.c. and dilute sulphuric acid (containing 70 per cent H_2SO_4) added to it. After standing for some time, this acid liquid became thick from a deposit of long white crystalline needles of a complex of new acids. In different experiments the relative proportions of the reacting substances were considerably varied, but the yield of the new acids in every case was small, a large portion of the picoline having been completely oxidised, while some of it had remained unacted upon. This must always be the case, as a large quantity of the original base, from the violence of the reaction, was driven away from the flask, and, when condensed, it fell back into a boiling liquid of increasing alkalinity, in which the base was comparatively insoluble. After separating, by filtration, the new crystalline acid referred to, the filtrate was transferred to a retort, and the volatile acids distilled off.

General Results of Oxidation.—When the dilute alkaline fluid was taken immediately after the permanganate was exhausted, it was found to contain carbonate of potassium. When neutralised with hydrochloric acid and chloride of calcium added, a white precipitate of oxalate of calcium was obtained. The oxalate was mixed with a small quantity of some higher acid, probably malonic, as the percentage of lime found was 36·3, oxalate containing 38·3 per cent. The quantity of volatile acid produced by the reaction was small. The presence of nitric and acetic acids, however, was readily proved. The surplus base remaining after the oxidation operation was transformed into the double platinum salt, and a fractional crystallisation made. The platinum was determined in these different salts as follows :—

	grm.		grm.	per cent.
1st Cryst.—	0·1770	pt-salt gave	0·0770	pt = 43·70
2nd ,, —	0·6913	,, ,,	0·2102	,, = 33·95

The first sample agreed in composition with the double chloride of platinum and ammonium ; the second, with that of a mixture of the double platinum salts of pyridine and picoline. There was produced by the reaction, therefore, carbonic, nitric, oxalic, acetic, and a complex of new acids, ammonia, and probably a small quantity of pyridine. The relative proportion of the products obtained depended on the quantity of the oxidising agent used, the volume of the solution, rapidity of the action, and the quantity of bases present in the field of action.

Di-carbo-pyridenic Acid, $C_5H_3N\begin{cases}CO_2H\\CO_2H.\end{cases}$

The crystalline acid substance separated by adding excess of sulphuric acid to the alkaline fluid, after standing over night was collected on a filter, and repeatedly crystallised from hot water (in which it was easily soluble) until free from oxalic acid and potassium salts. When first separated fram the acid solution it appeared in long needles ; but after several re-crystallisations from water it was obtained in the form of perfectly colourless plates, resembling naphthaline. (The crystallographic constants of this acid have not yet been determined). The crystals did not contain any water of hydration. The following are the results of the analyses of the acid and its silver salt :—

Acid.

	Grm.
Weight of acid taken	0·2195
Carbonic anhydride produced	0·0685
Water produced	0·4040
Nitrogen found by Gottlieb's method to bear to the CO_2 produced, the proportionate vol. of,	1 to 14·2

Calculated centesimally these figures give—

	Samples.	$C_7H_5NO_4$.
Carbon	50·19	50·29
Hydrogen	3·47	2·99
Nitrogen	—	—

*Silver Salt.**

As with the acid, the salt was dried at 100° C.

	I.	II.
Weight of salt taken	0·8766	0·5456
Carbonic anhydride produced	0·6985	0·4518
Water produced	0·0554	0·0602
Weight of salt taken	0·6980	0·3951
Silver obtained	1·5111	0·6525

Calculated centesimally these results give—

	I.	II.	$C_7H_3NAg_2O_4$.
Carbon	21·73	22·58	22·04
Hydrogen	0·70	1·22	0·78
Silver	56.60	56·78	56·69

In order to determine the equivalent of the acid, I took 0·5392 grm. of acid dried at 100° C. and titrated with pure caustic soda solution, every 1·813 c.c. containing 23 mgrm. of sodium; 11·7 c.c. of soda neutralised the acid taken. The point of saturation was well marked. The equivalent of the acid, from this determination was, therefore, 83·55; as determined by analysis of the silver salt it was 83·5. The atomic weight of the acid taken as $C_7H_5NO_4$ was exactly double the equivalent found, which, if true, would necessarily involve the acid being bibasic. In order to determine the basicity of the acid, the ammonium salts were the only combinations that I specially examined. 0·4739grm. of the acid carefully treated with excess of ammonia, and dried at 100° C., increased in weight by 0·0481 grm., gain = 10·13 per cent; gain on the acid ammonium salt of foregoing formula = 10·17 per cent. The neutral ammonium salt was extremely soluble in water, whereas the acid salt was much less soluble, and could readily be obtained in the form of fine silky needles when the solution was evaporated. From the above data there can be little doubt that the acid was bibasic, bearing the same relation to pyridine that phthalic acid does to benzol. The acid melted at a temperature of about 210° C., frothed, evolved a small quantity of carbonic anhydride, and emitted the readily recognisable smell of these bases. It was easily decomposed when heated with soda-lime, evolving a basic substance, no doubt pyridine. The mercury, copper, cadmium, and zinc salts were all readily soluble in water. The barium and calcium salts were also soluble, and were obtained by adding the respective chlorides to the neutral sodium or ammonium salt; they crystallised in minute prismatic needles. The silver salt of this acid was specially characteristic. On the addition of nitrate of silver to a solution of the acid or its neutral ammonium salt, a white gelatinous precipitate immediately separated out—it was insoluble in boiling water, and was not visibly affected by exposure to light. The insolubility of this salt would enable us more readily to separate the acid from the other products of the oxidation reaction than the process first quoted in this paper.

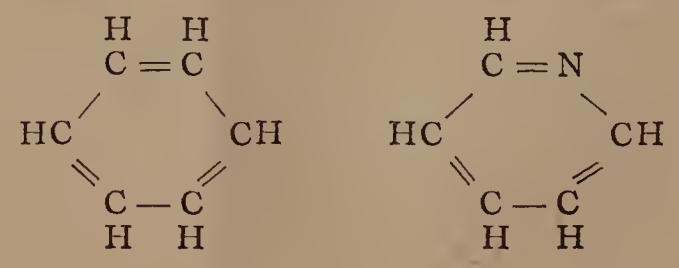
Along with the acid just described, as separated by the process detailed, there was found associated with it another acid substance having a very much higher atomic weight. The crystalline mass, obtained by the addition of sulphuric acid to the alkaline liquid from the oxidation operation, purified by solution in and re-crystallisation from alcohol, and dried over sulphuric acid by means of an air-pump, had an equivalent weight of 121. The crystals were hydrated, and lost 4·9 per cent of their weight when dried at 100° C. The sodium salt of this mixture gave, on the addition of nitrate of silver, a gelatinous precipitate agreeing in appearance and composition with that got from dicarbopyridenic acid; it contained 56·6 per cent of silver. The equivalent of this mixture, as determined by the composition of the ammonium salt dried at 100° C., was 336; 0·8335 grm. of acid mixture dried at 100° C., and treated with ammonia, increased in weight by 0·0422 grm. This acid substance, therefore, was clearly a mixture of dicarbopyridenic acid with some acid of a very much higher atomic weight. It remained solid when heated to 220° C., at which temperature dicarbopyridenic acid readily melted, and was much less soluble in water than the latter.

The difficulty and expense of obtaining these oxidation products in any quantity, prevented me from making the exhaustive investigation I would have liked.

The formation of dicarbopyridenic acid by the oxidation of a mixture of picoline and lutidine, whether obtained from lutidine alone or by the complete destruction of picoline, is quite analogous to the formation of phthalic or teraphthalic acid, by the oxidation of the homologues of benzol, or by the complete destruction of benzol itself, as shown by Carius; the only difference being that Carius employed the lowest member of the benzol series, whereas picoline is the second known member of the basic series. The production of the same acid from pyridine itself would in no wise influence speculation regarding the constitution of the high members of the series. For the present, we may consider pyridine as the nucleus from which all the other members of the series are derived. Athough such a supposition must be considered purely hypothetical, in reality it is a great advantage to classify by analogy, relatively to other series, disjointed groups of organic compounds. The two series of bases, viz., the coal-tar and the chinchonine, bear the same empirical relation to pyridine that benzol does to its homologues and to naphthaline.

Benzol.	Toluol.	Naphthaline.
C_6H_6	C_6H_6	C_6H_6
	CH_2	C_4H_2
Pyridine.	**Picoline.**	**Chinoline.**
C_5H_5N	C_5H_5N	C_5H_5N
	CH_2	C_4H_2

Now, although it has not yet been proven that lutidine and chinoline have a similar formative relation to pyridine that toluol and naphthaline have to benzol, still it is by no means an improbable analogy. The isomerism in the pyridine series, so far as is known, commences with the third member, lutidine, as found by Williams on comparing the chinchonine lutidine with the coal-tar lutidine; whereas the chinoline obtained from either source differs essentially in chemical characters. If we consider picoline as in all likelihood methyl-pyridine, then the α and β lutidines may, in all probability, be represented as dimethyl and ethyl-pyridine respectively, and we would expect the dimethyl-pyridine to give directly dicarbopyridenic acid, or an isomer, on oxidation. Pyridine may be written graphically as benzol in which nitrogen functions in place of the triatomic residue CH‴, and thus may be represented as a closed chain:—



* I. and II. were samples of silver salts obtained from different experiments. I. was by double decomposition of the sodium salt; II. from the ammonium salt.

And, considering the stability and mode of formation of these bases it is not at all improbable that they may not be produced by the simultaneous action of acetylene and its derivatives on hydrocyanic acid; thus as three molecules of acetylene condense and form benzol, so may two molecules of acetylene, and one of hydrocyanic acid, condense and produce pyridine.

There is a large class of substances that bear the same relation to the monamines that dicarbopyridenic acid does to pyridine, with this difference, that the best known are all monobasic instead of being bibasic acids. Thus glycocol, alamine, leucine, and their homologues, may be looked upon as the monocarbo-acids of the ethylamines, in which the carboxyl radicle is united directly to the carbon, the isomeric carbamic acids (or urethanes, as they are called) being the derivatives in which the carboxyl is united directly to the nitrogen :—

Ammonia.	Methylamine.	Glycocol.	Methyl-carbamic Acid.
H	CH_3	CH_2CO_2H	CH_3
N H	N H	N H	N H
H	H	H	CO_2H

Analogous derivatives are obtained from the aromatic ammonias. A class of derivatives similar to the above, must necessarily be derivable from the diamines. In the case of the nitrile bases or triamines, only derivatives could be obtained analogous to glycocol and its homologues. Bodies of a like constitution to glycocol readily break up into carbonic anhydride, and the corresponding ammonia; the reverse transformation has not yet been effected. The amido-bibasic acids bear the same relation to the monamines that dicarbopyridenic acid does to pyridine, with the exception of the difference in the constitution of the closed nitrile nucleus. No bibasic acid, other than pyridenic, has been discovered. A strictly analogous compound, in the monobasic series, is the acid carbo-pyrrolic, $C_5H_5NO_2$, which bears a similar relation to pyrrol that the amido-mono-carbon acids do to the ammonias, only that the pyrrol, although a nitrogenous body, is not, strictly speaking, a nitrile base. But the close analogy between pyrrol and true nitrile bases, they being simultaneously produced in the majority of reactions, would lead us to expect a like class of derivatives being obtainable from pyridine.

Hübner (*Ann. Chem. u. Pharm.*, vol. 141) has described the oxidation products of nicotine, got by the action of sulphuric acid and bichromate of potassium. In that memoir he describes an acid so obtained, having the formula $C_5H_5NO_2$. This acid is identical in composition with mono-carbopyridenic acid. The base nicotine

Nicotine.	Dipyridine.
C_5H_7N	C_5H_5N
C_5H_7N	C_5H_5N

itself differs from dipyridine by only four hydrogens; and as the nucleus of nicotine is a nitrile nucleus, it is not at all improbable that this acid may be a member of the series to which dicarbopyridenic belongs, so that nicotine may be similarly constituted to Anderson's polymerised bases.

The stability of these bases to the majority of reagents (especially the primary member of the group, pyridine) would predispose us to look upon it as analogous to benzol, and to suppose that the atoms are symmetrically grouped. The syntheses of Baeyer support this view, and there is not any reason why we may not have as many stable derivatives from this nucleus as from benzol. I have already pointed out the analogy between the chinoline series and the pyridine series, and in a short time I hope to be able to publish details supporting the theoretical relations above given.

In the meantime, the following analogies may be pointed out between benzol and nitrile derivatives; thus :—

Benzol.	Naphthaline.	Anthracene.	Pyridine.	Chinoline.
C_2H_2	C_6H_4	C_6H_4	C_2H_2	C_5H_3N
C_2H_2	C_2H_2	C_2H_2	NCH	C_2H_2
C_2H_2	C_2H_2	C_6H_4	C_2H_2	C_2H_2

Indol, from its general characteristics, evidently belongs to the pyrrol series, the following showing, in all probability, the relative structure of indol and pyrrol :—

Indol.	Pyrrol.
C_6H_4	C_2H_2.
NH	NH
C_2H_2	C_2H_2

According to this hypothesis indol is simply benzol-pyrrol.

CHEMICAL TABLES ACCORDING TO THE THEORIES OF MODERN CHEMISTRY.*

By Professor ALBERT R. LEEDS.

(Continued from page 30.)

TABLE IV.

Atomic Weights according to the Modern Chemistry.

Element.	Quantivalence.	Atomic symbol.	Atomic weight.	Logarithm.	Ar. Co.
Aluminum	II. or IV.	Al.	27·48	1·43902	8·56098
Antimony	III. or V.	Sb.	122·34	2·08757	7·91243
Arsenic	III. or V.	As.	75·00	1·87506	8·12494
Barium	II.	Ba.	137·02	2·13678	7·86322
Bismuth	III. or V.	Bi.	210·34	2·32292	7·67708
Boron	III.	B.	11·00	1·04139	8·95861
Bromine	I.	Br.	79·97	1·90293	8·09707
Cadmium	II.	Cd.	112·00	2·04922	7·95078
Cæsium	I.	Cs.	133·00	2·12385	7·87615
Calcium..	II.	Ca.	40·02	1·60228	8·39772
Carbon	IV.	C.	12·00	1·07918	8·92082
Cerium	II.	Ce.	91·33	1·96061	8·03939
Chlorine	I.	Cl.	35·50	1·55023	8·44977
Chromium	II. or IV.	Cr.	52·54	1·72049	8·27961
Cobalt	II.	Co.	59·08	1·77144	8·22856
Columbium	V.	Cb.	98·45	199·322	8·00678
Copper	II.	Cu.	63·44	1·80236	8·19764
Didymium	II.	D.	95·84	1·98155	8·01845
Erbium	II.	E.	112·60	2·05154	7·94846
Fluorine..	I.	F.	19·00	1·27875	8·72125
Glucinum	II.	G.	9·29	0·96802	9·03198
Gold	III.	Au.	196·73	2·29387	7·70613
Hydrogen	I.	H.	1·00	—	10·00000
Indium	II.	In.	75·63	1·87869	8·12131
Iodine	I.	I.	127·00	2·10380	7·89620
Iridium	II. or IV.	Ir.	197·13	2·29476	7·70524
Iron..	II. or IV.	Fe.	56·00	1·74819	8·25181
Lanthanum	II.	Ln.	92·80	1·96755	8·03245
Lead	II.	Pb.	207·00	2·31597	7·68403
Lithium..	I.	L.	7·00	0·84510	9·15490
Magnesium	II.	Mg.	24·60	1·39094	8·60906
Manganese	II. or IV.	Mn.	54·96	1·74005	8·25995
Mercury..	II.	Hg.	200·20	2·30146	7·69854
Molybdenum ..	VI.	Mo.	96·00	1·98227	8·01773
Nickel	II.	Ni.	59·02	1·77100	8·22900
Nitrogen	III. or V.	N.	14·00	1·14613	8·85387
Osmium..	II. or IV.	Os.	198·80	2·29842	7·70158
Oxygen	II.	O.	16·00	1·20412	8·79588
Palladium	II. or IV.	Pd.	106·56	2·02760	7·97240
Phosphorus	III. or V.	P.	31·00	1·49136	8·50864
Platinum	II. or IV.	Pt.	197·88	2·29641	7·70359
Potassium	I.	K.	39·13	1·59251	8·40749
Rhodium	II. or IV.	R.	104·32	2·01836	7·98164
Rubidium	I.	Rb.	85·4	1·93146	8·06854
Ruthenium	II. or IV.	Ru.	104·20	2·01787	7·98213
Selenium	II. or VI.	Se.	79·24	1·89894	8·10106
Silicon	IV.	Si.	28·02	1·44747	8·55253
Silver	I.	Ag.	107·95	2·03322	7·96678
Sodium	I.	Na.	23·05	1·36267	8·63733
Strontium	II.	Sr.	87·48	1·94191	8·05809
Sulphur	II. or VI.	S.	32·00	1·50515	8·49485
Tantalum	V.	Ta.	182·00	2·26007	7·73993
Tellurium	II. or VI.	Te.	129·00	2·11059	7·88941
Terbium	—	Tb.	75·36	1·87714	8·12286
Thallium	I. or III.	Tl.	204·00	2·30963	7·69037
Thorium	IV.	Th.	238·00	2·37658	7·62342
Tin	II. or IV.	Sn.	118·06	2·07210	7·92790
Titanium	II. or IV.	Ti.	50·34	1·70191	8·29809
Tungsten	VI.	W.	184·00	2·26482	7·73518
Uranium	III. or V.	U.	118·86	2·07510	7·92490
Vanadium	III. or V.	V.	51·33	1·71037	8·28963
Yttrium	II.	Y.	61·70	1·79029	8·20971
Zinc	II.	Zn.	65·06	1·81331	8·18669
Zirconium	IV.	Zr.	89·60	1·95231	8·04769

(To be continued).

* Communicated by the Editors of the *Journal of the Franklin Institute*. From advance-sheets.

JOULE AND SCORESBY'S EXPERIMENTS ON THE MECHANICAL POWERS OF ELECTRO-MAGNETISM, STEAM, AND HORSES.

By the Rev. H. HIGHTON, M.A.

SOME weeks ago I promised an examination of Jacobi's paper on "Electro-Dynamic Engines," in *Ann. de Chim. et de Phys.*, vol. xxxviii.; I have not lost sight of the matter, but think it as well to examine first Joule and Scoresby's experiments as given in the *Philosophical Journal* [3], xxviii.

Let me say how exceedingly valuable I consider Joule's experiments generally, and how fully I appreciate his title to the honours that have been bestowed on him; but, at the same time, though his experiments are exceedingly valuable for the legitimate conclusions which may be drawn from them, yet I consider that the deduction, that heat has a definite mechanical equivalent, and that the admission of the supposed axiom of conservation of energy as applied to electricity and heat as well as mechanics, or rather to electricity and heat as connected with mechanics, has done great mischief in a scientific point of view, and thrown back the progress of the science of electro-dynamics for many years.

The paper I now propose to examine contains in itself a sufficient refutation of these views; and it appears astonishing that it should have been allowed for so long to be quoted as a reliable authority in support of them.

Let me first give a brief description of their experiments. They first fixed strong permanent magnets; they next formed electro-magnets which, by means of commutators and the aid of a galvanic battery, rotated in front of the permanent magnets; by this rotation, through the medium of proper machinery, they raised weights. They measured and compared the intensities of the current and the consumption of zinc, when the engine was at rest and when it was at work, and compared the work done with the zinc consumed. So far, there is nothing to object to; it is their reasonings on the facts, and the mathematical formulæ which they introduce, which, to my mind, require nothing but an examination to show how hopelessly wrong is the theory of a definite mechanical equivalent of heat. They begin by asserting that as the heat evolved in a circuit is *(cæteris paribus)* as the square of the intensities; therefore, if a and b represent respectively the intensity (and, consequently, the consumption of zinc) when the engine is at rest and when it does work, the heat evolved in the two cases will be as a^2 to b^2; but, as this is not a case of *cæteris paribus*, there seems no reason why this statement is introduced, or what bearing it has on their experiments; and no use is made afterwards of this assumption, but they estimate in the sequel the heat produced by the quantity of zinc consumed. But Messrs. Joule and Scoresby's next computation is the one I wish particularly to draw attention to, as nothing can better show how utterly untenable are their views of the subject. They say that "the quantities of zinc consumed" (that is, respectively, when the engine is at rest and doing work) "being as a to b, $(a—b)$ represents the quantity of heat converted by the engine into useful mechanical effect." Therefore, since on the supposition of a mechanical equivalent of heat a grain of zinc consumed equals 158 foot-pounds, if x = pounds raised a foot high per consumption of a grain of zinc in the battery,—

$$x = \frac{(a—b)158}{a}$$

Hence the authors draw the conclusion:—"Therefore, when b vanishes, or becomes infinitesimally small, the economical duty is a maximum."

Certainly this is a most startling result; that the maximum of work should be done when no zinc at all is consumed! But it is quite evident that the authors assumed, and framed their equation on the assumption, that the battery produced the same amount of heat, whatever quantity of zinc was consumed; but that part of the heat was absorbed and disappeared in doing work. But I need hardly say that this assumption is entirely erroneous, that the only possible source of heat could be the consumption of zinc, and that when the zinc consumed fell from 500 to 200 grains per hour, the difference of heat corresponding to the two quantities could not be absorbed or disappear because it never was produced at all. It is really difficult to imagine how conclusions based on such a formula and on such a theory, would ever have been accepted. I am sure it is only necessary to call attention to it in order that the error should be recognised. I can scarcely help imagining that I must have misunderstood the authors' meaning; but I really cannot see how the equation or their statements will bear any other interpretation. Seeing, then, that this was the formula on which their calculations were based, no wonder if the results were not according to their calculations. In their first experiment they calculated that 92·9 pounds should be raised a foot per grain of zinc consumed; 102·9 were actually raised. In their second they calculated 97·8; the result was 93·8. When, according to the theory, the duty should have been less, it was actually more. The only wonder is (and it can have been due to little more than accident) that the agreement was anything nearly as great as what it is. They attributed the excess of performance over calculation to the fact that the acid of the battery was rather warm, and that, therefore, the battery produced greater intensity of current. But they forgot that if it produced greater intensity of current more zinc would be consumed, so that this would make so difference to the duty of a grain of zinc. I pass over their other experiments; for, inasmuch as in these they substituted for the electro-magnets used in the first experiments less powerful and less skilfully constructed ones, no wonder if they reduced the results to 34 foot-pounds per grain of zinc. They might in the same way have reduced the economical effect to any extent. But after having thus reduced the economical duty of a grain of zinc by badly constructed electro-magnets, it is very unfair to strike a kind of average, and to say that the *maximum* practical power of a grain of zinc is 80 foot-pounds, whereas they had themselves got 102·9 foot-pounds.

Well, now let us look at the general result of the real facts. They calculate the maximum theoretical power of a grain of zinc to be 158 foot-pounds, and yet, using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electro-magnets never approaching the permanent magnets nearer than ¼ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at hap-hazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot-pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased ten-fold.

Now contrast this with what they tell us of the performance of a steam-engine. The best steam-engine, according to their account, produces only about one-tenth of the theoretical duty of the mechanical equivalent of the coal consumed; and a horse only about a quarter of the theoretical duty of the hay and oats which he consumes, and yet in an electro-dynamic engine constructed at hap-hazard, without any definite principles or calculation of economy, about two-thirds of the maximum theoretical duty is at once secured.

I conclude, then, that these experiments, instead of being supposed to have confirmed, should at once have shown the falsity of the theorem of a definite mechanical equivalent of heat or chemical change.

Putney, January 7, 1871.

PS. I find that in a letter, a few weeks ago, on the question of whether a magnet, in contact with a piece of

soft iron, produces a pole of an opposite name or of the same name only, I did an injustice to Professor Tyndall in assigning the first correction of the mistake to Du Moncel, in 1858. Tyndall pointed out the error in 1851, and says he finds that Poggendorff and Van Rees had both also made the same observation. Is it not strange that now for twenty years all our text-books (as far as I know) go on repeating the error?—H. H.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, January 19th, 1871.

Professor Odling, F.R.S., Vice-President, in the Chair.

The following gentlemen were elected Fellows:—R. Bannister, H. T. Brown, J. Moss, R. J. Moss, E. Potts. The following papers were read:—

"*On the Action of Sulphuric Acid on the Natural Alkaloids*," by Henry E. Armstrong.

The action of sulphuric acid on the natural alkaloids was first made the subject of investigation by Arppe, who, in 1845, shortly described a body obtained from morphine, to which he gave the formula $4(C_{35}H_{40}N_2O_6)+5SO_3$. Laurent and Gerhardt, led by their researches on the amides and anilides to doubt the correctness of this formula, as, if true, a reaction had taken place entirely without analogy, re-investigated the matter, including both morphine and narcotine in their experiments. From the results obtained they considered the composition of the bodies which they succeeded in preparing, to be represented by the formulæ $C_{34}H_{36}N_2O_8S$ and $C_{46}H_{52}N_2O_{18}S$, and termed them respectively, sulphomorphide and sulphonarcotide, looking upon them as derived from the neutral sulphates of the bases by the abstraction of the elements of two molecules OH_2.

$$2C_{17}H_{19}NO_3.H_2SO_4-2OH_2=2C_{17}H_{18}NO_3.SO_2,$$
$$2C_{23}H_{25}NO_7.H_2SO_4-2OH_2=2C_{23}H_{24}NO_7.SO_2.$$

They considered them, in fact, to be true amides, and as bearing the same relation to the neutral sulphates of the respective bases as do sulphamide and sulphanilide to the neutral sulphates of ammonia and aniline.

By the action of SO_4H_2 on codeine, they obtained a body identical with it in composition, and to which they gave the name of modified or amorphous codeine.

The light thrown on the constitution of the organic bases by the researches of Dr. Matthiessen and others rendered it highly improbable that the above-mentioned bodies were constituted as Laurent and Gerhardt believed, for the discovery of apomorphine at once seemed to point to the possibility of the morphine product being none other than the sulphate of that base. Their identity was proved by Drs. Matthiessen and Wright, who succeeded in isolating apomorphine from the product of the reaction of sulphuric acid on morphine in sealed tubes at 100°. This rendered an analogous constitution of the narcotine and codeine products probable, a surmise which has, however, not been confirmed by experiment.

On heating narcotine with an excess of acid, of the strength of equal volumes of ordinary concentrated sulphuric acid and water in an open porcelain dish on a water-bath, it is observed that the colour of the mass gradually darkens, and that, after a time, and almost suddenly, the whole becomes of a dark pink colour. It is then thrown into a considerable quantity of warm water, in which it entirely dissolves, and a slight excess of NH_4OH added, which throws down the base as an amorphous, almost white precipitate. This was brought on a filter and washed with warm water. It was found to be very easily soluble in alcohol, insoluble in Na_2CO_3, readily soluble in KOH, and when heated under water to cake together in the form of a sticky semi-fluid mass. This behaviour at once suggested the probable nature of the base, for all the above properties agree with those exhibited by the first product of the action of HCl narcotine, viz., dimethylnarcotine, and a further examination proved this to be the case. It was purified, dried at 100°, and analysed. The figures obtained lead to the formula $C_{21}H_{21}NO_7$. The reaction has, therefore, taken place according to the equation—$C_{22}H_{23}NO_7+H_2SO_4=C_{21}H_{21}NO_7+CH_3HSO_4$. The methylsulphorinic acid is then further decomposed.

On treating codeine in a similar manner, and heating until the precipitate produced by Na_2CO_3, did not visibly increase, dissolving the product in water, precipitating with Na_2CO_3, re-dissolving in HCl, re-precipitating with Na_2CO_3, which operation was twice repeated in order to remove any codeine, then, extracting with ether, and shaking up the ether with HCl, a crystalline hydrochlorate was obtained which, on analysis, gave numbers agreeing with those calculated for codeine hydrochlorate. The first product of the action of H_2SO_4, on codeine, is, therefore, a body isomeric with it. The hydrochlorate crystallises in hexagonal pyramids, radiating from a common centre, and differs from the corresponding codeine hydrochlorate by losing both its atoms of water of crystallisation at 100°. The platinum salt is amorphous, and may be obtained anhydrous by drying at 100°, whereas the platinum salt of codeine is crystalline, and does not lose the whole of its water of crystallisation below 120°.

By the further action of sulphuric acid one molecule of H_2O is taken out from two molecules of codeine, then one of H_2O from one of codeine, and by still advancing action CH_3 is removed, and thus apomorphine formed. On this point, however, some more evidence is required.

Other bases, like strychnine and papaverine, are not in the least affected by H_2SO_4.

"*On the Origin of Nitrates in Potable Waters*," by Ch. Ekin.

The author found considerable quantities of nitric acid in spring water, for which he could not account by supposing it to come from some sewage contamination. Closer examination showed that the water in question had passed through a fossiliferous stratum. This observation necessitates a modification of the "previous sewage contamination theory."

"*On an Alkaloid from Cinchona Bark hitherto undescribed*," by D. Howard.

In experimenting upon impure crystallisations of salts of quinine obtained from the mother liquors of the manufacture of sulphate of quinine, I have occasionally been perplexed by an unusual loss in re-crystallising, which the mechanically adhering mother-liquor did not seem to account for.

A more careful examination of some of these substances shows that the cause, in some cases at least, is the presence of an alkaloid hitherto undescribed, the extreme solubility of the salts of which both distinguishes it at once from the cinchona alkaloids already known, and renders it very difficult to separate from the uncrystallisable quinoidin.

The most convenient method of obtaining it is to purify the alkaloids contained in the mother-liquor from the re-crystallisation of such impure products as I have mentioned by solution in ether, and, after evaporation of the ether, to dissolve with oxalic acid in as small a quantity of water as possible, and to allow it to crystallise. The oxalate thus obtained may be purified by re-crystallisation from water with the addition of animal charcoal, but I have never been able to free it entirely from a yellow colour.

The most satisfactory salt for analysis is the platinochloride, which is prepared in the usual manner. It is almost insoluble in water or in cold hydrochloric acid; it

forms a crystalline powder by precipitation, and well-defined crystals by solution in acid.

The analysis shows that it is isomeric with the platino-chloride of quinine, but anhydrous, instead of containing one atom of water of crystallisation given off at 120°, as does the salt of quinine.

The author has investigated the sulphate, tartrate, citrate, hydrochlorate, phosphate, and sulphocyanide of the new base. He has, however, not yet found out whether this alkaloid is contained in all the species of cinchona, or, if not, in which of them. He hopes to settle this question at some future time.

At the conclusion of the meeting Mr. Macleod exhibited an ingenious little contrivance by means of which eudiometer tubes which have lost the outer portion of their discharging wires may yet be made use of.

NOTICES OF BOOKS.

Beet-Root Distillation; Containing a Report on the subject by Dr. A. Voelcker. F.R.S., &c., and Particulars and Prices of Beet-Root Distilleries. D. Savalle, and Co., Paris, and 10, Basinghall Street, London.

This pamphlet, copiously illustrated with woodcuts, representing the stills and some of the accessory apparatus, is divided into the following sections:—Introduction; Extracts from Reports; List of Some of the Most Important Distilleries; Dr. Voelcker's Report; Opinion of the Right Hon. J. Howard, M.P., and W. Crookes, F.R.S., on the Savalle Stills; a short paper taken from the *Food Journal*; Is Beet-Root Distillation Profitable? Beet-Root Distillation and Sugar Making; Value of Beet-Root Grown in Great Britain; Value of the Pulp as Food for Cattle; What is Beet-Root Spirit good for? Useful Information; Erection of Distilleries; Alphabetical Index. The work, the contents of which we have just quoted, contains a very large amount of useful knowledge for landowners aud farmers who can lay out a portion of their capital so as to convert part of their estate to what may be termed agricultural industry. It appears, from Dr. Voelcker's report, by far the most interesting and valuable portion of the book, that the distilling and rectifying Savalle stills, which the Doctor saw in operation on Mr. R. Campbell's estate, Buscot, Berkshire, produce an extremely pure alcohol of very high strength, and not distinguished in any way from pure spirit of wine. We are, moreover, informed by the same author that distilling and refining the crude beet-root by these stills may be carried out by any man of moderate means and ordinary intelligence, for in great measure they may be said to be self-regulating, while, in point of economy and superiority of the products of distillation, the apparatus alluded to leaves little to be desired. We can recommend this little book, in the first place, to all agriculturists who desires to gain information on an important subject, and, secondly, to all who take an interest in the progress and improvements of technology and industrial art.

Victoria.—Patents and Patentees from 1854 *to* 1866 (both inclusive). Vol. ii., Indices for the year 1867. Vol. iii., Indices for the year 1868. Compiled from Specifications lodged in the Patent Office attached to the Registrar General's Department, Melbourne. By William Henry Archer, Registrar General of Victoria. Melbourne: by authority, John Ferres, Government Printer.

From the works, of which we have just quoted the titles, some idea may be obtained of the high state of industrial importance already prevailing in the comparatively young colony of Victoria. The arrangement of these official publications is very similar to that adopted for this subject at the Patent Office of the United Kingdom, where the above-named books can be inspected.

CORRESPONDENCE.

THE SEWAGE QUESTION.

To the Editor of the Chemical News.

Sir,—I have only to day been able to read Mr. Stanford's paper on this subject which appeared recently in the Chemical News (vol. xxii., pp. 289, 301), but perhaps in view of the extreme importance of the subject you will still allow me to say a few words in reply, especially as my friend Mr. Stanford evidently intended to "draw" me.

I desire first to point out that in quoting from a paper I read last year at the Society of Arts, Mr. Stanford has made it appear that I was seriously contemplating "a convalescent hospital for diseases of the chest in the middle of my sewaged fields," whereas I was laughing at some theories of my friend Dr. Carpenter (of Croydon), and reducing them to a *reductio ad absurdum* by suggesting, as their logical sequence, a "copropathic establishment" on my farm at Romford.

In another place Mr. Stanford triumphantly exclaims that I have never disproved some "assertions" which he had made in a previous paper which appeared in the Chemical News (vol. xix., p. 255), but the simple truth is that as reading the Chemical News is, with me, a matter of relaxation and not a matter of business, I am not always able to read it, and I never *saw* his other paper, but as he now repeats his "assertions," and I thus have cognisance of them for the first time, I have much pleasure in contradicting them, and beg to apologise to him for not having done so before. He "asserts"—and I am glad he admits it is only an assertion—that "irrigation puts an amount of money value on the ground out of all proportion to the return obtained by the ratepayers." I admit that this statement is perfectly true of the great majority of sewage farms at this moment, but this is not because of any fault in the principle of sewage irrigation, but owing to the ignorance of those who have laid out the farms. As a rule, the area of the land is quite insufficient for the population, and the method of distribution is also wasteful. For the sewage of the town of Romford, I already pay not far from 2s. per head of the population, and, I believe, the full value of the manurial constituents of sewage will be given by farmers at no distant day, as soon as they understand how to use it, which I am teaching them as fast as possible.

With regard to Mr. Stanford's allusion to Dr. Spencer Cobbold's views as to irrigation, I would only say that, although I have the greatest admiration for Dr. Cobbold's talent and indefatigable researches, and consider that as a helminthologist his authority is second to none, yet I do not feel in any way compelled to agree with him in these particular views, because he has not a single *fact* to adduce in support of them.—I am, &c.,

W. Hope.

Parsloes, January 22, 1871.

CHLORALUM AS A DISINFECTANT.

To the Editor of the Chemical News.

Sir,—My friend Dr. Ballard has done good service by stimulating discussion and investigation as to the nature of a disinfectant. He asks accomplished microscopists such as Dr. Beale and Dr. Sanderson to determine the destructive powers of reputed disinfectants on "those minute amoebiform particles of matter which constitute the simplest form of living things," and he calls for repeated and prolonged observation, of the use of any reputed disinfectant, in the arrest of the spread of contagious disease. No one can doubt the wisdom of these suggestions. Regarding, however, chloralum, permit me to

inform Dr. Ballard that it shrivels, arrests the movements, and kills the amoebiform particles above referred to, and it does more—it destroys many of the lower forms of parasitic life, whether animal or vegetable.

Dr. Ballard well knows how impossible it is to anticipate time and secure such co-operation as is required for "repeated and *prolonged* observations" relating to contagious diseases prevention.

Since I first thought of the chloride as an antiseptic, just a year ago, much work has been done in regard to the process of its manufacture and determining its chemical properties and physiological action.

In writing to the *Lancet*, last autumn, I published a number of general results based on careful observations, and asked for co-operation on the part of medical men to determine, as promptly as possible, how far the chloride of aluminium was available in medicine. I should have asked in vain if all had acted as Dr. Ballard has done, and not tried chloralum, notwithstanding "abundant opportunities of doing so." Fortunately it has been tried; several important facts have come to light and some have been published.

It was full six months after my first experiments with chloralum that I wrote a line about it, and then to the medical papers asking others to investigate and report. I cannot conceive a more desirable course to be adopted, when an agent, calculated to be universally available for sanitary purposes, presents itself to one who has for many years laboured at the subject of contagious disease prevention.

That carbolic acid is not universally available, I believe is so widely admitted, that I can leave it to be settled by medical opinion whether or not I am in error when I state that the odour has affected to a large extent the use of the agent, notwithstanding that it is, as I have frequently stated, "a most efficient agent for destroying contagia."

Very recently, the vague distinctions between antiseptics and disinfectants have struck me as quite unwarranted. I am now convinced that every good antiseptic is really a destroyer of disease germs. Tar and its compounds, arsenious acid, sulphuric acid, sulphates of iron and copper, sulphurous acid and sulphites, hydrochloric acid and certain chlorides, are all disinfectants if used in such a way that an antiseptic action, or, in other words, *an arrest of development*, is ensured. I consider this an important generalisation tending to "a grand clearance of our foggy notions about disinfectants."

Dr. Ballard is well aware that I do not rest in my work, and I trust he may, in future, honour me by deeming my suggestions—never made without thought and some research—of sufficient importance to merit being tested by experiment. Chloralum is harmless, and in virtue of its harmlessness can be used more freely than any other disinfectant in the sick room. For those who have not read what I have previously written on this subject it may be as well to add that the properties of chloralum are almost identical with the active *antiseptic and disinfectant* properties of hydrochloric acid. The aluminium base gives an available and innocent compound of this undoubted antidote to contagion.—I am, &c.,

John Gamgee.

January 23rd, 1871.

ANTISEPTICS AND DISINFECTANTS.

To the Editor of the Chemical News.

Sir,—Mr. Gamgee has been allowed to protest in your columns against Dr. Calvert's recently published experiments with antiseptics. You will no doubt permit me also to enter my protest, not only against those experiments, but likewise against Mr. Gamgee's assertions, wherein my name has been somewhat gratuitously introduced.

My main objection to Dr. Calvert's experiments is that he has classed permanganate of potash (Condy's fluid) among antiseptic agents, whereas that substance has in reality nothing in common with those bodies. There is no scientific warrant whatever for considering permanganate of potash to be possessed of "colytic" or preserving properties, except very indirectly. The disinfecting effects produced by the alkaline permanganates are due, not to antiseptic virtues, but to their oxidating powers. In this respect they resemble free air. You will, I think, agree with me that the latter agent, which is the one whereby ventilation operates as a disinfectant, is no antiseptic. One of the great aims in the preservation of food, for instance, is the exclusion of air. It would therefore seem to me that to class the permanganates with antiseptics, and to experiment with them as such, is much the same thing as to class in that category pure air and experiment with it in the apparent expectation that it might prove capable of preserving from decomposition organic bodies. I cannot see that any good would arise from proving that free air has no antiseptic value; but, on the contrary, believe that only misapprehension as to the value of that essential to health would thereby be engendered in the public mind. which is not yet sufficiently instructed to distinguish between antiseptics and disinfectants. In the same way, experiments undertaken to prove that permanganates are destitute of antiseptic properties, which everyone qualified to experiment already knows, instead of being of any service to sanitary science, must, on the contrary, when published, be detrimental thereto, by erroneously leading popular opinion to conclude that they are therefore inefficient disinfectants, which is exactly the contrary to the truth.

What I chiefly object to in Mr. Gamgee's protest are the following words:—"Condy's fluid, valuable in its way, has been supposed to act like carbolic acid, physicians prescribing it gladly in the erroneous belief that it added to its deodorising properties that of destroying disease germs. Its success has been due to its freedom from odour." In saying that Condy's fluid has been supposed to act like carbolic acid, Mr. Gamgee, I presume, knew by whom such a supposition has been entertained. Whoever he may have been, he was no great chemist, for hardly any two bodies could be more opposed in their nature and mode of action than Condy's fluid and carbolic acid. The former is an oxidating agent, whose effects are similar to those of ventilation by pure air; the latter is a deoxidating agent which, when added to pure air, vitiates it, inasmuch as it behaves exactly like animal respiration, robbing the atmosphere of oxygen. The one is in harmony with ventilation, the other antagonistic to it. The power to destroy disease germs which may be possessed by Condy's fluid is therefore precisely the same as that exerted by pure air.

Without needing to inquire into the *modus operandi* of ventilation—whether it be by oxidising and destroying the infectious emanations of the sick, or by acting on and removing the nidus or pabulum of disease germs, I feel confident that few, either among the advocates of chloralum, or the adherents of carbolic acid, will venture to dispute that, when sufficient, it is, in normal circumstances, an efficient means of natural disinfection. Those who are practically familiar with the uses of Condy's fluid, know that, as an artificial disinfectant, that preparation is almost to an equal degree efficacious. This, and not its mere want of odour, as Mr. Gamgee has been pleased to suppose, is "the secret" of that small measure of success which may have attended my labours of many years.—I am, &c.,

H. B. Condy.

Battersea, January 23, 1871.

Dr. Wagner's Chemical Technology.— The eighth edition of Dr. Wagner's well-known work on "Chemical Technology" is soon to appear. An English edition of this valuable work is in preparation, and will be published shortly after the German edition.

CHEMICAL NOTICES FROM FOR EICN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Pharmaceutische Zeitschrift für Russland, Nos. 10 and 11, 1870.

The only original paper relating to chemistry and allied sciences contained in these two numbers is the following:—

Researches on the Chemical Constituents of the Daphne Mezereum (Herb Laurel).—Dr. A. Casselmann.—Already noticed by us (see Chemical News, vol. xxii., p. 179), abstracted from another periodical.

No. 12.

Contains no original papers.

No. 13.

Contains a lengthy original essay on the—

Cellulose contained in Fungus.—Dr. E. Masing.—This exhaustive paper contains the following sections:—*Agaricus albus; Trametes suaveolens; Polyporus marginatus; Polyporus ignarius; Polyporus fomentarius; Russula alutacea; Polyporus albidus; Polyporus zonatus;* behaviour of the cellulose of fungus with various reagents; researches on the resin contained in fungus (*Boletus laricis*). This paper is too lengthy to admit of any useful abstraction, but it is a valuable contribution to the chemistry of cryptogamic vegetables.

No. 14.

Contains an original paper on—

Cumaric Acid, Hydrocumarine, and Hydrocumarinic Acid.—C. Zwenger.—This very lengthy essay treats, first, on the preparation of cumaric acid, $C_9H_8O_3$. This body is difficultly soluble in cold, somewhat more so in boiling water; crystallises in long needles; is readily soluble in ether and alcohol; fuses at 195°; may be sublimed by careful manipulation, but is decomposed by dry distillation, yielding chiefly phenol. The author describes at length a series of the salts of this acid, among which are the following:—Cumarate of barium, $(C_9H_7O)_2Ba+H_2O$, a crystalline substance which contains 1 molecule of water, which cannot be expelled by heat without decomposition of the salt; cumarate of lead, $(C_9H_7O_3)_2Pb$; cumarate of zinc, $(C_9H_7O_3)_2Zn$; cumarate of silver, $(C_9H_7O_3)Ag$. The second portion of this paper treats on hydrocumarine, and on hydrocumarinic acid; this latter body is a solid substance, difficultly soluble in cold, but more readily in boiling water; is crystalline; readily soluble in alcohol and ether. By the action of dilute acids, hydrocumarinic acid is converted into hydrocumarine, which is also formed when the acid is sublimed. The formula of hydrocumarinic acid is $C_{18}H_{18}O_6$; it forms, with bases, well-defined salts, many of which assume the crystalline form. Hydrocumarine is a solid crystalline body, which fuses at 222°, is almost insoluble in water, alcohol, and ether, and exhibits a neutral reaction to test-paper; it is best dissolved in chloroform, and yields, when treated with nitric acid, nitro compounds; its formula is $C_{18}H_{14}O_4$. The last portion of this essay treats at length on disalicyl-aldehyde, $C_{14}H_{10}O_3$.

No. 15.

Contains a lengthy paper on—

Cocculus Indicus and the Chief Constituents thereof, especially Picrotoxine.—W. Ganss.—This memoir contains the following sections:—Botanical and pharmacognostic description of the *Cocculus indicus*; on the use made of the berries; physiological action of the berries; general constituents of the berries (under this heading the author quotes the older analysis of this substance, made by MM. Pelletier, Couërbe, and others). The next section treats at length on the constituents of the berries, viz.:—Menispermine, $C_{18}H_{12}NO_2$, a solid crystalline body, which bears some resemblance to the bicyanide of mercury; it is a tasteless body, insoluble in water, but soluble in alcohol and ether; fuses at 120°; it is not, as far as experiments go, a substance which exerts any marked action upon animals. Parasperminе crystallises in quadrilateral rhombic prisms, fuses at 250°, and may be sublimed in sealed tubes; it is also insoluble in water, but readily dissolved by ether and absolute alcohol. Menisperminic acid, an as yet ill-defined body which, according to M. Boullay, exists in *Cocculus indicus*, but which neither M. Pelletier nor also M. Casaseca has found in that seed. Picrotoxine, discovered in 1812 by M. Boullay: The formula of this substance is, according to MM. Pelletier and Couerbe, $C_{12}H_7O_6$; while M. Oppermann gives to picrotoxine the formula $C_{12}H_6O_4$. The preparation of this substance from the berries is described by the author at great length; it may be briefly stated to consist in exhausting the previously pulverised berries with boiling alcohol, removal of the latter by distillation, taking up of the residue with boiling water, addition of a solution of acetate of lead to the aqueous extract, removal of the excess of lead from the filtrate by means of sulphuretted hydrogen gas, and re-crystallisation of the picrotoxine from the aqueous solution. Picrotoxine, in the pure state, is a crystalline body, which MM. Pelletier and Couerbe have called picrotoxinic acid, on account of its combining with oxide of lead forming a soluble compound therewith, which is, however, slowly decomposed by the action of the carbonic acid of the atmosphere. Picrotoxine acts as a sugar-containing body, reducing the hydrated oxide of copper, and assimilating, when boiled with dilute acids, water; it combines with bromine, $C_{24}H_{12}Br_2O_{10}$; with nitric acid it forms nitro-picrotoxine, $C_{24}H_{13}(NO_4)O_{10}$; and when distilled with caustic lime, it yields some metaceton. The larger portion of this paper is devoted to the exhaustive description of the physiological action of picrotoxine upon animals, which is continued in No. 16.

No. 16.

Contains no other original paper.

No. 17.

Contains a lengthy original essay on the—

Pharmacy of the Past (that is to say, a century and a half ago), and on the Present Condition thereof.—C. Frederking.

No. 18.

Contains an original paper on the—

Action, Nutritive Value, and Mode of Application of Extract of Meat.—Dr. E. Kemmerich.—The contents of this paper belong rather to the domains of physiology and therapeutics than to chemistry.

Zeitschrift für Chemie von Beilstein, No. 19, 1870.

The only original paper contained in this number is—

Acids which are Formed when the Hydrogen of Amides of the Toluol-Sulpho Acids is Replaced by Radicals of Acids.—Mdlle. Anna Wolkow.—This essay contains, in the first place, the statement that the amides $N(C_7H_7SO_2)H_2$ are, as far as their chemical behaviour is concerned, alcohols; in the second place, we meet with the description of the following substances:—Benzoyl-paratoluol-sulpho acid amide, $N[C_7H_7(SO_2)p](C_7H_5O)H$: This body is prepared by heating in an oil-bath, to a temperature of from 150° to 160°, a mixture of 33 grms. of paratoluol-sulpho acid amide and 28 grms. of chlorbenzoyl; the operation is continued as long as hydrochloric acid is given off. The reaction which ensues is represented by the formula—

$$N[C_7H_7(SO_2)p]H_2+C_7H_5OCl=N[C_7H_7(SO_2)p](C_7H_5O)H+HCl.$$

The resulting body is a crystalline substance, fusing at from 147° to 150°, readily soluble in boiling alcohol, but very difficultly so in cold alcohol, in ether, and in boiling water; this substance is an acid which expels carbonic acid from the carbonates of potassa, soda, lime, and baryta. The salts this acid forms with these bases are described, as is also the silver salt, $N[C_7H_7(SO_2)p](C_7H_5O)Ag$. Benzoyl-meta-toluol-sulpho acid amide, $N[C_7H_7(SO_2)m](C_7H_5O)H$, also a solid crystalline body, soluble in alcohol and ether, fusing at from 110° to 112°, this amide is also an acid. Several of the salts of this acid are described, and the lengthy formulæ thereof quoted. Succcinyl-paratoluol-sulpho acid amide, $N(C_7H_7SO_2m)(C_4H_4O_2)''$, prepared by heating 25·5 grms. of para-amide with 20·5 grms. of chlorsuccinyl first to 125° to 130°, and lastly to 150°. The result of this operation is the formation, in the first place, of monobasic acid, $N_2(C_7H_7SO_2p)(C_4H_4O_2)H_3$, readily soluble in boiling water, fusing at 180°. There is secondly formed a substance insoluble in water, a bibasic acid—

$$N_2(C_7H_7SO_2p)_2(C_4H_4O_2)H_2,$$

soluble in boiling alcohol, and crystalline. The salts and some of the products of the decomposition of these acids are described, accompanied with a series of very complex and lengthy formulæ.

No. 20, 1870.

The only original paper contained in this number is—

Question of the Existence of Antozone.—Dr. O. Loew.—This paper opens with an introduction containing a review of the various attempts made to explain the oxidising principle of oil of turpentine, and also the quotation of the leading ideas expressed on the nature of antozone. The author's chief object is to prove that the statement made by MM. Engler and Nasse (*Ann. Ch. Pharm.*, May, 1870), setting forth that antozone should simply be gaseous peroxide of hydrogen, is not quite correct. The results of a series of experiments made by the author, and described at great length, lead to the following conclusions:—(1) That the active principle in oil of turpentine is not ozone; because, when that body is shaken up with water, it does not yield peroxide of hydrogen. (2) That principle is, also, not peroxide of hydrogen; because, when an acid is not present, no reaction of iodine ensues (viz., when in contact with an alkaline iodide), while the formation of an acid in the oil of turpentine only takes place very slowly, and after considerable length of time. (3) There appears to exist a peculiar modification of oxygen which, when in contact with water, yields peroxide of hydrogen. This last modification of oxygen the author presumes to exist in the condition of free atoms, and he quotes the following formulæ for the various modifications of oxygen:—

[O].	[OO].	[OO]O.
Antozone.	Ordinary oxygen.	Ozone.

The active properties of oil of turpentine are explained by the author to be due to the presence in the oil of that kind of atomistic (*atomistischer*) oxygen, which we name antozone, being contained in the oil more in a state of physical than chemical combination, and, moreover, combined, or, at least, enveloped with heat (caloric). In a foot-note, the author states that, when pure oil of turpentine is left exposed to

sunlight for several weeks in a sealed tube along with dry oxygen gas, water is formed, which is observed in drops on the sides of the tube; it appears, also, that the oil alluded to, when in contact with water, yields directly peroxide of hydrogen.

No. 21, 1870.

This number contains the following original papers and essays:—

Dinitrobenzoic Acid.—D. Muretow.—The author prepares this acid by heating 100 grms. of nitrobenzoic acid with a mixture of 300 grms. of fuming nitric acid (sp. gr. = 1·48 to 1·49) and 600 grms. of oil of vitriol; the temperature should be about 140°. After having been purified, the dinitrobenzoic acid is obtained as a crystalline body, rather difficultly soluble in cold water, more readily so in boiling water; alcohol dissolves this substance easily. Dinitrobenzoic acid fuses at about 204°; it forms, with baryta and oxide of silver, crystallisable compounds. The formula of the former salt is $Ba[C_7H_3(NO_2)_2O_2]2+5H_2O$; that of the silver salt is $AgC_7H_3(NO_2)_2O_2$. Both these salts explode on being heated. By the reduction of dinitrobenzamide, the author prepared diamido-benzamide—

$$NH_2.C_7H_3(NH_2)_2O,$$

and describes several compounds thereof.

Azobenzol-Sulpho Acid.—Dr. Skandarow.—The body alluded to is prepared by heating azobenzol with 5 parts of strong fuming sulphuric acid (so-called Nordhausen acid); the substance thus obtained is a solid, orange-coloured, crystalline, and difficultly soluble in water. The potassa salt of this acid also crystallises; its formula is $C_{12}H_9(KSO_3)N_2+2H_2O$. The chloride of the azobenzol-sulpho acid is prepared by heating the potassa salt, previously dried at 165°, with pentachloride of phosphorus; this chloride is insoluble in cold water, and, by being boiled with that liquid, is slowly decomposed (viz., the chloride) into HCl and azobenzol-sulpho acid. The formula of the chloride is $C_{12}H_9(SO_2Cl)N_2$. The composition of the amide of the azobenzol-sulpho acid is $C_{12}H_9(SO_2.NH_2)N_2$.

Notices.—P. Alexejeff.—Under this title, the author quotes several minor matters, among which are the following:—Azonaphthaline and Nitrophthalen.—The percentical composition of the former is C, 85·1; H, 4·96; that of the latter, C, 85·1; H, 4·8. The only difference between these two bodies is that sulphuric acid colours azonaphthaline red, and the other blue. Bromated acetylen may be prepared by adding to solid sodium alcoholate, placed in a flask, drop by drop, $C_2H_3Br.Br_2$; the result is the formation of a spontaneously-combustible gas, which is to be collected over mercury, while the generation of acetylide of copper is readily exhibited by introducing into the gas a strip of paper moistened with a solution of ammonio-chloride (proto) of copper.

Hydrates of Oxychloride of Magnesia.—Dr. C. Bender.—This essay contains the results of a series of researches made with the view to elucidate the scientific principles involved in the formation of the so-called Sorel magnesia cement. The author describes at length the following substances:—$(MgCl_2+5MgO+17H_2O)$; that is to say, a piece of the cement alluded to, which had been made six months previous to the analysis, and had become quite hard. If this substance is treated with water, a portion of the chloride of magnesium is eliminated, and there remains, after drying over SO_3, a compound, $(MgCl_2+9MgO+24H_2O)$. By being treated with boiling water, this compound is entirely deprived of its chloride of magnesium, and there remains, after drying over SO_3, a hydrate of magnesia, $(2MgO+3H_2O)$. All these compounds are as hard as good sandstone, take a good polish, and are not disintegrated even by boiling water.

Hydrate of the Nitrate of Oxide of Uranium.—C. Schultz-Sellack.—While evaporating the solution of the nitrate alluded to with a large excess of nitric acid, the author obtained, on cooling, beautifully-fluorescent crystals, $UONO_3+1\frac{1}{2}H_2O$, or $2UONO_3+3H_2O$.

The Hydrate of Chloracetic Aldehyde.—G. Glinsky.—The author describes, at great length, the preparation of this substance, which is a rather complicate proceeding. The formula of the hydrate alluded to is $C_2H_3ClO+\frac{1}{2}H_2O$. It is a solid body, which fuses at between 64° and 65°; is volatile when exposed to air, becoming simultaneously oxidised; in dilute state, it gives off a smell of rotten apples, and, when concentrated, its vapours are very irritating, while it burns the skin of the hands like nitric acid; it reduces ammoniacal silver solution.

Gmelin's Work on Chemistry.—Dr. K. Kraut.—From a short notice of the editor of this periodical we learn that the author just mentioned has edited the few of the hitherto unfinished parts of Gmelin's work, including an excellently-arranged and very complete general index. The publisher is M. K. Winter, at Heidelberg.

NOTES AND QUERIES.

Amount of Tannin in Valonia.—The present prices of valonia are as follows:—Greek, £14 17s. 8d. per ton; Smyrna, £16 18s. 10d. per ton. I have recently examined three specimens of each kind, sold as first quality, and I find that the average amount of gallotannic acid in Smyrna valonia is 31·60 per cent, and in Greek valonia 31·12 per cent. Why is there so great a difference in the price of the two varieties?—CHARLES A. CAMERON, M.D., Dublin.

Solubility of Tin in Nitric Acid.—A friend of mine, a gentleman of undoubted scientific eminence, whom I met just this morning in the street, has informed me that Proust mentions the fact of the solubility of tin in "very dilute nitric acid with the production of nitrate of ammonia." I therefore renounce all claim to the discovery, so far as regards mere priority. There is, however, one circumstance in which my discovery differs from that of Proust; viz., that the acid employed by him was very dilute, while mine was very strong, being a mixture of equal volumes of the monohydrated acid and water. To speak as Mr. Gibbins does of the "evolution of ammonia" in presence of nitric acid is simply to talk nonsense, and to say that such a result "might have been expected" is to make nonsense more nonsensical. Honour to whom honour is due—I yield to Proust; although certainly for me the discovery was as real as if Proust had never existed; indeed, I have not yet seen his book or paper.—GEORGE HAY.

Solubility of Tin in Nitric Acid.—Mr. Hay has asked any of your readers to point out where it has already been mentioned that tin is soluble in nitric acid. In most of the modern text-books on chemistry, the action of nitric acid is described as being confined to the production of stannic oxide; however, in the tenth edition of Henry's "Chemistry" (1826), a statement is made which appears to show that it was known that, under certain conditions, tin is soluble in nitric acid. It will be better to transcribe the whole passage, which is taken from vol. ii., p. 43:—When nitric acid, highly concentrated, is poured upon tin filings, very little effect is produced; but when a small quantity of water is added, a violent effervescence follows, and the metal is reduced to a bulky powder, which is the white oxide retaining a little acid. If more water be added, an acid liquor is obtained holding very little tin in solution, and containing nitrate of ammonia, the alkaline base of which is formed by the simultaneous decomposition of the water and nitric acid, and the union of the hydrogen of the former with the nitrogen of the latter. Tin, however, is slowly dissolved, without effervescence, in nitric acid greatly diluted. The solution is yellow, and deposits tin by keeping." The two last sentences will, I think, be a sufficient reply to Mr. Hay. His attention may also be drawn to a paper by Mr. John M. Ordway, in the *Chemical Gazette*, vol. xv., p. 427, where it is stated that "if flattened tin be put into very weak nitric acid, say of sp. gr. 1·15, a small quantity is quickly taken up, but, in a short time, is thrown down again as a white powder containing no nitric acid." It is very likely other quotations of a similar kind could be found here and there in the periodical literature, and, if omitted from the more modern text-books, it is, perhaps, owing to ignorance of the most favourable conditions for the solution to take place. In regard to the occurrence of ammonia during the action of nitric acid on tin as noted by Mr. H. B. Gibbins, the above quotation from Henry will show that it has already been observed. The same fact is mentioned by Muspratt (see article on "Tin," vol. ii., p. 1037).—EUREKA.

MEETINGS FOR THE WEEK.

MONDAY, Jan. 30th.—Medical, 8.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology. (Educational Course.)
TUESDAY, 31st.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
WEDNESDAY, Feb. 1st.—Society of Arts, 8.
—— Pharmaceutical, 8.30. Dr. W. B. Carpenter, F.R.S., "On the Microscope and its Revelations."
THURSDAY, 2nd.—London Institution, 7.30. Mr. F. S. Barff, M.A., F.C.S., "On the Action, Nature, and Detection of Poisons."
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— Chemical, 8. Dr. Frankland, "On the Development of Fungi in Potable Water."
—— Royal, 8.30.
—— Royal Society Club, 6.
FRIDAY, 3rd.—Royal Institution, 9. W. Spottiswoode, F.R.S., "Some Experiments on Successive Polarisation of Light made by Sir C. Wheatstone."
—— Geologists' Association, 7.30. Anniversary.
SATURDAY, 4th.—Royal Institution, 3. Rev. W. H. Channing, "On Laws of Life Revealed in History."

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Student.—The editor is engaged on a work on the subject you name; it will shortly be published.

E. W. T. Jones.—Your letter has been referred to the secretary of the Chemical Society, who will, probably, send you the information.

R. Thomson.—You will find full particulars in our last Student's Number, published in September.

J. Mercer.—Mr. Baldwin's book you can obtain through your bookseller; the publishers are Brown and Nolan, Dublin. The other book you must order from a foreign bookseller.

W. H. Wood.—The experiments on the "Synthesis of Indigo-Blue" are not yet completed; we shall give full particulars as soon as the experiments are published.

F. H. T. A.—Dussauce has written a work "On Soap and Candle Making," in which fats and oils are fully treated of. It is at the library of the Commissioners of Patents.

W. M. Watts.—Received.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 584.

ON THE SPECTRUM OF THE BESSEMER FLAME.

By W. MARSHALL WATTS, D.Sc.

A recent number of the Chemical News contains a paper by Mr. J. Spear Parker, in which he gives reasons for doubting the theory, maintained in the previous numbers by Mr. J. M. Silliman, that the lines of the Bessemer spectrum are caused by manganese, and not by carbon. Mr. Silliman says, "Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum, found the lines in the blue and green fields to completely harmonise in the two spectra." I am not acquainted with the research by Lichtenfels referred to, but, if such a coincidence has been established, manganese must be capable of giving two quite different spectra, since it is impossible to conceive spectra more widely different than the Bessemer spectrum and the spectrum of manganese as given by Huggins and Angström. The brightest lines of the manganese occur just in the part of the spectrum in which, according to Mr. Silliman's own observations, no lines are given by the Bessemer flame.

I can confirm Professor Lielegg's statement, that the Bessemer spectrum is produced also when coke is burned in the converter. I have never failed to see it, either in the first "blow" after re-lining, or at any other time.

Further, the Bessemer spectrum, with all its principal lines (though not very brilliant), can be obtained by burning coke with a blast of air in a re-heating furnace not containing iron or iron-slag; it is incredible that, in this case, the spectrum should be produced by manganese. In view of this observation, and the observation of Mr. Parker, there cannot, I think, remain the faintest hesitation in attributing the Bessemer spectrum to carbon or some carbon compound.

Mr. Silliman quotes from my paper on the Bessemer spectrum, but attributes it to Professor Roscoe. I desire to explain that Professor Roscoe is in no way responsible for any of its statements, and in particular for the statement that the hydrogen line occurs, at times, as an absorption-band.

I do not remember ever observing the flame without seeing the sodium line momentarily as an absorption-band.

The occurrence of the three iron lines in the Bessemer spectrum, which Mr. Silliman does not consider has been satisfactorily established, is the one most certain result of my experiments. In an actual comparison of the Bessemer spectrum with the spectrum of the electric spark between iron poles in hydrogen, these three lines were observed to be absolutely coincident in the two spectra; and in a second simultaneous comparison of the Bessemer spectrum with that of sunlight, these lines were seen to be coincident with Fraunhofer lines marked by Kirchhoff as due to iron.

THE ISOMERS OF AMYL.

By HERBERT McLEOD, F.C.S.

Notwithstanding Mr. Loew's reply to my criticism of his previous communication, I still adhere to the opinion that his two formulæ, Nos. 4 and 9—

$$\left.\begin{matrix} H_3C \\ H_3C \\ H_3C \end{matrix}\right\} C \Big\} C \qquad \text{and} \qquad \left.\begin{matrix} H_3C \\ H_3C \end{matrix}\right\} C \Big\}'' \begin{matrix} H_3 \\ \\ H_2 \end{matrix} \Big\} C \Big\} C$$

are identical; that is, they are so if the four atoms of hydrogen in marsh gas are of equal importance. If these atoms are of different values, then Mr. Loew's estimate of the number of the isomers of amyl is very much too low; for, as I endeavoured to show, the number of ethyls is at least thirty, on the assumption that the four attractions of carbon differ from one another (which is the same in effect as assuming the four atoms of hydrogen in marsh gas to be of different values), consequently, the number of isomers of amyl would be enormously larger.

Mr. Loew insists that in his two formulæ the three atoms of carbon, each of which is united to three atoms of hydrogen, are in different positions; but this differentiation seems merely a typographical one. In chemical compounds, we can only form a notion of the positions of elements from the manner in which they are combined amongst themselves, and not from the way we may please to write them on paper; this would be a lamentable abuse of chemical formulæ. But these three atoms of carbon are in perfectly similar positions, each being united to three atoms of hydrogen, and all of them with the same atom of carbon, this last atom being united to CH_2. This description is equally applicable to the two formulæ, notwithstanding their apparent dissimilarity.

Mr. Loew points out that M. Lwow, of St. Petersburg, has obtained an isomer of amylic hydride, by the action of the iodide obtained from trimethyl-carbinol on zinc methyl, and suggests that another isomer would probably be obtained by treating the chloride of aceton with zinc methyl. But, if these two hydrides were found to be identical, what would become of Mr. Loew's argument? A few clearly-established isomerisms of this kind would go far to show that the four attractions of carbon are of different values; but, until such is proved to be the case, chemists may be excused from further complicating their formulæ by labelling the four atoms of hydrogen in marsh gas.

In conclusion, either Mr. Loew believes that the four attractions of carbon are of equal value, in which case he has over-estimated the number of isomers of amyl, eight only being possible; or he believes they have different values, in which case he has enormously under-estimated their number, and I will gladly leave the calculation in his hands.

Royal College of Chemistry, London,
Jan. 24, 1871.

NEW VOLUMETRIC METHOD FOR THE ESTIMATION OF COPPER.*

By M. F. WEIL.

The well-known fact that there has been hitherto no thoroughly reliable method for the estimation of copper in ores and alloys, and also when mixed with other metals, which interfere with the estimation of the copper, induced me to seek for a mode of operation which answers well in practice and is readily executed. It is as follows:—

The principles upon which my method is based are:—(1). That with an excess of free hydrochloric acid, and at boiling heat, the presence even of the smallest quantity of chloride of copper may be detected by the greenish yellow colour of the solution, this colour being the stronger and more prominent the more free hydrochloric acid prevails. (2). The aqueous solutions of chloride of copper which contain free hydrochloric acid become, while boiling, upon the addition of protochloride of tin, instantaneously converted into colourless solutions of protochloride of copper, according to the formula $2CuCl + SnCl = Cu_2Cl + SnCl_2$. The reaction is quite finished as soon as, by the addition, drop by drop, of the protochloride of tin, the green colour of the perchloride of copper is changed to the colourless

* Communicated by the Author.

protochloride of copper. Even a single drop of the protochloride of tin added in excess can be readily detected by the addition of a single drop of a solution of corrosive sublimate, which produces a precipitate of white protochloride of mercury. It is clear, therefore, that the quantity of the solution of protochloride of tin which is required for the perfect decolouration of the copper solution will indicate the quantity of the copper present in the solution, which, if it contains iron along with the copper, the iron will have to be estimated in a separate portion of the solution by means of permanganate of potassa; while, in the estimation of the copper, the quantity of protochloride of tin, equivalent to and converted by the iron into perchloride of tin, has to be deducted.

I. *Preparation and Keeping of the Solution of Protochloride of Tin.*—Take about 6 grms. of tin-foil (this material ought first to be tested for its purity), dissolve in 200 c.c. of pure hydrochloric acid at a boiling heat, and with the addition of some pieces of platinum wire; the tin solution thus obtained is diluted to 1000 c.c. by the addition of distilled water, and is poured into a wide-mouthed glass bottle, after which the solution is covered over with a layer of petroleum. This bottle is provided with a glass syphon-tube and stop-cock attached, and also with a funnel-tube for replenishing the solution, which, although kept under petroleum, and thereby protected from the action of the air, only keeps sufficiently steady and unaltered for a single day, so that the liquid has to be tested and accurately titrated every morning before using it for the estimation of copper in solutions wherein the quantity of that metal is unknown.

II. *Titration of the Protochloride of Tin for Pure Copper.*—Take chemically pure, re-crystallised, pulverised sulphate of copper deprived of adhering moisture by pressing between filtering-paper; weigh off 7·867 grms. (equal to 2 grms. of pure metallic copper), dissolve in distilled water, and dilute to 500 c.c.; keep this solution as normal solution of copper in a glass-stoppered bottle. Take of this solution, by means of a pipette, 25 c.c. (equal to 0·1 grm. of pure copper); pour this quantity into a flask capable of containing 100 c.c.; add 5 c.c. of pure hydrochloric acid, which causes the blue fluid to become deep green; and next boil the liquid upon a sand-bath; then take a burette, divided into c.c. and 1-10th c.c., fill it with the solution of protochloride of tin up to zero, and immediately after pour rapidly into the boiling copper solution sufficient of the tin salt to cause the same to become very nearly decolourised. After this, the tin solution is added, drop by drop, until the copper solution is as clear and colourless as distilled water is; as soon as this point has been reached, add with a pipette again 5 c.c. of pure hydrochloric acid, and, if by this addition the slightest green colouration is produced, protochloride of tin is added, drop by drop, until the decolouration is complete; after this, the quantity of tin solution employed is read off. If it is desired to make sure that the end of the reaction is properly reached (a precaution which is quite unnecessary if 10 c.c. of hydrochloric acid have been added), 1 c.c. of the colourless solution is taken, by means of a pipette, and poured into a test-tube; this tube is placed in cold water, to promote the rapid cooling of the contents; and, after cooling, there is added to the solution one drop of a concentrated aqueous solution of chloride of mercury. If this does not bring about a turbidity or precipitate, it is best to add to the contents of the flask another drop of protochloride of tin solution; this having been done, the testing of the contents of the flask by means of chloride of mercury, as just mentioned, is repeated; and if a turbidity or precipitate ensues, the quantity of tin solution applied is read off, care being taken to deduct therefrom 1-20th of a c.c. Every 16·2 c.c. of the tin solution correspond to 0·1 grm. of metallic copper.

III. *Example of Titration of any Compound of Copper not containing either Iron or Nickel.*—Take 4 grms. of the metal, either reduced to powder, or at least cut (not filed) to a convenient size; dissolve in strong nitric acid contained in a long-necked flask; expel the excess of this acid by boiling with excess of sulphuric acid, or with hydrochloric acid, in case silver is contained in the substance taken for assay; next, water is added, and the bulk of the fluid brought to from 250 to 500 c.c., according to the presumable quantity of copper present, care being taken to mix the liquid thoroughly, and thereby render it uniform throughout. It is not absolutely required to remove by filtration insoluble substances, such as silica, sulphate of lead, stannic acid, antimonic acid, chloride of silver, and the like, since these substances settle readily to the bottom of the tall cylindrical jar employed for containing the liquid, while the assay is being made. The tin solution is added after the addition, as above stated, of some 5 or 10 c.c of hydrochloric acid. Example:—Two grms. of an alloy containing much copper were made into a solution of 250 c.c.; 25 c.c. thereof having been taken for assay required 25·42 c.c. of protochloride of tin solution, of which 16·2 c.c. corresponded to 0·1 grm. of pure copper. The 25 c.c. of the solution of the alloy taken for assay contain, therefore, 0·1569 grm. of copper, because 16·2 : 25·42 = 0·1 : 0·1569; consequently the quantity of copper contained in 250 c.c. is = 1·569 grm., because 0·1569 × 10 = 1·569; and therefore 100 grms. of the alloy contain, according to the following equation:—

$$1{\cdot}569 \times 50 = 78{\cdot}45 \text{ per cent of copper.}$$

IV. *Titration of a Compound of Copper which also contains Iron.*—The weighing off of the substance, its solution in acid, and the titration of 25 c.c. of the joint solution of the two metals, is performed as described under III., with this difference, that the operation of adding the tin solution is performed with the solution of the metals contained in a flask of a capacity of at least 250 c.c. After the total quantity of tin solution required at the first titration has been noted, the titration of the iron is executed in the following manner:—Some 25 or 50 c.c. of the sulphuric acid solution are diluted with a large quantity of water, and placed in a flask of 250 c.c. capacity; to this liquid, metallic zinc and platinum wire are added, and left in the solution until it is perfectly colourless; the entire quantity of copper, tin, lead, arsenic, antimony, &c., which might be present is precipitated in the metallic state; the colourless fluid is then decanted, and the iron volumetrically estimated by means of permanganate of potassa. Since perchloride of iron, as well as the chloride of copper, are reduced by protochloride of tin, according to the following formulæ—

$$Fe_2Cl_3 + SnCl = 2FeCl + SnCl_2, \text{ and}$$
$$2CuCl + SnCl = Cu_2Cl + SnCl_2,$$

it is evident that the quantity of protochloride of tin which corresponds to the iron is found by simple calculation from the experiments made, as will be best illustrated by the following example:—Four grammes of a copper ore, which also contained iron, were dissolved, and the solution made to 250 c.c.; of this solution, 25 c.c. were taken for assay, and found to require 26·75 c.c. of protochloride of tin solution of 16·2 litre. By titration with permanganate of potassa, 25 c.c. of the solution under assay were found to contain 0·0809 grm. of iron. The protochloride of tin solution employed corresponds with 18·34 c.c. for 0·1 grm. of iron; therefore—

1 equiv. copper : 1 equiv. iron = copper : × iron.
31·7 : 28 = 0·1 : 0·0883.

Consequently, 0·1 grm. of copper corresponds to 0·0883 of iron, and likewise to 16·2 c.c. of protochloride of tin; on the other hand, 0·1 grm. of iron corresponds to 18·34 c.c. of protochloride of tin, because 0·0883 : 0·1 = 16·2 : 18·34 c.c. protochloride of tin; therefore, again, 0·0809 grm. of iron requires 14·837 c.c. of protochloride of tin, because 0·1 : 0·0809 = 18·34 : 14·837.

Calculation of Results.

	Protochloride of tin. c.c.
For copper and iron together	26·750
Deduct for iron alone	14·837
Leaves, for copper..	11·913

25 c.c. of the solution therefore contain 0·0735 grm. of copper, because 16·2 : 0·1 = 11·913 : 0·0735. 4 grms. of the ore = 250 c.c. contain, accordingly, 0·735 grm. of copper, and 100 grms. of the ore contain, therefore, 18·38 per cent of copper.

Observation on the Titration of Compounds of Copper containing Iron.—If the operator does not happen to have ready at hand a titrated permanganate solution for the estimation of the iron, the titration of the fluid containing copper and iron can be readily performed in the following manner:—Precipitate, as above mentioned, by the aid of zinc and platinum, all the copper, &c.; decant the supernatant fluid; wash the precipitated metal (or metals) with distilled water, and dissolve these in sulphuric acid; and estimate, next, in 25 c.c. of that solution (after previous addition of from 5 to 10 c.c. of pure hydrochloric acid), by titration with protochloride of tin solution, the copper directly without the necessity of determining the iron at all.

V. *Titration of a Compound of Copper which contains Nickel.*—Since the green colour of the salts of nickel prevents the complete and perfect decolouration, by protochloride of tin, of a solution which contains copper and nickel together, the titration by means of the tin salt, as described, can be applied, but the end of the reaction must be tested for by the use of the solution of corrosive sublimate. In preference, however, to this I employ the following method:—The substance to be tested (about 4 grms.) is dissolved in nitric acid, or, if need be, in nitro-hydrochloric acid; next, the greater part, but not all, the acid is saturated with carbonate of soda; then add, after having diluted the fluid with cold water, an excess of freshly-precipitated carbonate of baryta, to which some chloride of ammonium is added, and which together are suspended in water. This milky fluid is thoroughly stirred through the metallic solution; by this proceeding, a precipitate ensues, which contains all the copper as hydrated oxide and (if present) the iron also as hydrated oxide, while the nickel remains in solution. The precipitate is first washed by decantation, next collected on a filter, thoroughly washed, and, after having been re-dissolved in hydrochloric acid, titrated as above described. The presence of arsenia or its compounds does not in the least interfere with the process; while, if cobalt happens to be present, which will, however, be only rarely the case, the treatment is the same as for nickel. By a series of test experiments, and by comparison with gravimetrical analysis, the author states that he has found the process above described to be thoroughly reliable and to give accurate results.

ON THE CHEMISTRY OF THE BESSEMER PROCESS.

The following is an abstract of the paper on the chemistry of the Bessemer process, read before the American Association for the Advancement of Science, at its Troy meeting, by Lieut. C. E. Dutton, U.S.A.:—

After some general statements concerning the chemical composition of crude pig-iron, Lieutenant Dutton passed to discuss the theoretical changes possible when air is brought in contact with it in a melted state. According to his view, the silicon is first oxidised, then the phosphorus, next the manganese and sulphur, and, lastly, the carbon. Chemically, there is nothing new in the Bessemer process. It may be said to be "the employment of entirely new mechanical methods and appliances for effecting old and familiar reactions."

The process itself was then minutely described. Since the sulphur is but partially removed and the phosphorus not at all, the iron selected must be free from these substances. It must also contain at least two per cent of silicon. This iron is melted in a cupola furnace, run into the converter—a charge being 12,000 pounds—and the blast turned on. "The first change is in the oxidation of iron and silicon. The silicon becomes silicic acid, and enough of the iron oxidises to satisfy the affinities of the acid, and does not decompose during the remainder of the blast. It is during this stage of the conversion that the remarkable heat of the conversion is developed. It will be remembered that when silicon oxidises, it takes up three equivalents of oxygen. Carbon takes up only one in this process, becoming carbonic oxide. It is a common error to suppose that any very great quantity of heat is generated by the combustion of the carbon, that is, as compared with that derived from the silicon. . . . . The heat generated by the silicon burning to silicic acid will be found, by the application of the coefficients and formulæ of the mechanical theory of heat, to be from two and a-half to three times greater than that generated by the burning of the carbon to carbonic oxide. Another circumstance of importance is that the silicic acid remains as a dense fluid in the converter, no part of its heat being lost, except such as is carried out of the converter by the atmospheric nitrogen; and none is rendered latent by converting it into vapour, while the carbon is vapourised, a physical change absorbing much heat, and the vapour thus formed is carried out of the converter at a very high temperature. Hence will be seen the necessity of employing irons containing high percentages of silicon. At least two per cent of this element is essential, any less quantity being insufficient to generate heat enough to keep the iron thoroughly liquid and fluent until the end of the casting process. It is often asserted that irons for Bessemer conversion must be 'grey irons,' as they are called, *i.e.*, irons rich in carbon. Now, although this happens, as a rule, to be true enough, it is apt to lead to misapprehension. The fact that Bessemer pig-irons are carbonised varieties is an accident, and not an essential feature. What is essential is that it should contain a large quantity of silicon, and very little—indeed, the least possible—of sulphur, phosphorus, and manganese. Now, a pig-iron containing much silicon and no sulphur, or manganese, is pretty sure to contain a high percentage of carbon, as all smelters are aware. This fact is a feature of the blast-furnace, and almost without exception. If an iron could be produced with much silicon, a little carbon, no phosphorus, it would, I think, be not altogether unsuited to the Bessemer treatment. In a word, the quantity of carbon is approximately immaterial, except so far as it implies proper conditions with respect to other elements. The main element required is the silicon and not the carbon."

Of phosphorus, "the arch-enemy of the iron-maker, but the very scourge and pestilence of the steel-maker,"—fifteen thousandths of one per cent ruining Bessemer metal past all remedy—Lieut. Dutton said:—"I have already ventured the opinion that phosphorus increases its affinity for iron with every increase of heat; at least relatively if not absolutely. The fact seems to be absolutely. If we accept it, we can instantly explain what seem, otherwise, to be many anomalies and contradictions. It will explain to us why, in the great heat of the blast-furnace, it leaves every other combination and enters the iron; why, in the much lower heat of the puddling furnace, it seems to waver between staying with the iron, or forming a new alliance with oxygen, ready to choose either, at the influence of any third substance which may affect the question; why in the Bessemer process it clings to the iron with a desperate tenacity which nothing seems able ro resolve. These three facts, then, are all of them formidable: (1) that a minute quantity of phosphorus is capable of working

terrible injury, and that it is omnipresent throughout nature; (2) that whatever quantities of it are charged into the blast-furnace, as fuel, flux, or ore, are almost wholly concentrated in the resulting pig-iron; and (3) that no portion is eliminated in the Bessemer converter."

The sudden change of the flame at the close of the conversion Lieut. Dutton thus explained:—

"When two combustibles are intermixed, like oxygen and hydrogen, or hydrocarbon gas, it is well known that the relative proportion of the two elements in the mixture influences the readiness with which they combine. Thus oxygen and hydrogen cannot combine explosively unless their proportions lie within certain definite and rather narrow limits. May not the same law hold good in the present case? It is certain, or nearly certain, that the iron either does not oxidise in the bath during the blow, except in quantity sufficient to furnish a base for the acids present; or, if it oxidises beyond that, it is immediately reduced again, leaving little or no free oxide of iron in the bath. But after the change of flame all this is reversed, and iron oxidises rapidly and freely, and remains undecomposed, while the residual traces of the other elements as suddenly cease to oxidise rapidly. I freely grant that in referring this back to what is supposed to be a conceded, but unexplained law, we are merely putting the question in another, a more general, and more abstract shape—still it is, in a qualified sense, an explanation."

The theory of the action of the spiegeleisen, run in after the blast is stopped is next discussed, and also the quality of the metal produced, which Lieutenant Dutton calls a "cast wrought-iron."

The paper closed with some comparisons of Bessemer with other metal, and a discussion of the uses for which Bessemer metal is most valuable."

AN EXAMINATION OF GROVE'S ARGUMENT FOR AN INVARIABLE MECHANICAL EQUIVALENT OF HEAT.

By the Rev. H. HIGHTON, M.A.

In his "Correlation of Physical Forces" (ed. 1867), Grove urges the following argument for an invariable mechanical equivalent of heat. Not having his book at hand while I write, I give his argument in my own words, but I believe with perfect fairness.

If, says he, two different gases by the application of equal quantities of heat could raise different weights, we should have the following anomaly. Suppose "a definite source of heat (say a pound of mercury at 400°)" were applied to each of two gases, each working a piston, and they were to be set to work one against the other; then, if they exerted unequal pressures, the one must expand and compress the other; consequently, the one which was compressed must evolve heat, and would by such evolution of heat raise the heat of its pound of mercury to, say, 401°. Thus, says he, we should have the anomalous result that one pound of mercury at 400°, could raise another pound from 400° to 401°.

This I believe to be a perfectly fair representation of his argument.

In answer to this, let me observe, that if one gas or vapour when expanded by a given amount of heat will raise 10 lbs., while another by the same amount of heat will raise only 1 lb., and if this be a real fact, a fact established by our best experimenters in observing the specific heat, and rate of expansion, and elasticity of vapours and gases, it cannot be the slightest use to raise theoretical and imaginary difficulties to prove, theoretically, that such a fact is impossible. Theories must be squared by facts, not facts by theories.

But let us see the fallacy of the argument.

To say nothing of the fact that a pound of mercury at 400° cannot strictly be said to be "a definite source of heat," that is, the source of a definite amount of heat, without knowing the circumstances of the body with which it is to be brought into contact, Grove seems to suppose that the first pound of mercury heats and expands a gas or vapour without losing any of its own heat. He supposes it still remains at 400°. But if it sinks to 398° in expanding and heating the gas to which it is applied, what anomaly, or even difficulty, is there in supposing that while it sinks to 398° it may raise another pound to 401°?

It is just what we should expect. Steam at 100° C., condensed in a solution of chloride of calcium will raise it to a much higher temperature than 100°.

Now, here is another nut to crack for Mr. Grove, or any other advocate of an invariable mechanical equivalent of heat. Let him calculate the specific gravity, the specific capacity for heat, the expansion by heat, and the elasticity or compressibility of mercury and water. I think this calculation (unless I am much mistaken) will show that a given quantity of heat applied to expand mercury will raise seven times the weight that it will if applied to expand water.

The idea of an invariable mechanical equivalent of heat has taken such deep root in the scientific world, that it will not be easy to extirpate it; but till it is extirpated, together with the supposed axiom of conservation of energy as applied to mixed questions of mechanics and other sciences, it will be a continual stumbling block to true progress.

What is each individual life, vegetable or animal, but a centre for the evolution of force, by disturbing or restoring chemical, electric, calorific equilibrium, by burning and unburning; each of the two kinds of life, generally speaking, working against one another, the one undoing what the other does? And what limit is there to life? The only limit in nature to the production of motion (in the largest sense of the word) is that whatever amount of motion in a positive sense be produced, an equal amount must be produced of motion in a negative sense. But both may be utilised for work to subserve man's convenience.

Putney, Jan. 16, 1871.

HISTORY OF THE COLOURS PRODUCED FROM COAL-TAR.

By H. BOCK BINKO.

It is admitted in the scientific, as well as the industrial world, that the German chemist Runge, of Oranienburg, was the first to discover the production of colour from coal-tar, and the year of this discovery is 1837.

At a later period only, were English, French, and German chemists successful in producing the splendid aniline colours, which are hardly to be excelled anywhere.

We are indebted for aniline itself to Hofmann and Fritzsche since 1840. Aniline-violet was then produced by Perkin, and introduced into colouring in 1856, aniline-brown was discovered by Laire in 1861, aniline-orange by Mène in 1861, aniline-blue by Persoz, in 1862, aniline-black by Lucas, in 1863, aniline-green by Isébe, and aniline-brown by Nicholson.

Now followed year after year further discoveries. Aniline oils were produced and again aniline colour factories were erected, and at present it has been brought to such perfection that colours have been extracted from aniline, napthaline, and anthracene, and even from phenol, so that coal-tar colours may be counted by the dozen.

This production may seem surprising, but it is yet

exceeded by the fact that similar colours were discovered in 1816 by a single man, and had been practically applied by him in dyeing.

The compiler of these lines hopes that he will be able to throw a light on this important subject, and also to forward the writings of this great but unknown author, so that the world may be enabled to appreciate the proofs of his activity.

The *Neue Freie Presse* has taken notice of the same in No. 2005 (trade and industrial news). Even to-day this discussion is not by any means concluded, although that which has been already brought to light, may cause universal interest to be centred therein.

Johann Nepomuk Jassnüger, as this discoverer was named, wandered from his birthplace, Puchow, in the Trentschiner district, in Hungary, as a poor student to Vienna; obtained there, by dint of unexampled diligence, an excellent physical and chemical education; became doctor of medicine, and, from 1802, professor of chemistry at the Imperial Theresian Knight-Academy in Vienna, where he died in 1827. To the kindness of his son-in-law, the notary, Dr. Olschbauer, we are indebted for the few written notes from Jassnüger's own hand, and which great discovery will always remain preserved by him as private property.

They are as follows:—The first document relates to an application to the Chamber of Commerce for a patent.

"After many repeated and wearying chemical researches, the undersigned has been successful in extracting from coal and turf a substance for colouring, and not only galls, campeachy wood, sumach wood, and other materials necessary for colouring may be added to some foreign colouring materials, and also those used in the preparation of black inks. Still it is needless to add that all these articles may be entirely dispensed with through the above discovery.

"By means of this colour, extracted from coal and turf by the undersigned, it is possible to colour just as lasting and beautiful in less time and expense than is necessary to colour black, with the usual means, all sorts of wool, silk, cord (flax and hemp), prepared spun stuffs and cloth, all sorts of hats, whether made from beaver, hare, or rabbit-hair, or whether prepared from sheep's wool. This new colour, which the undersigned desires to give the name of Vienna black, has, besides the properties already described, one particular quality, that is, that the blue ground in colouring black substances can be dispensed with, by which means a very important quantity of indigo can be spared, and that the well-known prejudicial effect known under the name of burning, and which is noticed in the application of acids, is disposed of with reference to the above on the strength and lasting properties of cloths.

"Black ink can also, by means of this new colour, be prepared to the darkest tinge of black in a very cheap and quick manner without boiling. This ink has, then, the particular quality that mildew never settles upon it, and it thereby does not so soon become spoilt as in the case of the usual inks.

"But not only in black-colouring, but by all other colours where acids have been applied, is this colouring matter from turf and coal designed to take its place, but those substances used in the colouring of brown, green, and yellow tinges can be dispensed with.

"When, now, the undersigned ventures to remark important sums of money have to be paid away to foreign countries annually, for the so-much used acids in colouring, and likewise for other colouring substances, and when he, at the same time, considers that it has never occurred to any artist, colourman, or manufacturer to invent such an article that will be produced from turf and coal, thereby dispensing with the expensive foreign colouring substances, he is inclined to think with certainty that no person can doubt the usefulness and originality of this invention.

"The undersigned wishes this invention to become now universal. It is apparent that this wish might be speedily and safely realised by the undersigned publicly announcing his method of producing colour substance from coal and turf, as also the mode of application in colouring, but as the waste of time and money demanded by the necessary trials and researches for this invention is not to be reimbursed in the usual manner by attracting public attention, he is prompted to petition the worthy and honoured Imperial Chamber of Commerce to grant him the sole privilege of imparting this invention for the space of fifteen years to the entire Imperial Austrian monarchy, including the Hungarian and Italian provinces, namely, 'The production of colouring substance extracted from turf and coal, as applied to the colouring and preparation of ink, &c.'—(Signed) Jassnuger, Vienna, May 24th, 1817."

The second document is a petition from Jassnüger to the City Provost. It runs:—

"In the decree of the 17th ult., Z. 45,790, it was announced to the undersigned by the honoured City Commandant, that his Imperial Majesty had agreed to grant him the exclusive privilege for eight years, for the extent of the entire monarchy, to impart the production of colouring matter under the name of Vienna black, on condition that he should make another sample and afterwards deposit an exact and likewise sealed description of his mode of producing the above with the high Commission sitting on Commerce.

"On the 28th ult. the undersigned had the honour to present this sample to the Commission sitting, and of showing its efficacy by trials that from coal this colouring substance is produced, and that not only can wool, silk, and cotton be coloured black, but also red, different sorts of yellow, brown-red, and violet, &c.

"The undersigned is prepared, for the purpose of obtaining the above sole right, to send in the sealed description, but as he has reason to hope, according to the declaration given in his protocol of the 28th ult., that the production of all colour substances from coal used in the preparation of colours and ink can be decidedly further extended, he is prompted to beg the City Provost to allow further details to stand over to allow of a more decided and closer description.—(Signed) Jassnuger, Vienna, Dec. 17th, 1817."

Besides this, there is an original certificate of the council in the possession of the common councilman, Mr. Samuel Ritter von Lindemann, May 27th, 1818, in which it is stated that he attended Jassnüger on the 6th of April, 1818, in the Imperial Theresian Ritter Academy Laboratory, during the researches into the extraction and application of colour substance from coal; and the above-named Professor himself, in the presence of several persons, had extracted colour from coal, and had coloured silk wool with a brownish-black tinge, &c.

From these documents it will be seen that Dr. Jassnüger discovered the art of extracting black, brown, grey, sepia (?), yellow, red, brown-red, and violet from coal-tar. Green and blue are not mentioned. The exact dates, with reference to the effect of the colour on cloth, and of the quality of the ink from the new black, leave no doubt behind that we have really before us the first invention of "colours extracted from coal-tar."

There is also the fact that, in the application for a patent, it is mentioned that linen and hemp can be coloured by means of a substance extracted from coal-tar colour, and the same remarks are repeated. In latter periods it was discovered that silk and sheep-wool, as also cotton, hair, &c., may be coloured by means of aniline, but never in the same degree with regard to linen.

Through the machinations of his rivals, Prechtl and Jaquin, Jassnüger was obliged to carry the secret of extracting and the preparation of coal colours into his grave, for the above-named, through their eloquence, caused the Government to grant a patent only on the condition that they should be likewise informed of the secret, but (they being the bitter enemies of Jassnüger) this proposition was refused by him. In this manner the glory of Austria perished not only in seeing this colossal modern branch of industry fail to make the usual advance,

but also embittered the last days of the inventor in the joy and splendour of his discovery.

When my further researches are successful I shall have much pleasure in concluding the article.

January, 1871.

ON THE STEARIC ACID INDUSTRY.*

By J. LAWRENCE SMITH.

Saponification by Sulphuric Acid without Distillation.

Fats saponified by sulphuric acid by the ordinary method contain more or less tarry matter, arising from the decomposition of the fats, causing more or less loss in the raw material.

M. De Milly undertook a series of experiments, by which he finally showed that fats could be saponified by sulphuric acid without the formation of tarry matter. This point being established, he further showed that the fatty acids obtained without the formation of tarry matter were identical with the fatty acids formed by the lime saponification. These he submitted to cold and hot pressure under special conditions, and obtained cakes of candle stuff most beautifully white. As to the oleic acid, it is of a dark colour, but in no way decomposed; it makes a fine brown soap, or it can be distilled and made white.

M. Balard, in a report on this process, expresses himself in the following terms:—

"In the establishment of M. de Milly, the fat is melted and heated to 120° C.; it is then allowed to flow from its reservoir and mix with a stream of strong sulphuric acid, in the proportion of six per cent of the latter. The mixture is rendered perfect by means of agitation. The action takes place immediately, and is arrested in two or three minutes by allowing the mixture to flow into boiling water, when the sulphuric acid and unaltered glycerine enters into solution and the fatty acids float on the surface being of a dark colour. But, contrary to what takes place in the ordinary method of saponification by sulphuric acid, the colouring matter is completely soluble in the liquid fat acid, so that by cold and hot compression the solid fat acids are obtained perfectly white, ready to be moulded into candles. The entire operation can be accomplished in one hour.

"Nevertheless, it is preferable, when the cold pressure has furnished the solid acid but still coloured, to melt it again and put into the pans; then, on cooling, to submit it to all necessary pressure, when a fatty acid, fusible at 55° C. is obtained, admirably adapted for the very finest candles.

"It is readily seen that a certain portion of the solid fat acid will find its way into the liquid acid, as the crystallisable sugar in molasses; this can be submitted to distillation. Here, however, the distillation, conducted in such manner, only operates on less than one-fourth of the original solid fat, as the other three-fourths have been separated before the distillation. It is evident that the method unites the advantage of lime saponification and the process of distillation. Three-fourths of the acid obtained is fit for the best quality of candles, and the other fourth for a second quality. The yield of solid fat acids by this method, when properly conducted, is greater than by the other method of saponification with sulphuric acid, there being no loss by carbonisation."

This new process, which has been carried on for some time in the factory of De Milly, introduces an extreme simplicity into the stearic acid industry. All that is necessary to conduct the operation are a few receivers and two or three hydraulic presses, requiring but a small amount of capital. It is being introduced into various parts of Europe. M. De Milly has taken out a patent in this country, but as yet it has not been carried out practically. This process bids fair to take a high rank among the known processes for manufacturing candles.

Machinery Connected with the Manufacture of Candles.

There is nothing under this head exhibited worthy of special notice. Leon Droux showed a new form of autoclave for treating fat under pressure with water, but it appeared to the writer to be inferior to several forms now in used. Leroy and Durand exhibited a new still, in which there was nothing new except a method for regulating the temperature of the superheated steam. There was a square box inserted in the masonry acting as a recipient of two jets of steam, one highly superheated and the other from the steam boiler; both jets were regulated by valves that could be adjusted to admit more or less of one or other flows of steam, thus regulating in a very ready manner the temperature of the steam admitted to the still. Morane and others exhibited moulding machinery, but there was no new improvement on the already admirable machines for accomplishing this part of the manufacture of candles.

Oleic Acid from the Manufacture of Candles.

The employment of this acid formed a part of the original patent of Chevreul and Gay-Lussac, viz, to convert it into soap; but when the candle industry was first successfully conducted by De Milly and Motard, they found it impossible to dispose of the oleic acid to the soap makers, they being prejudiced against its use; so these manufacturers, in their unconquerable perseverance, converted it into soap themselves, and now a soap department is the natural adjunct to every candle factory. This acid is also employed to replace the oil used in the manufacture of woollen cloth, an application first suggested by Peligot and Alcan. Attempts have been made to procure elaidic acid (a solid oil acid) by the well-known reaction of hyponitric acid upon it, thus increasing the amount of fat which can be used in making candles; but, owing to the fact that this reaction produces a red substance that becomes mixed with the fat acid, the method has never found its way into industrial chemistry, and the solution of this question is left to future chemists.

Quality of Candles.

The following are some of the points to be noticed in forming a correct opinion of the quality of candles:—

1st. The nature of fatty matter used in their manufacture.
2nd. Their whiteness.
3rd. Their transparency.
4th. Their hardness.
5th. Their dryness to the touch.
6th. Their point of fusion.
7th. Their form and their moulding.
8th. The nature of the wick.
9th. The nature of the flame: if it be uniform, long or short, well supplied, illuminating, brilliant, with or without smoke.
10th. Does the cupping at the top of the candle burn dry, or is it more or less filled with melted fat?
11th. Is the fatty matter free from mineral substances?

The above list, as made out by Stas, gives all the points worthy of note to furnish a correct idea of the quality of candles. The point of fusion of the best quality of candles should not fall short of 55° C. The candles from palm oil acids, although giving very excellent light, seldom exceed 51° C., and often fall short of it, To obviate the disadvantages of using the more fusible candles some manufacturers now envelope them in a layer of material that melts at 56° C. This is done in moulding them; and, while it is advantageous, it is only an imperfect substitute for those made altogether of harder material. The candles made from distilled material give a whiter light than those from lime saponification, but are less fusible and colour more or less in contact with the air, for

* From "The Progress and Condition of Several Departments of Industrial Chemistry," by J. Lawrence Smith.

which reason the star candle from lime saponification ranks highest in commerce.

Soap.

Under this head nothing new was developed, except the use of oleic acid from the new method of De Milly, of saponifying fats by sulphuric acid without subsequent distillation. The detergent property of this soap is not inferior to that of any other, although the colour is very much darker. The manufacture of soap increases very rapidly, every day new factories springing into existence in all parts of the world. Marseilles still ranks first in the soap industry, although the inferior fats and oils are used more than formerly. We are indebted for the present condition of this industry to M. Chevreul, as well as for that of stearine candles, which now both go hand in hand, as the candle-maker must be a soap-maker to consume the excess of oleic acid which is formed in the manufacture of candles.

The different makers try to rival each other in the manner of making up the soap for market, both in appearance and size of separate masses. For the consumers the pound lump is more and more commonly used, and nowhere is the appearance of the soap attended to with more care than at the factory of the Barriére de l'Etoile, where De Milly first engrafted soap-making or star-candle making.

Conclusion.

To describe the machinery, and to give statistics of separate establishments which manufacture one or more products, would extend this report into several volumes.

The gigantic nature of some of these establishments, and their operations as now conducted, may be seen in the establishment of Messrs. Casthelaz and Co., of Paris. They decompose daily two tons of nitrate of soda by sulphuric acid to furnish the nitric acid they use for the purpose of producing the nitrogen compounds of benzol and toluol, for transforming arsenious into arsenic acid, phenic acid into picric acid, the bichloride of naphthaline into phthalic acid, &c. They transform daily a ton of benzol into nitro-benzol and aniline ; they also make naphthaline for the purpose of producing benzoic acid, first forming phthalic acid from the bichloride of naphthaline. The phthalate of ammonia, distilled, gives phthalimide of lucuine ; distilled with powdered lime, the phthalimide produces benzonitrile, and benzonitrile distilled with caustic soda gives benzoate of soda, from which hydrochloric acid precipitates benzoic acid. Attacked by nitric acid, the bichloride of naphthaline leads to an oil and forms binitrated chloride, or binitro-chloroform of Berthelot, of which the odour is so penetrating and the action on the eyes and respiratory organs so terribly deleterious. A few grms. are sufficient to cause the most painful burning to a thousand persons.

PROCEEDINGS OF SOCIETIES.

NEWCASTLE-UPON-TYNE CHEMICAL SOCIETY.

December 22nd, 1870.

John Glover, Esq., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

Messrs. Walter Bowman, Gateshead, and W. M. Sowerby, Jarrow, were unanimously elected as members. The names of Messrs. William Crowder and Henry Langdale were read for the first time.

The President read the following inaugural address :—

Gentlemen,—As I mentioned to you at our last meeting, the paper I am about to read was prepared for delivery on that occasion, but was not then read from causes which I explained. The honour you conferred on me by my selection as your President was quite unexpected on my part, and the notice of my election being of the shortest, I shall not attempt any formal address, but ask your indulgence while I point out some lines of thought and action which I think lie fairly before us as members of a chemical society. The first subject, both in absolute and relative importance, is that now stirring the public mind so deeply, viz., the application of the scientific knowledge of the age to the preservation of the public health generally, and its special application to the prevention of the serious mischief and waste consequent on our sewage arrangements. Many of you will be aware that this subject has been approached from almost every conceivable point, and yet, partly from its inherent difficulties, and perhaps still more from the financial difficulty and the force of habit, we appear to be as far from a true and practical solution of the problem as ever. If any of you can discover a process by which you can rapidly and cheaply remove from ordinary sewage all organic matter, whether soluble or insoluble, and all phosphatic salts, you will have almost solved the problem, and may expect to reap an abundant harvest both in honour and profit, besides securing the blessing of the "peacemaker," for surely he is entitled to that title who introduces harmonious action among natural laws where there was previous strife and consequent evil.

I will not detain you by describing any of the processes that have been tried to effect this end, for their name is legion, and I am not aware of one now in operation on a large scale which at the same time effects the end I have named and can be proved to be a financial success. Excepting the old plan of irrigation, all plans yet tried have been, practically, failures. The atmospheric conditions most conducive to a healthy life are pretty well known, and hitherto the evils arising from sewer emanations have been combated (when combated at all) by disinfectants, some based on their oxidising, and others on their deoxidising powers. The best disinfectant, other things being equal, is of course that which shall come into most intimate contact with the thing to be disinfected ; therefore, for air pollution gaseous disinfectants must be absolutely essential, while for the disinfection of solids or liquids, disinfectants in a liquid form may be most effective. I mention this subject here to draw your attention to the mischief that may be done by the use of materials which are often puffed as being applicable to air disinfection, when their only possible use can be for the disinfection of solids or liquids, and even for this the use of some of them is questionable. In connection with this question it may fairly be asked why sewers should be allowed to poison the air with their gaseous emanations ; surely the engineering skill of this country is equal to the task of so ventilating sewers that every opening shall draw air in, instead of allowing poisonous gases to issue from them. Newcastle, from the inclination of its sewers and cheapness of fuel, seems to be peculiarly well adapted for such ventilation.

As part of this question, the manufacture and purification of coal-gas have a peculiar claim on our consideration from circumstances to which I need not further allude ; whether the balance is in favour of iron or lime purification is a subject we might profitably investigate, and also the relative merits of each in connection with the somewhat peculiar situation of our own gas-works. How far the existence of gas-works in large towns is compatible with health and comfort is a question which will require a combined medical, chemical, and mechanical knowledge ; some other towns appear to have settled the question in the negative, as a few days ago I saw most extensive and expensive preparations making to remove a gas-work from a part of London where the population is very dense, to a distance of seven or eight miles down the Thames.

An examination of the water supplied to our town

would furnish a very excellent paper and one that would probably give rise to some valuable discussion.

If I mistake not, such chemical examinations would place our water supply in a more favourable position than many would suppose; you may gather my opinion from the fact that, after a chemical examination of many springs of water near my own residence, I have given the preference to the Newcastle company's water as a beverage. Beyond an occasional turbidity, which is easily removed, for drinking purposes, by a small filter, or even by subsidence, there is little to be complained of as a rule. This opinion I ask some of you to bring to the test of experiment, and as the experiments are not very difficult, I trust some of our members will make it the subject of a paper during the session.

I shall not risk wearying you by further dwelling on these subjects, as you may consider them too general in their nature, but I do ask you most earnestly to prepare papers on these, or similar subjects, as well as papers describing processes in which you find your daily work, *especially* describing any alteration or improvement in any known process, and, more important still, stating defects and needs in existing processes, so that discussion might be provoked, and thought be directed to useful ends. For instance, our medical members (of whom I am sorry we have so few) might give us some most interesting and useful papers on the germ theory of disease, and follow up the researches so ably carried on by Dr. Angus Smith and others. To some of you may, perhaps, fall the honour of proving whether or not zymotic diseases are propagated by living germs, and, it may be, proving that each disease has its specific germ, requiring a specific soil or condition for its germination or evolution.

The place of dialysis in the phenomena of life would also be a most interesting subject for investigation, or even the gathering up of all the information known on the subject would furnish a most interesting paper. The physics and chemistry of coal-mining would also be an interesting and useful channel for our thoughts and action, and one peculiarly appropriate to a Newcastle Society. The cause of the high temperature of coal-mines, whence is it? The opinion that it is due to mere depth is, I think, disproved by the fact that salt-mines, nearly as deep as the Monkwearmouth coal-mine are at a much lower, in fact, a comfortable working temperature; perhaps, investigations on the part of some of our members connected with the working of coal mines would show that the high temperature is caused mainly by a chemical action still going on in the coal itself. The important problem of producing a light which shall be safe under the varying conditions of coal-mining is yet unsolved, and any of you who can discover a practicable mode of generating light without heat, or with such a moderate degree of heat as would fail to ignite carburetted hydrogen, would solve the problem, and be hailed as one of the world's greatest benefactors. A safer mode of getting coals than by the use of gunpowder is much needed, and any substance you could discover for this purpose that should have a great and rapid expansive force, combined with a moderate elevation of temperature, say, below 1000° to 1200° of Fahrenheit, would bring both honour and profit to its discoverer, and confer an almost inestimable boon on the coal miner. The manufacture of pig-iron has been so very ably treated here and elsewhere by your ex-President (whose eminent fitnees for the work has been unanimously conceded), that it would be presumptuous in me to do more than allude to it, but he would be the first to tell you that there is room enough for all the ability and time you can devote to the consideration of that process. Especially in the matter of heating the blast is this true, while in the process of manufacturing pig into malleable iron and steel the Bessemer process has not exhausted improvement. The limited class of irons to which that process is applicable still leaves the field comparatively unoccupied. The enormous amount of fuel used in this process above that indicated by theory should engage the attention of such of you as have the combined chemical, mechanical, and physical knowledge, and the field is ample notwithstanding the many experimenters now occupying it. What is the essential difference between iron and steel, is it chemical or physical, or both? would furnish an interesting and useful paper from some of our members engaged in metallurgical chemistry, and would probably also give rise to considerable discussion.

The alkali manufacture may be said to still await the coming man. I will not say to introduce a shorter or more economical process than the present round-about process of Leblanc, as that would lead me into an examination and description of the almost numberless processes which have been proposed to effect that end, but rather to perfect that process. How far we are from having developed all the apparent possibilities of Leblanc's process may be judged by the results obtained in practice when brought to the test of its theoretic possibilities. Taking the best commercial sulphate of soda to contain 97 per cent of pure sulphate, all alkali manufacturers will agree with me that the amount of available soda obtained from such sulphate falls far short of that which theory indicates ought to be obtained. To make this clearer to you I have put in a tabular form what is actually got, and what we should have by theory.

100 of Commercial Sulphate at 97 per cent NaO,SO_3 contains 42·3 Soda (NaO=31).

Soda-Ash at 52 per cent soda.			Ash at 54 per cent.	
To obtain of soda-ash from 100 of sulphate.	Shows loss of soda (NaO).	Equivalent in soda-ash.	Loss of soda.	Equivalent in soda-ash.
70	5·9	11·3	4·5	8·3
71	5·4	10·4	4·0	7·4
72	4·9	9·4	3·5	6·5

From this table you will observe that, taking the best commercial sulphate to contain 97 per cent of sulphate of soda, we should obtain from it 42·3 of soda (NaO=31). Now, in practice, different manufacturers obtain varying results, but they will be about covered by the figures in the table. Thus, some manufacturers obtain a soda-ash containing 52 per cent of soda, and a yield of 70 per cent from the sulphate used; in this case they are short of the theoretic amount they should obtain from 100 of sulphate, 5.9 of available soda, or 11·3 of such ash. If 71 per cent of a 52 per cent ash is obtained from the sulphate used, then are they minus 5·4 of available soda or 10·4 of such ash, and at 72 per cent from sulphate (which is a quantity scarcely if ever exceeded) they are minus 4·9 of available soda or 9·4 of ash. To pass to the next division of the table—taking the work of a manufacturer who is making a soda-ash containing 54 per cent of soda, we still find there is ample room for improvement; thus, taking his yield of ash from sulphate at 70 per cent (which is, I believe, common at this strength) he is (for each 100 of sulphate used) minus 4·5 of available soda or 8·3 of such ash; at 71 per cent yield of ash he is minus 4·0 of available soda or 7·4 of such ash; and at 72 per cent of ash from sulphate he is still minus 3·5 of available soda, or 6·5 of ash, containing 54 per cent of soda. Now, we know that a considerable portion of this loss of soda occurs in the furnaces and lixiviating tanks, but how it occurs there, or in what form some portion of it exists in the tank waste, has not been satisfactorily shown. To prevent misunderstanding here, allow me to observe, what many of you are, I have no doubt, aware of, that the largest portion of this loss of available soda is due to the imperfect decomposition of sulphate of soda in the ball furnace, and also (but, I believe, in a much less degree) to imperfect lixiviation of the balls. Probably as much as 4 of soda in the 52 per cent ash, and 2 of soda in the 54 per cent ash, is due to these causes. And here I would urge upon such of our members as have time and opportunity to institute some experiments of research in this direction, and let us have the scientific result of their labours.

To those of you who are more directly engaged in

analytical chemistry I would suggest the institution of researches, having for their object to explain, and so go far to prevent, the varying results we get in the examination and valuation of the material used and made in our various manufactories; all of you who are actively engaged in manufactures will know the great annoyance we have from this source.

As my warrant for the observations I have just made, I beg to draw your attention to this tabulated result of the examination of seven different samples of sulphur ore (pyrites) by three different chemists, each having the same sample to operate on.

No.	Sulphur, per cent.			Difference between A and B.	Difference between A and C.
	A	B	C		
1	38·8	38·0	40·2	0·8	1·4
2	40·9	40·9	42·7	—	1·8
3	39·6	39·1	41·1	0·5	1·5
4	39·3	39·5	40·7	0·2	1·4
5	41·6	41·4	43·5	0·2	1·9
6	38·2	38·0	40·0	0·2	1·8
7	39·7	39·2	41·1	0·5	1·4
		Average		0·34	1·6

You will observe that between A and B there is comparatively little difference, the maximum amount being 0·8, while between A and C the maximum difference is 1·9; the average difference between A and B is only 0·34, but the average difference between A and C is 1·6.

I ought to mention that A and B are local chemists, but residing and working in different parts of the district, and acting quite independently of each other, and that C is a metropolitan chemist; also that A and C are professional analytical chemists; B is chemist in the laboratory of the purchasers of the ores. I need not pursue this branch of the subject further, although many of you are aware that my observations would apply with even greater force to manganese and other materials used in the manufactures of the district. If I had time, and the requisite knowledge, I might go through all the manufacturing operations of the district, and point out in each the need and opportunity for useful work. I trust these observations will induce you to give what aid you can in the development of our infant society, so that it may take a place among the scientific societies of our land worthy of the manufacturing district in which we are placed. In justice to myself, a few words more, and I have done. Some of you may have imagined that most of my recommendations have a mere money value for their end, and have not been made in the interests of pure science. If this it so, it is not, I assure you, because I undervalue the pursuit of science for its own sake; for, I believe, that such of you as devote yourselves to purely scientific investigations, from the promptings of a pure and devoted love, may not, as your reward, gain material wealth, but you will have formed, what is of infinitely greater value, a noble character, and a *capacity* for enjoyment which mere wealth cannot give.

Mr. I. L. BELL.—Mr. President and Gentlemen,—I am sure you will at once divine the object which I have in rising to address a few words to this meeting; it is to tender our very hearty thanks to the President for the very admirable and painstaking address which he has just delivered. I trust the advice which he has tendered to the members of this society, and I will say more particularly to the younger members of the society, will be taken note of, and that we shall at no distant period be able to say that we have reaped some advantage from the address which he has placed before us in so pointed, and indeed I may say in such eloquent, terms. No doubt the address itself has partaken, as our President has justly stated, in a great degree of the utilitarian phase; but I ventured, in an address which I had the honour of delivering before this body, to state that it could scarcely be expected of gentlemen engaged, not in the pursuit of abstract science, but rather in the application of science to the general purposes of life, that any great or important contribution could be afforded by us to abstract science. But notwithstanding, it cannot be denied that of all classes of the community there is none so capable of affording knowledge of a particular kind as we ourselves, engaged as we are in the application of those great scientific truths which have been taught to us by the chemists of the day—engaged as we are, as I have stated, in the application of those truths to practical purposes. Possibly, indeed certainly, there can be no application of science of a more interesting character than that where it seeks to elevate the standard of public health; and I am glad indeed that our President has drawn your attention to this subject. I will only state in reference to this, and in justice to a body with which I have the honour of acting, namely the Town Council of Newcastle, that to a certain extent the recommendations of the President have been forestalled by that body, because we have, I am glad to say, an accomplished chemist (Mr. Pattinson), a member of the Newcastle Chemical Society, from whom we receive periodically reports on the quality of the water supply to the town as well as on the quality of the gas; and I take this to be a very important function, indeed, of every municipal corporation, that in a matter affecting, so directly as it does, the public health, every pains should be taken to assure to those who consume the water an article of the highest possible purity. In conclusion, I would ask the meeting to do that which I have no doubt they will be too happy to acquiesce in, namely, to record a vote of very hearty thanks to the President for the very able address with which he has just favoured us.

The proposition was carried by acclamation.

The PRESIDENT—I have to thank you, gentlemen, for the very hearty manner in which you have responded to Mr. Bell's proposition. It will always give me the greatest pleasure to contribute what I can to the prosperity of this society; and, with Mr. Bell, I do hope that the younger members of this society will not be diffident in preparing papers to submit to the society. We want some young and fresh thought; and although the ideas may sometimes be crude, yet they may give rise to very useful discussions, discussions which may in the end prove of value where the papers themselves might, perhaps, not be of value. I thank you heartily, gentlemen.

ROYAL IRISH ACADEMY.

A GENERAL meeting of the Academy was held on Monday, the 23rd. Professor HENNESSY in the chair.

Professor ROBERT S. BALL, read a report, which was illustrated by diagrams and experiments, "*On the Resistance of the Air to the Motion of Atmospheric Vortex Rings.*"

This was one of the reports for which the Academy had voted a grant of money.

If it were not for the friction of the air these vortex rings would move on for ever, but the resistance of the air finally stops and disperses them. These vortex rings made apparent by smoke or steam are sometimes well seen proceeding from the orifice of a railway engine.

The result of Professor Ball's experiments, which had been very numerous, was the determination of a law—which law may be stated as follows:—The resistance of the air is directly proportional to the first power of the velocity.

There were three more papers read, but the only scientific one was by Dr. Macalister, "*On Observations on Muscular Anomalies in Human Anatomy.*"

NOTICES OF BOOKS.

A Laboratory Text-Book of Practical Chemistry; or Introduction to Qualitative Analysis. By William G. Valentin, F.C.S. Published by Churchill. Price 10s. 6d.

Conscious, no doubt, of the great dearth of text-books, Mr. Valentin has, under the title quoted above, given to the world what must be regarded as the official primer, so to speak, of the only school of chemistry which is acknowledged by government.

For years the painstaking and successful instructor in the laboratory of the Royal College of Chemistry, and a thoroughly practical and experienced analyst, our author is highly qualified to deal with the requirements of the student. He has also the advantage of acting under the superintendence of Dr. Frankland, so that both theory and practice are ably represented in the work before us.

Beginning with the preparation of gases, Mr. Valentin, with excellent practical examples at every step of the way, takes the student through the theory of modern chemistry, combining proportions, atomic theory, molecular volumes, &c.; the last chapter of this portion of the book giving, curiously enough, a definition of the science. In the second part, the subject of qualitative analysis is taken up. The metals, separated into groups, are illustrated by an elaborate and varied series of reactions, each of which is fully explained to the student, with the chemical formula for each step. Coming from the school of Frankland, the "graphic" mode of indicating the composition of substances is, of course, religiously given; indeed, occasionally, from the omission of the usual summary of the constituents, a person accustomed to the well-known manner of writing well-known compounds does not recognise his old acquaintances till he has added up his atoms in much the same manner as a column in an account book. This is, however, not likely to incommode a student going regularly through the course. After each division of the subject, a series of capitally chosen questions is given, leading the student to the application of what he has previously learnt, to the calculation of most chemical problems, and to the formation for himself of schemes for separation of metals not given in the text—a series of mental gymnastics not unduly disproportioned to his strength, but admirably adapted to his attaining a sound mastery of his subject. About sixty pages at the end of the volume are devoted to very full analytical tables, including the identification of many organic acids, more than commonly enter into such tables.

It is somewhat singular that, with great profuseness of illustrative reactions, the precautionary measures occurring at every step are somewhat meagrely dealt with, except in so far as a reader of previous experience may notice them incidentally in the body of the work. No summaries of special methods whatever are given.

And one very great deficiency is felt in the scheme of analysis given in this the text-book, as we have said, of the only chemical school the British Government has ever deigned to assist and countenance, namely, that about one-third of the elements are *ignored*, except as to placing them in a list.

This saves, it is true, an infinity of trouble, and, since all the commoner substances are fully handled, for trade purposes he is not likely to require more. But, if ambitious to become a philosophic cultivator of science, and an investigator, he will have, after mastering a scheme of analysis, subsequently to modify and add to that method. Besides which, sundry elements, commonly called rare, are met with much more frequently in mineral examinations than when they were more seldom looked for, as, for instance, lithium, titanium, &c.; yet, during the last twelve years, the translation of "Fresenius," and the tables in Dr. Noad's work, and Connington's translation of Will's book, are the only attempts, so far as we are aware, in our language at comprehensive analysis. It is, surely, beneath the dignity of a laboratory, aspiring to be considered national, that its manual should take such a low and unphilosophical position as to neglect completeness, and miss the opportunity of dealing with imperfectly occupied ground. But, although the scantiness of the course, and its omissions, as it at present stands, would be fatal to the acceptance of this work as a standard treatise on practical analysis, yet these are faults which might, and we would fain hope will, be removed by an appendix to the present edition and an incorporation with a future one. And, as regards a practical foundation of thorough knowledge of chemical reaction and of "learning the elements" as it is popularly termed, Mr. Valentin's book will take a remarkable position, and one thoroughly well deserved, from the great thought and pains evident throughout it, to advance the student quickly, soundly, and comprehensively.

The Retrospect of Medicine: being a half-yearly journal containing a retrospective view of every discovery and practical improvement in the medical sciences. Edited by W. Braithwaite, M.D., &c., and by James Braithwaite, M.D. Lond. Vol. lxii., July—December, 1870. London: Simpkin, Marshall, and Co. 1871.

This well-known bi-annual periodical deserves, as usual, the attention of medical practitioners. It is evident, at a glance, that the authors are exceedingly well up to the arduous task of compiling a volume, as this is made to contain, compressed and condensed into some 400 pages, an enormous amount of very judiciously and carefully written matter relating to that extensive field of sciences included in the general term of "Medicine." For the chemist we quote the following new test for albumen:—Add to the liquid to be examined, in a test-tube, 10 minims of alcohol of 0·805 sp. gr., shake thoroughly, but gently, so as to avoid the production of froth; then add, by drops, the same quantity of carbolic acid, and shake very thoroughly; allow the tube to stand for a minute and, if the merest trace of albumen is present, distinct flocculi will be seen floating in the liquid. This test is much more delicate than the heat and nitric acid test. Although, as might be expected, Dr. C. M. Tidy applies this test especially for the detection of albumen in urine, it (the test) may, perhaps, be applicable to detect albumen, when present in small quantity, in other fluids. Of the work before us we may say that, in order to render reference an easy matter, the authors have added, in addition to a table of contents and copious index, a very carefully made synopsis, which greatly enhances the value of the volume.

CORRESPONDENCE.

ANTISEPTICS AND DISINFECTANTS.

To the Editor of the Chemical News.

Sir,—When Dr. Calvert was experimenting upon disinfectants, I am surprised that he did not include amongst his experiments the powder known as "Mudie's Disinfectant." I think he would have found that the antiseptic powers of that body would have been equal to the carbolic acid powder; as "Mudie's Disinfectant" contains so large a percentage of anhydrous sulphate of iron mixed with a certain proportion of alum and red sulphate of iron, it plays the treble parts of deodoriser, disinfectant, and antiseptic.

With regard to the much vaunted "Chloralum" I see Professor Gamgee says in his letter of January 13th, "The agent (chloralum) had never been thought of in therapeutics until last January;" again, in his letter of the 27th, "since I first thought of the chloride as an antiseptic just a year ago." In Ure's "Dictionary," 1863, Article "Disinfectants,"

chloride of aluminium is mentioned as an antiseptic; it says, "Meat, if well packed, cleaned, and washed with a solution of chloride of aluminium, will keep three months." After that I hardly think Professor Gamgee can lay claim to the discovery of the antiseptic and therapeutic properties of chloride of aluminium.

He also says that chloralum covers a larger field of useful applications than carbolic acid; surely there must be some slight mistake here, for chloralum is not able to remove the smell from alkaline sulphides, as any one can prove by a simple experiment. I sent for a bottle of chloralum, and compared it with Mudie's disinfectant. Not having a badly smelling midden or cesspool close by, I used some yellow sulphide of ammonium to test the deodorising powers of the two compounds. With Mudie's the smell immediately disappeared; instead of chloralum removing the odour, it made it rather worse, as sulphuretted hydrogen was liberated in abundance, thus showing that it is not able to remove so noxious a gas. Sulphide of ammonium being generated in middens and cesspools, I cannot think that chloralum is a good deodoriser for water-closets and such like places.—I am, &c.,

J. Carter Bell.

Manchester.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Annalen der Physik und Chemie, von Poggendorff (Supplementary number), vol. v., No. 2, 1870.

This number contains the following original papers and essays:—

Researches on the Specific Heat of Mixed Fluids.—J. H. Schüller.—This paper is the concluding portion of an algebraico-physical essay containing the following chapters:—Alcohol and chloroform; alcohol and benzol; chloroform and sulphide of carbon; sulphide of carbon and benzol; chloroform and benzol. The general result of the author's researches is, that in the case of mixed liquids, as those referred to, the true specific heat can be either greater than the mean specific heat of each liquid by itself, or it can be the same; but these researches do not show whether, when each of these instances occur, there may not also occur what is frequently observed with saline solutions, viz., that the specific heat observed is less than the mean. The author concludes by stating that he is engaged with another series of experiments to elucidate this matter.

Maximum of Density and Expansion of Distilled Water, of the Water from the Adriatic Sea, and of some Saline Solutions.—Dr. F. Rossetti.—This monograph contains the account of a series of experiments on the subjects above-named. The paper is not suited for any useful abstraction, on account of the large number of tabulated forms exhibiting the results of experiments.

Subjective Phenomena of Colours.—J. K. Becker.—This paper contains a lengthy discussion on a statement made by Dr. Helmholtz in reference to physiological optics, which statement the author thinks is erroneous; and he tries to explain certain phenomena of vision on purely physical grounds.

Passivity of Iron; and on Electrolysis.—L. Schönn.—The author states, that when a piece of iron is tightly fastened to a piece of charcoal, care being taken to make the contact betwen the charcoal and well-polished iron as perfect as possible, and also to immerse both these substances simultaneously into the nitric acid, the iron is not dissolved; but, as soon as either the metal or the charcoal are touched, under the surface of the acid, with a strongly electro-positive metal (for instance, zinc), the iron becomes at once active again, and is dissolved in the acid with a copious evolution of gas. When some very dilute hydrochloric acid, so weak that it hardly acts upon zinc, is poured into a platinum basin, and a piece of zinc placed in that liquid in metallic contact with the platinum, a copious evolution of hydrogen takes place at once, precisely on the spot where the zinc, platinum, and acid are in contact. If, instead of the very weak acid, an aqueous solution of corrosive sublimate is taken, and the experiment repeated, metallic mercury is separated at the point of contact between the zinc, platinum, and the solution. The author finally states that, from a series of experiments made by him, he has found that all desired electro-chemical actions can be called forth at pleasure by simply placing either two different metals, or charcoal and metals, in contact with a fluid.

The American Journal of Science and Arts, January, 1871.

This number contains the following original papers and essays relating to chemistry and allied sciences:—

Quaternary, or Post-Tertiary, of the Newhaven (Connecticut, U.S.), Region.—J. D. Dana.—The first portion of a memoir which, although bearing more especially on the locality above named, concontains matters of general interest to geologists.

Corona Seen in the Total Eclipses of the Sun.—Professor W. A. Norton.—Reserved for reproduction in full.

Duration of Flashes of Lightning.—O. N. Rood.—The result of the author's experiments on this subject is that the duration of flashes of lightning, as observed by him, and measured by means fully described in this memoir, during a violent thunderstorm in August last, amounts, in round numbers, to about 1-500th of second, the average length of the streak being 9°.

Physical Condition of a Closed Circuit Contiguous to a Permanent and Constant Voltaic Current; or, on "the Electrotonic State."—Dr. A. M. Mayer.—Illustrated by diagrams.

Gahnite from Mine Hill, Franklin Furnace, New Jersey, U.S.—G. J. Brush.—The author describes the mineralogical characters of this rare species of mineral found in a zinc mine. The mineral is crystalline; colour, blackish green; hardness, 7·5; sp. gr., 4·89 to 4·91; infusible before blowpipe; with fluxes, reacts for iron and manganese, and with soda on charcoal gives a zinc coating. The composition of this mineral, in 100 parts, is—Alumina, 49·78; ferric oxide, 8·58; zinc oxide, 39·62; manganous oxide, 1·13; magnesia, 0·13; silica, 0·57. This variety of gahnite (so named after the celebrated Swedish mineralogist, Gahn) shows a larger percentage of zinc than any specimen of this mineral heretofore analysed; it is associated with black mica, apatite, calcite, and a brownish variety of chrysolite, which, on partial analysis, was proved to be a mono-silicate of iron, manganese, and zinc.

Meteors of November 13th and 14th, 1870.—H. A. Newton.

Some Phenomena of Binocular Vision.—J. Leconte.—The fourth part of a lengthy memoir on this subject, largely illustrated with wood engravings.

Earthquake of October 20th in North-Eastern America.—A. C. Twining.—A very full and careful account of this phenomenon as observed and felt in the country alluded to. Among the surprising results detailed in this paper, we find that the earthquake wave progressed from about E. 6° N. to about W. 6° S. at the rate of 160 miles a minute, being six minutes and a half from St. John's, New Brunswick, to Chicago, Illinois, a distance of nearly 1200 miles in a straight line. The ordinary direction of the subordinate undulation was about N. by E.

American Journal of Pharmacy, No. 1, 1871.

This number contains the following original papers more particularly relating to chemistry and physical sciences:—

Carré's Apparatus for Making Ice.—W. Procter, jun.—The invention of M. Carré consists in the use of ammoniacal gas, liquefied by pressure, as the agent for freezing water, which it does by abstracting and rendering latent the heat necessary to the liquid condition of water. The manner of using the aqua ammoniæ (concentrated liquid ammonia) to effect this purpose is exceedingly ingenious, and apparently paradoxical, inasmuch as "heat is applied to produce cold;" "fire to make ice," this being one of the claims of originality made by the patentee; the other claim being "the application of the power of absorption due to mutual affinity as a means of producing vacuum, volatilisation, the removal of heat, and the consequent production of cold." The greater part of this paper is devoted to the description of the apparatus used for the manufacture of ice, the machinery being illustrated by engravings.

Sulpho-Carbolic Acid and Sulpho-Carbolates.—J. Creuse.—After briefly referring to the more extended use of the substances just named in medicine and pharmacy, the author says that his intention is to prove that—(1) sulpho-carbolic acid is composed of 3 equivalents of sulphuric and 1 equiv. of carbolic acid; (2) that to combine carbolic and sulphuric acids without waste of carbolic acid, it is necessary to take 6 equivs. of the latter and 1 of the former; (3) that sulpho-carbolates are composed of 2 equivs. of the acids and 3 of the base. The process of examination employed by the author consists in heating together various proportions of the two acids, and, by means of carbonate of baryta, determining what quantity of sulphuric acid is converted into sulpho-carbolic acid, what proportion of it remains free, and how much carbonate of baryta is transformed into the sulpho-carbolate of the base. The mode of preparation of sulpho-carbolic acid, to be used medicinally, is described at length. The pure sulpho-carbolic acid is colourless, has no smell, dissolves in dilute alcohol and ether in all proportions; the acid has no antiseptic properties, because its aqueous solution becomes mouldy in forty-eight hours during hot weather; nitric acid, especially if aided by heat, decomposes it, forming, among other compounds, picric and sulphuric acids. The sulpho-carbolate of soda may be prepared by saturating sulpho-carbolic acid with carbonate of soda; the ensuing sulpho-carbolate is colourless, but reddened by direct exposure to sunlight. The shape of the crystals is like sulphate of zinc; the soda-salt neither effloresces nor deliquesces; it tastes like sulphate of soda, slightly bitter and saline, but not acrid; nitrate of baryta does not precipitate the soda-salt. The

sulpho-carbolate of zinc crystallises in the shape of flattened prisms; is colourless; soluble in water and alcohol; not precipitated by salts of baryta; is far more readily affected and coloured by direct sunlight than the soda salt, and, like it, cannot be kept in weak solutions during hot weather without becoming mouldy.

Cheap Process for Preparing Artificially-Precipitated Carbonate of Baryta.—J. Creuse.—After referring to the great value of artificially-precipitated carbonate of baryta to chemists, and the imperfections of the modes of obtaining it, the author suggests the following method of preparing the substance alluded to:—Take of witherite (native carbonate of baryta), in small lumps or in powder, a convenient quantity, add to it from four to five times its weight of water, and dissolve it with hydrochloric acid gradually; keep rather an excess of baryta than of acid, allow to settle, and decant the clear liquid. To this add a solution of oxalic acid as long as a precipitate is formed, and, though a slight excess is not objectionable, 30 grains are generally sufficient for each pound of witherite (the proportion may vary, however, according to the purity of that mineral). Let the liquid stand for one hour, filter, and add to the filtrate a quantity of caustic soda just sufficient to give it a decided alkaline reaction. After an hour's rest filter again, and treat the liquid by a solution of carbonate of soda; collect and wash the precipitate in the usual manner. This process of purifying is based on the fact, observed by him, that when a solution of oxalic acid is added to a liquid containing salts of lime and baryta both, all the lime is precipitated first, and immediately; after the lime is eliminated, caustic soda precipitates all the foreign bodies likely to be present, such as iron, alumina, &c. It may be objected that this process does not remove strontia; but, in the first place, strontia seldom occurs in witherite, and the old process for making baryta salts from the sulphate also leaves strontia if it is present; while, if the precipitated carbonate be required absolutely pure, it is only required to modify slightly the above-described *modus operandi*, and use pure chemicals.

Resignation of W. Procter, jun., as Editor of this Periodical.—From the Minutes of the College of Pharmacy we learn that, after having held the post of editor of this work for a period of twenty-five years, Mr. Procter has tendered his resignation. His resignation was received with sincere regret on the part of the Philadelphia College of Pharmacy, which institution loses a most able editor and very widely-known exponent of pharmaceutical, as well as, we may add, general sciences. The regret felt in America is shared by us, for we have long known and esteemed the eminent editor who has so ably filled this responsible post.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 18, 1870.

This number contains the following original papers and essays:—

Constitution of the Native Combinations of Tantalum and Niobium.—Dr. C. Rammelsberg.—This paper treats on hjelmite, a mineral met with near Kararshof, Sweden. The mineral is infusible in the blowpipe-flame; black-coloured; sp. gr., 5·655. In 100 parts this substance contains—Ta_2O_5, 54·52; Nb_2O_5, 16·35; SnO_2, 4·60; WO_3, 0·28; UO, 4·51; MnO, 5·68; FeO, 2·41; CaO, 4·05; YO, 1·81; CeO, 0·48; MgO, 0·45; H_2O, 4·57. The author quotes in full the results of the analysis and of other researches made on this substance by Dr. Nordenskiold. The chief difference between these analyses is, that the author of this paper found more (6·5 per cent) tantalic acid, and less Fe and Y.

Some of the Properties wherein Hydrogen and Oxygen Agree.—J. Thomsen.—From this paper we can only quote some of the main points which the author has deduced from the results of his experiments. One litre of dry hydrogen at 0° and 760 m.m. at latitude 45° and sea level yields, on combustion, 0·8041 grm. of water; at the same latitude, and under the same normal conditions, 1 litre of oxygen weighs 1·4293 grms., and two litres of hydrogen weigh 0·1791 grm., together a weight of 1·6084 grms. The author found that the combustion of 2 litres of hydrogen yield 1·6082 grms. of water, whence it follows that 2 volumes of hydrogen combine exactly with 1 volume of oxygen to form water. From this fact the author concludes that, since $\frac{1·4293}{0·08954} = 15·963$, the atomistic number of oxygen is equal to 15·963. The quantity of heat developed by the combustion of 1 grm. (absolute weight) of hydrogen to water is equal to 3782 caloric units (calculated for vacuum); the calorific factor of hydrogen may therefore be taken at 34,000, a number which differs about 1 per cent from the figures generally adopted.

Some Lecture Experiments.—J. Thomsen.—The first of these experiments is described under the title of the *Reciprocal Combustion of the Elements of Water.*—In order to illustrate the arrangement of the required apparatus, a woodcut has been added, which is required for the proper understanding of the mode of displaying the phenomenon of first burning hydrogen in an atmosphere of oxygen, and next the last-named gas, in an atmosphere of hydrogen. *Combustion of Oxygen with a Sooty Flame.*—Take a long-necked flask; pour into it some benzol or oil of turpentine; close the flask with a doubly-perforated cork through which two short glass tubes are passed, one of which should be of about 1 centimetre internal diameter, the other is narrower and somewhat bent sidewards. Let the liquid in the flask be boiled, and, as soon as the vapours issue from the wider tube, ignite them; and, this having been done, there is passed through that tube another narrower glass tube connected with a suitable gas-holder, or other vessel, containing oxygen. This tube should be provided with a platinum burner bent upwards and fitted with a cork, which closes the opening of the wider tube. This oxygen-carrying tube is made to pass deep into the flask; and immediately after the closing of the 1 centimetre wide tube, the oxygen begins to burn with a sooty flame, while the excess of the vapours of the boiling liquid are discharged by the narrower glass tube above alluded to. *Oxidation and Reduction, and the Change of Weight accompanying this Phenomenon.*—Take oxide of copper; make it, by means of weak gum water, into a paste, and shape that into a cylindrical form 1 centim. in diameter by 3 centims. in height; dry it; ignite it gently; next reduce it, by means of hydrogen, to the metallic state at as low a temperature as possible; cool in the current of the gas just named; and next wind round the cylinder a platinum wire, the free end of which should be molten into a glass rod, so as to be used as a handle. This having been done, have ready two tubulated bell-jars—one turned with its wide opening downwards, to hold hydrogen gas, to be carried into it by a flexible tube through the tubulus; the other jar, with its wide opening turned upwards, to hold oxygen. Now heat the copper cylinder (which, although very porous and spongy, has sufficient cohesion to be submitted to the experiments) gently, taking care not to make it red-hot, and immerse it into the oxygen, whereupon it will become suddenly red-hot, and continues so until the oxidation is finished. After having sufficiently cooled, the cylinder is transferred to the jar containing hydrogen, when again it begins to ignite, throwing off a strong light, while water condenses on the sides of the jar. Since the change of weight by the alternate process of oxidation and deoxidation, of a cylinder of the above dimensions is nearly 1 grm., it may be exhibited in a lecture-room by an ordinarily good balance.

NOTES AND QUERIES.

Protochloride of Antimony.—Will any of your readers favour me with the process of making protochloride of antimony, or inform me where I can get it ready prepared?—AMATEUR CHEMIST.

Coating Cisterns.—Could any of your correspondents inform me if silicate of potash, or any other similar material could be advantageously used to coat brick and cement cisterns in order to make them perfectly water-tight?—R. BINNS.

Soldering Iron and Steel.—Can you inform me, through your valuable paper, of a solution suitable for soft-soldering iron and steel, and which, if left on, will not rust it? Such a solution is used by some, but I cannot tell how it is made or where to buy it. Chloride of zinc is said to answer, but in my experience it rusts the work if left on it. GEORGE G.

MEETINGS FOR THE WEEK.

MONDAY, Feb. 6th.—Medical, 8.
—— Royal Institution, 2. General Monthly Meeting.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology. (Educational Course.)
TUESDAY, 7th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
—— Zoological, 9.
—— Ethnological, 8.
WEDNESDAY, 8th.—Society of Arts, 8.
—— Geological, 8.
—— Microscopical, 8. Anniversary.
THURSDAY, 9th.—London Institution, 7.30. Mr. F. S. Barff, M.A., F.C.S., "On the Action, Nature, and Detection of Poisons."
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— Royal, 8.30.
—— Royal Society Club, 6.
FRIDAY, 10th.—Royal Institution, 9. E. J. Reed, Esq., C.B., "On some Fallacies connected with Ships and Guns."
SATURDAY, 11th.—Royal Institution, 3. Rev. W. H. Channing, "On Laws of Life Revealed in History."

TO CORRESPONDENTS.

*** Vol. XXII. of THE CHEMICAL NEWS, containing a copious index is now ready, price 11s. 8d., by post, 12s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our office, or, if accompanied by a cloth case, for 1s. Subscribers wishing to complete their sets of volumes are requested to apply to the publisher, who will give them information respecting scarce numbers and volumes. Vol. xxiii. commenced on January 6th, and will be complete in twenty-six numbers. READING CASES, price 1s. 6d. each post free, may also be obtained at the Office.

George Cathcart.—We believe there is no such process.

E. C. C. Stanford.—Your communication arrived too late for insertion in the present number.

J. R.—We shall probably give the process in an early number.

BOOKS RECEIVED.

On a Localised Outbreak of Typhoid Fever in Islington during the Months of July and August, 1870. By Edward Ballard, M.D. London: J. and A. Churchill.

Sixth Annual Catalogue of the Officers and Students, and Programme of the Course of Instruction, of the Massachusetts Institute of Technology.

THE CHEMICAL NEWS.

Vol. XXIII. No. 585.

ON THE DETERMINATION OF SULPHUR IN CAST-IRON.

By ARTHUR H. ELLIOTT.

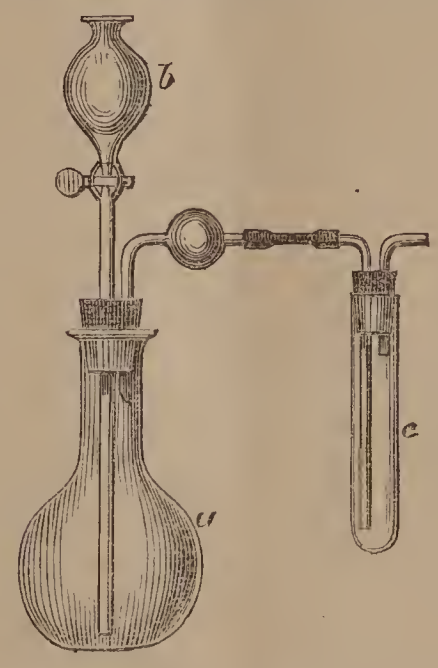
Most of the methods at present in use for determining the sulphur in cast-iron are worked so as ultimately to obtain the sulphur as sulphuric acid, and precipitate the latter as sulphate of barium. In some methods, this end is attained by treating the iron with acid, eliminating the sulphur as sulphuretted hydrogen, catching the latter in an alkaline solution of lead or zinc, and, after collecting the metallic sulphide formed, oxidising it by fusion or treatment with acids. In other methods, the sulphur is oxidised direct in the iron, without previous separation from the latter. There are many objections to these methods. In the first, the operation of fusing is well known to be a troublesome experiment; and, to add to the number of preliminary operations, the oxide of the metal soluble in alkalies, left in the solution of the fusion of the metallic sulphide, has to be separated previous to precipitating with chloride of barium. In the second case, of oxidising direct by treatment with acids, a tedious evaporation is incurred to separate silicic acid before precipitating with chloride of barium. Again, when a number of analyses have to be performed, the precipitating with chloride of barium becomes an operation it would be well to dispense with if possible.

To meet these objections, and place in the hands of chemists a ready, and, at the same time, accurate, method for the determination of sulphur in iron, I have devised and used the following process, which I find is expeditious and gives concordant results. It depends on the evolution of the sulphur as sulphuretted hydrogen, the absorption of the latter in solution of soda, and determination of sulphuretted hydrogen in the acidified soda solution with standard solution of iodine.

Preparation of the Standard Iodine Solution.—Dissolve about 5 grms. of iodine, with about 7 grms. of iodide of potassium, in 1000 c.c. of water. Now prepare a solution of hyposulphite of sodium containing about 25 grms. of the salt in 1000 c.c.; this is used to standardise the iodine by means of standard solution of bichromate of potassium. The standard bichromate is made by dissolving 4·92 grms. of the pure fused salt in 1000 c.c.; 1 c.c. of this solution is equal to 0·0127 of iodine. To find the value of the iodine solution, proceed thus: Take 20 c.c. of bichromate solution, add 10 c.c. of solution of iodide of potassium (1 in 10), and about 15 c.c. of hydrochloric acid (1 part of strong acid to 2 parts water). Now add from a burette the hyposulphite solution, till the solution in the beaker is greenish yellow; add to this a little clear starch-paste, and continue adding the hyposulphite till the blue colour produced disappears, leaving a clear green solution. You will now have the value of the hyposulphite in terms of iodine. From this, standardise the iodine solution, by taking 20 c.c. and adding the hyposulphite till one drop makes the solution colourless. The calculations are made as follows:—Suppose 20 c.c. of bichromate are equal to 21·8 c.c. of hyposulphite, and 20 c.c. of iodine are equal to 7·6 c.c. of hyposulphite. Then, as 20 c.c. of bichromate are equal to 0·254 of iodine, 21·8 c.c. of hyposulphite are equal to 0·254 of iodine; therefore 1 c.c. of hyposulphite is equal to 0·01165 of iodine. As 7·6 c.c. of hyposulphite are equal to 20 c.c. of iodine solution, 20 c.c. of the iodine solution are equal to 0·08854 of iodine; therefore 1 c.c. is equal to 0·004427 of iodine. But, according to the following reaction, $H_2S+I_2=2HI+S$, I_2 is equal to S, or, 254 of iodine are equal to 32 of sulphur. Therefore, by further calculation, 1 c.c. of iodine solution is equal to 0·000557 of sulphur.

The Process.—The apparatus is used for dissolving the iron and catching the sulphuretted hydrogen formed. The narrow-necked flask, *a*, holds about 500 c.c.; the funnel-tube, *b*, has a bulb holding about 70 c.c., and is provided with a glass tap. The exit-tube from the flask has its end ground obliquely, to allow the liquid condensing in it to drop back into the flask without stopping the passage; it is also provided with a bulb to catch the condensed liquid, and prevent it passing into the absorption-tube. The tube, *c*, is about 10 centimetres long, and 18 m.m. wide; it is provided with tubes, one passing to the bottom, the other an exit-tube. This tube is of thick glass, and is one of those usually sold to keep specimens. It is half filled with solution of soda, made by dissolving 1 part of the soda made from sodium in 5 parts of water; this solution does not use up any iodine after being acidified and mixed with starch paste. A second tube, containing a strong solution of iodine in iodide of potassium, may be connected with the tube containing the soda, to absorb the phosphoretted hydrogen, which has a very disagreeable effect upon the operator.

Five grms. of the finely-powdered drillings of iron are taken, and transferred to the flask by means of a piece of



glazed paper. The paper containing the iron is coiled up so as to form a cylinder; it is then placed in the neck of the flask whilst the latter is held horizontally; the flask is then turned up vertically, and the iron allowed to fall in, retaining the paper with the finger; the last particles are removed from the paper by tapping the neck of the flask. After adjusting the cork containing the funnel-tube, and attaching the absorption-tube, 70 c.c. of dilute hydrochloric acid (1 part of strong acid to 2 parts of water), are poured into the bulb of the former, the glass tap being closed. A little acid is now allowed to enter the flask, and the contents of the latter are thoroughly mixed by shaking. When the action has abated, more acid is allowed to enter, the contents of the flask being mixed by shaking at each addition. When the iron is thoroughly decomposed, place a gentle flame under the flask, regulating the heat by the rate of bubbling through the absorption-tube. When the contents of the flask boil, keep boiling for some minutes, until they threaten to boil over. Now remove the lamp from under the flask, and, simultaneously, open the glass tap of the funnel-tube. Care should be taken that the tap works easily before beginning the experiment. Remove the absorption-tube from the rest of the apparatus, wash the contents into a beaker holding about 500 c.c., dilute with about 200 c.c. of boiled water,* acidify with hydrochloric acid, add a little clear starch

* The reaction between the sulphuretted hydrogen and iodine cannot be depended on if the solution contains more than 0·04 per cent of sulphuretted hydrogen.—Bunsen.

paste, and titrate with the standard solution of iodine till blue.

Phosphoretted hydrogen, which is also formed on treating the iron with acid, is not absorbed by solution of soda. I proved this before proceeding to work with the above process.

The following determinations were made by this method on two samples of iron :—

A. Per cent S.	B. Per cent S.
0·136	0·077
0·120	0·072
0·136	0·080
0·119	0·076
0·130	0·085
	0·083
	0·081

When the solutions above mentioned are already prepared, this process will be found very expeditious. Having the standard solutions of bichromate and hyposulphite, it is easy to prepare a standard solution of iodine at any time. When once prepared, the solution of iodine keeps correct for months if preserved in a dark bottle and in a cool place.

My sincere gratitude is due to Mr. Vacher, for his kindness to me whilst pursuing these experiments in his laboratory.

NOTES ON THE

EFFECTS OF COLD UPON THE STRENGTH OF IRON.*

By WILLIAM BROCKBANK, F.G.S.

The severity of the present winter has brought the question of the effects of low temperatures upon the strength of iron very prominently before the public; and it is a curious circumstance that a subject of so great importance should have escaped the attention of writers on iron to such an extent, as that it is either ignored, or dismissed with a few brief remarks or inconclusive experiments, which leave the subject altogether unsettled.

After referring to the observations and experiments on the effects of low temperatures on the strength of cast- and wrought-iron in the works of Sir W. Fairbairn, Dr. Percy, and David Kirkaldy, and pointing out the inconclusiveness of all the experiments hitherto recorded, the writer went on to detail the following experiments, which he had, by the kindness of the several parties named, caused to be made during the severe frosts which have recently prevailed, and which have in all cases been carried out with the greatest care and exactness.

Experiments on the transverse strain of cast-iron bars were made at the works of Messrs. P. R. Jackson and Co., of Salford, and were repeated thrice, with the following results :—

In Mr. Fairbairn's experiments, only one sort of pig-iron was employed. It is now well known that a much sounder and more regular casting can be obtained by a judicious admixture of several kinds of pig-iron ; a very probable source of error would thus occur in Mr. Fairbairn's experiments, and this will probably point to the unsatisfactory results he obtained.

The bars employed in the present experiments were made from a mixture of four pig-irons of the highest class, added to some good scrap-iron ; they were all poured from the same ladle, and were moulded from the same model, and they were remarkably regular in size and quality, so that the results may be fairly relied on. The castings were all made on Friday, the 30th of December last, and the bars were tested on the following Tuesday, January 3rd, 1871. The machine used was a powerful lever or steelyard, the bars having a 3-foot bearing, and the results were taken with all possible care, and are detailed in the following table.

Experiments upon the transverse strain of cast-iron at low temperature, made at the works of Messrs. P. R. Jackson and Co., Salford, January 3rd, 1871, by W. Brockbank, F.G.S.

The mixture of metals was Cleator hæmatite, Pontypool cold blast, Blaenavon cold blast, and Glengarnock hot blast pig-iron, with some good scrap-iron. All the bars cast from one ladle.

The results show a gradual and considerable decrease of strength in the bars, with the increase of cold below the freezing-point. They also lost their elasticity in a similar degree.

A further trial was made at Messrs. Jackson's Works with similar bars and the same admixture of metals, on January 10th, as detailed in the following table. One bar was cooled to a temperature of 15° by a mixture of snow and salt.

No.	Size of Bar.	Deflection.	Breaking Weight.	Average.	Temperature.
		ins.	lbs.	lbs.	°F.
1	3 ft. long between bearings by 1 in. by 1 in.	0·6875	780	780·00	15
2		0·7500	815	844·30	35
3		0·7600	840		35
4		0·8125	878		35
5		0·7500	845	859·25	52
6		0·6875	855		52
7		0·8125	867		52
8		0·8125	870		52
9		0·8800	893	893·00	70

These experiments are borne out by the general experience of ironfounders, many instances having come to my knowledge during these investigations, a few examples of which may be cited.

(1). I find it a matter of general opinion that pig-iron breaks much more easily in frost than in ordinary temperatures, and that breakages of castings are much more frequent in frosty weather.

(2). In rolling mills, and particularly where chilled rolls are employed, especial care has to be used in frosty weather to warm the rolls before using them, and when in use to keep them carefully covered from the frosty air. If not properly protected and carefully managed, they are found to be very liable to fracture.

(3). Mr. Edgar Gilkes, of Middlesborough, informs me that the cast-iron wheels of the Chaldron waggons of the Stockton and Darlington railway are found to fracture very frequently in frosty weather, and in a severe frost it is sometimes quite a serious matter.

(4). Messrs. Peel, Williams, and Peel had a remarkable example on January 5th (20° F.). A hydraulic cylinder had been cast upon a cast-iron hollow core bar 7 inches in diameter and 1¼ inches thick, coated with 1½ inches of loam and hay. It was put out in the yard to cool during the severe frost, and when they came to draw the core bar it broke by the mere torsion, and was found to be quite brittle. A portion of this core bar was warmed, and it was then found to have recovered its nature and to be quite strong and tough. The lowest temperature on this date was 19° F., and the casting was exposed to it for many hours. Numerous other examples could be readily furnished if required.

There can, therefore, be no doubt whatever that the strength of cast-iron is very materially lessened by severe cold.

For experiments in wrought-iron I am indebted to many friends, and the results are of similar import. My first experiments were directed to the method adopted by Mr. Kirkaldy, and I soon found that neither by torsion nor gradual tensile strain could the true result be ascertained,

* Read before the Manchester Literary and Philosophical Society, January 10th, 1871.

as the bar almost immediately became heated under the strain, and the effects of frost at once disappeared. The following experiments made by Mr. William Johnson, of the Messrs. Johnson's Ironworks, Bradford, near Manchester, will illustrate this conclusively.

A coil of galvanised wire, 5½ B.W. gauge, was left in the open air for twenty-four hours during severe frost, December 24th, 1870; twenty-four pieces, 1 yard in length each, were then cut off. Of these, six were tested for tensile strength by the direct application of weights, and six for torsion; the same tests were used for the remaining twelve after they had been warmed to about 80°. The results were as follows:—

	Tension.		*Torsion.*	
	At 20°.	At 80°.	At 20°.	At 80°.
	2142 lbs.	2142 lbs.	16½ twists.	14½ twists.
	2114 ,,	2058 ,,	15½ ,,	14½ ,,
	2114 ,,	2086 ,,	9 ,,	13½ ,,
	2142 ,,	2086 ,,	14½ ,,	14½ ,,
	2114 ,,	2128 ,,	16 ,,	12½ ,,
	2114 ,,	2086 ,,	18½ ,,	14 ,,
Total..	12740 ,,	12586 ,,	90 ,,	83½ ,,
Average	2123·3 ,,	2097·6 ,,	15 ,,	13·9 ,,

Thus, in both experiments, the iron tests worse when warm than when frozen. In each case the wire immediately became warm. Mr. F. Monks, of the Whitecross Wire Works, Warrington, also tested wire rods for me with precisely similar results. Finding these experiments to be unsatisfactory, I arranged for a series to be tried by the more rough and ready method of the striker's hammer, which I judged would be more likely to show the true state of the iron in its frozen condition. The result, either of gradual torsion or tension, is to expel the frost there may be in the bar almost immediately, so that in the further progress of the trial there is no difference between bars which were originally either cold or warm.

If low temperatures have any influence in rendering iron weaker or more brittle, the only way in which the amount of such influence can be realised is by a sudden impact, and the striker's hammer was the readiest appliance for the purpose. In the following experiments, great care was taken that the blows should be as nearly as possible of the same force in each trial, and as the experiments were all carefully conducted, and are vouched for by the parties named, they may be fairly relied on as representing truly the facts of the case.

(1). William Bouch, Esq., C.E., Engineer of the Stockton and Darlington Railway, made the following experiment December 29th, 1870, the temperature at the time being about 26°, but it had been as low as 19° over night.

A bar of round iron, 1½ inches in diameter, of best quality, was taken from the yard, being then coated with ice; it had been exposed to a week's hard frost. It was held over the edge of a smith's anvil, and one blow from a 12 lb. hammer by the striker broke a piece 4 inches long, short off, the fragment flying 12 yards along the floor of the workshop. The same bar was then put into the mouth of a furnace, but not in contact with any flame, for a short time, to unfreeze it. The heat received into the bar was so moderate that a smith could grasp it with his hand. It was then allowed to lie on the floor for some time, until it had quite cooled down to the temperature of the workshop. It was now placed on the anvil, and the same striker as in the first experiment, with the same hammer, gave fourteen blows without causing the slightest fracture, the bar being merely bent about two inches. Mr. Bouch adds that he has, in his experience, met with many cases nearly as convincing as the above.

(2). Mr. Robert Peel, of Messrs. Peel, Williams, and Peel, Manchester, has kindly made for me the two following experiments with boiler-plate iron, as shown by the samples now on the table, viz.:—

No. 1. A strip of boiler-plate of best best quality was taken from the open yard, where it had lain during several days of severe frost, January 5th, 1871; temperature about 20° F.

It was laid across the anvil, and a striker, with a single blow of a 14 lb. hammer, broke off the piece now exhibited.

The fracture shows a very "short" crystalline face, without any appearance of fibre, and is torn and irregular, in remarkable contrast to the sample No. 2, which is from the same piece, viz.:—

No. 2. The remainder of the above strip was slightly warmed to dispel the frost, and then allowed to cool to the temperature of the shop. It required several blows from the same hammer, and bent considerably before breaking, being exceedingly tough and fibrous.

The fracture shows a good fibrous structure, except on the inner side of the curve, where there is a thin crystalline skin.

The difference of appearance in these two fractures is very striking and remarkable, and can only be accounted for by the action of extreme cold.

No. 1*a*. This experiment was made on January 6th; temperature about 26° F. A strip of Low Moor best best boiler plate was taken out of the snow, having lain there during several days of intense frost. It was laid across the anvil, and broken off short with a single blow from a 14 lb. hammer. The fracture is fibrous, but with patches of crystals, especially on the edges of the plate; the general appearance is "short" or "tender," very different from the usual character of Low Moor iron in its normal state.

No. 2*a*. The remainder of the same strip was placed in the drawing office at a temperature of 70°, and allowed to lie there for some hours. It then required six blows from a 14 lb. hammer, the plate being reversed each time, the grain being thus severely bent backwards and forwards, under heavy blows, before it severed. The outer skin still remained in cohesion, and it had to be separated by bending backwards and forwards in the smith's hands. This fracture shows a splendid quality of iron, the fibre being bent in both directions as the blows were alternately reversed. There is a slight crystalline line on the skin of one side.

Mr. F. Monks, of the Whitecross Wire Company, Warrington, has kindly made the following experiments with wire billets, which are the very toughest form of iron manufactured. The wire exhibited is made from one of the same bars, and will clearly show the quality of the iron.

The billets are 1¼ inches square, being in the semi-manufactured form ready for the final heating and re-rolling into wire. They had been lying in the open air several days during severe frost. Experiment tried January 1st, 1871; 10° lowest to 30° highest temperatures.

Three bars were broken in the open air. They failed to break with twenty-two blows with a 15 lb. hammer. A small nick ⅛ inch deep was then cut, with three light blows on a "set," in the top of each bar, and at another part of it, after which a single blow sufficed to break each bar.

The bars were then thawed and allowed to cool to the usual temperature, or about 70°. Twenty-two blows were given to each as before, and the same nick was made on one side as nearly as possible like the frozen bars had been treated.

One bar then broke after eleven blows, one after ten blows, and one after six blows.

The frosted bars are more crystalline, and show no signs of fibre; the other bars show a good amount of fibre, and are slightly crystalline in the fractures.

The following experiments with rails were made at the works of the Darlington Iron Company, November 30th, 1869:—

The rails were taken promiscuously from a lot of 1000. all supposed to be of the same quality, weight, and exact

section. It had been found that the rails which were then in course of manufacture for the East-Indian Railways at these works, and which were of a very high quality, failed to pass the required test in frosty weather, whereas in ordinary temperatures a failure was a very rare occurrence. The ten rails were accordingly selected to settle the question whether higher and lower temperature affected the strength of the rails. Four rails were heated up to 120° F.; the other six were tested cold, the temperature of the atmosphere being about 26°.

Test of East-Indian Railway Rails, 82 lbs. per yard, November 29th, 1869, tested by a Falling Weight of 2000 lbs.; Centres of Support, 3 feet 6 inches apart.

No.	No. of blows.	Height of fall. ft. in.	Permanent set.	Temperature.	Remarks.
1	1st blow	5 0	7-16ths	120°	Not broken.
	2nd ,,	5 0	3-4ths		
	3rd ,,	7 0	—		
2	1st ,,	5 0	3-8ths	Do.	Ditto.
	2nd ,,	5 0	13-16ths		
	3rd ,,	7 0	—		
3	1st ,,	5 0	3-8ths	Do.	Ditto.
	2nd ,,	5 0	13-16ths		
	3rd ,,	7 0	—		
4	1st ,,	5 0	3-8ths	Do.	Ditto.
	2nd ,,	5 0	7-8ths		
	3rd ,,	7 0	—		
5	1st blow	5 0	3-8ths	26°	Broke with 2nd blow.
	2nd ,,	5 0	broke		
6	1st ,,	5 0	3-8ths	Do.	Passed test.
	2nd ,,	5 0	5-8ths		
7	1st ,,	5 0	3-8ths	Do.	Broke with 2nd blow.
	2nd ,,	5 0	broke		
8	1st ,,	5 0	3-8ths	Do.	Ditto.
	2nd ,,	5 0	broke		
9	1st ,,	5 0	broke	Ditto with 1st blow.	
10	1st ,,	5 0	3-8ths	Ditto with 2nd blow.	
	2nd ,,	5 0	broke		

At 120°, all the bars stood two 5-foot blows and one 7-foot blow.

At 26°, only one bar stood two 5-foot blows, three broke at the second 5-foot blow, and one at the first 5-foot blow.

At 60°, all would probably have passed the test easily, many thousands having previously done so from the same lot.

It will therefore be seen that the results are in perfect agreement in all these experiments, showing that bar-iron, boiler plates, wire billets, and rails are most materially weakened by the action of intense cold, losing all their toughness, becoming quite brittle under sudden impact, and having their structures changed from fibrous to crystalline.

Similar instances could be given in illustration of this in the daily practice of engineering. In large works, the breakages of wrought-iron are very considerable during frosts. Quarrymen find that their chains are very liable to fracture from the same cause; and doubtless the numerous accidents of failing tires in our railways may be attributable to it. In many cases, however, the contraction of iron must also be taken into account, as it is a serious item.

In conclusion, I think it cannot be doubted, after the above recital, that iron does become very much weaker, both in its cast and wrought state, under the influence of low temperatures. This subject is one of such paramount importance, that a careful series of investigations ought to be undertaken by one of our scientific bodies, to ascertain the precise nature of the changes which are thus shown to take place, as there is herein an item which materially affects the stability of all iron structures during frosty weather, and which has not hitherto been adequately recognised.

ON THE

RELATION EXISTING BETWEEN CHEMISTRY AND MINERALOGY.

By Dr. C. RAMMELSBERG.

Nobody doubts, nowadays, that all the properties of a substance are very closely connected together; and since isomorphism was discovered to exist, some fifty years ago, every chemist knows that the crystalline form of a substance is intimately connected with the chemical nature of the body. The crystalline form is the result of a juxtaposition of molecules according to properly-defined symmetrical directions. That form defines, in the interior of the body, the directions according to which cleavage takes place, while differences of hardness, as well as other physical properties, depend also upon the specific crystalline form; while we need not doubt that the optical properties of the bodies are very closely allied to the form just alluded to, because we know that crystals possessed of one optical axis obey totally different laws of symmetry than do crystals having two optical axes; while it is also plain that crystals belonging to the regular system, in which we do not distinguish upper or under, front and back, left and right side, are on all sides symmetrical and singly refractive. Is not circular polarisation bound to, and intimately connected with, the existence of peculiar forms of the crystals, on which we distinguish a difference of right and left? All agree that the thermal, electric, and magnetic properties of bodies are connected with the geometrical figures they exhibit, while the volume-density (specific gravity) and specific heat of bodies are most intimately connected with the chemical nature and properties thereof. The perception that there exists of necessity a connection between all the properties of a body, and the fact that the sum total of the properties is required for determining the difference of that body from all others, is not of very remote date. It was for a great length of time the criterion of chemistry that it only occupied itself with the material nature and combinations of substances, while chemists only studied the composition and reaction of bodies, and cursorily added thereto some few particulars on colour, fusing-point, boiling-point, and specific gravity. In our days, however, this is not deemed sufficient; and it is by far more important for the thorough building-up of science that a limited number of substances should be thoroughly studied and investigated in all their relations and all their properties, than that a very large number of bodies should be only imperfectly and fragmentarily known, involving the additional trouble of having to return to complete these studies afterwards. It is the especial duty of the chemist to study bodies in all their relations; and therefore, according to what is accepted as a rule, these properties embrace those termed geometrical, physical, and chemical, and only after having mastered all these is it possible to say that we know the substance. It is, however, clear that it is beyond the limits of the ordinary capability of the human mind to master all the subjects thoroughly; while, moreover, not all the properties are of the same value, it having been found that by far the most important properties of substances are those termed chemical and geometrical, next to which stand the optical properties. Crystalline form and composition are therefore to be investigated in the first place, and jointly because they are the foundation stone, as it were, of the whole of the rest of the properties. A chemist ought to be just as expert in measuring and calculating the angles of crystals as he is capable of making analyses and of testing substances. The late Dr. Mitscherlich became the discoverer of isomorphism and heteromorphism because he was a crystallographer, and M. Pasteur would never have made his brilliant discoveries if he had not known that the rhombenoctahedron yields two enantiomorphous tetrahedrons. I think it is very desirable that our young chemists should become well acquainted with crystallo-

graphy, which is too much neglected at present, so as to enable them to study the relations existing between chemical constitution and crystalline form. If it be alleged, against this proposition, that many other celebrated chemists, the late Berzelius, for instance, never gave any proof of his acquaintance with crystallography, my reply thereto is that, far from being any addition to, it rather was a defect of his merits; and, moreover, times have so changed that we may safely assert that, in the first place, universal chemists will be no more heard of, and, secondly, no sound knowledge of chemistry can exist without crystallographical knowledge combined. Mineralogy is truly a German science, which has sprung into existence from mining and metallurgical operations; for a long time this science was in a rude state, and bore an empirical character, only taking cognisance of a very few external properties, remaining in this condition even after Werner's days. Romé de l'Isle and Hauy discovered the mathematical laws of the crystalline forms, while the latter, very materially aided by Vauquelin, studied and investigated the physical properties of the crystals, which were chemically, also, analysed by the last-named *savant*. But the parties just named, and their successors—viz., MM. Weiss, Mohs, and many others—were only capable of studying minerals one-sidedly, because they were only crystallographers; and those on the other hand, who, preceded by Klaproth, investigated the chemical nature of minerals, were unacquainted with the methods of studying the geometrical properties of these bodies. This state of affairs has been at the root of an endless number of errors in mineralogy; because, what had been studied by one, and analysed under the same name by the other, frequently turned out to be thoroughly different substances, and the division of labour has in this instance impeded and greatly impaired the progress of mineralogy.

What are minerals? Chemical combinations which, either sporadically or in large masses, constitute the solid structure of our globe. These compounds may, and can be artificially prepared, as is the case with all so-called artificial compounds; these compounds are formed without our aid, just as is the case with those formed in organised bodies.

Is there any difference between the crystals of sulphur deposited from solutions and those obtained from Sicily and from the Solfatara? Is the oxide of iron obtained from the fumaroles of the lava of the Vesuvius different from that which has been obtained in crystals artificially? Is the felspar obtained by some processes of copper smelting different from the native mineral? All these native compounds may be obtained synthetically by artificial means. The materials mineralogy has to deal with are limited to a small number of chemical combinations; the properties we mineralogists have to investigate and the methods of research are exactly identical with what obtains in chemistry. Nobody is, in the true meaning of the word, a mineralogist, unless he be equally capable of making a chemical analysis of his minerals as of determining crystals exactly. It was a curious blunder made by M. Mohs, when he tried to prove that minerals, like animals and plants, were only to be classified according to their external appearances, and that chemistry had nothing whatever to do with them. This opinion Mohs, a celebrated and very eminent crystallographer, was led to because he know nothing whatever of chemistry, and because the discovery of isomorphism had remained unknown to him; his so-called natural history mineral system is perfect nonsense. But while we speak in these terms about Mohs, we are in justice bound equally to condemn the error of the late Dr. Berzelius, who tried to set up a purely chemical mineral system, simply based upon the chemical composition of the minerals. It is sufficient to say that heteromorphism alone is sufficient to prove the utter impossibility of such a proceeding.

We have at present a so-called mixed system, that is to say, a more or less successful plan of grouping minerals in accordance with crystalline form and composition. Might it be allowed to call chemistry and mineralogy one science? It is very possible that, by so doing, many a so-called mineralogist, who knows nought about chemistry, might feel offended. Even if the distinctive name of mineralogy be preserved, it is clear that it will be entirely absorbed by chemistry, because the mutual relations of minerals towards each other, and their modes of formation and decomposition have long since become treated as separate sciences under the names of geognosy and geology. It was said, very many years ago, by the late Dr. Berzelius, "mineralogy is the chemistry of those compounds which are met with in the native state." The domain of chemistry is, however, far more extensive; but the properties we determine for every single substance are the same, and so are the methods and means we employ. Since the last quarter of a century, chemistry has made enormous progress, not only in the collection of a mass of facts, but also in speculative matters. It is self-evident that this progress has operated advantageously on mineralogy; and it is certainly the duty of all mineralogists to apply the facts and discoveries of chemistry, obtained and made in every direction, to their department of science. Some of the views formerly held by chemists have been entirely modified; but it is superfluous to say that the views held at the present day are simply due to the enormous and almost incredible increase of facts.

The following are only a few, yet essential, portions of the present magnificent structure of chemistry:—The full recognition of the law of Avogadro; the sharply-drawn distinction between the idea of atoms and molecules; the doctrine of atomic weights; the quantivalence and equivalence of the elements; the analogy of the constitution of all acids, bases, and salts. I need not put the question whether there are at present living any chemists who yet hold the old views? Suffice it to say that no chemist of any standing would dare to defend these views; but, when chemists adopt certain views, theories, and hypotheses, because these better agree with facts, nobody also considers that these views are anything else than a step forwards in the right direction, which in future may give way to better. That which the labour, zeal, and acumen of a large number of industrious scientific men has built up during the past twenty-five years must undoubtedly be designed *progress*. How is it that we meet here and there mineralogists who speak with disdain of these things, and who, declining to have anything to do therewith, stick to the old views, taking these new-fangled ideas as so much useless change? Just as in the domain of chemistry there is a predilection for cultivating and extending the organic portion, so in mineralogical science are geology and geognosy preferred; while, as regards the mineralogy proper, only a few cultivate it, and these are chiefly occupied with the morphological, not with the chemical portion. It is clear that these mineralogists will only apply to their purposes such researches of chemistry as directly relate to minerals; but they are incapable of understanding the real bearing of modern chemistry, and of making such changes in the science they cultivate as are rendered necessary by the progress of chemistry. The chief objection made is against the formulæ; these, as regards minerals, were, since they were first introduced by the late Dr. Berzelius, either empirical or rational. The former only indicate the kind and the relative number of the elementary atoms met with in the compound; and, as the old atomic weights are yet in use, the empirical formulæ are also those of older date. $CuFeS_2$ = copper pyrites, $CaSiO_3$ = wollastonite, $CAAlSi_2O_8$ = anorthite, $BaSO_4$ = heavy spar, are even yet the empirical formulæ of these bodies. It is evident that the changes made in many of the atomic weights has brought about considerable changes of formulæ:—Fluor spar = $CaFl_2$, instead of CaFl, as formerly; leucite = $K_2AlSi_4O_{12}$, instead of $KAlSi_4O_{12}$; orthoclase = $K_2AlSi_6O_{16}$, instead of $KAlSi_6O_{16}$, &c.

As regards the rational formulæ, the matter is different, because they expressed a definite view as regards the

mutual arrangements of the atoms in the molecule, as it was formerly surmised that the oxy-salts were the products of the addition of base and acid, the formulæ were arranged accordingly. $CaO+SiO_2$, $BaO+SO_3$, also—

$$(2CaO+SiO_2)+(2AlO_3+3SiO_2)=\text{anorthite},$$

were the expressions at one time generally in use as the result of a hypothesis universally adopted in chemistry. This hypothesis has been set aside by chemists, not for the mere sake of change and novelty, but upon sound grounds; another hypothesis has been substituted, and according to it rational formulæ are now made up. Whether we write the formula of sulphate of baryta—

$$\left.\begin{matrix}Ba\\(SO_2)\end{matrix}\right\}O_2,\ \text{or}\ Ba.O_2.(SO_2),$$

or any other way, we desire to express thereby that one molecule (two atoms) of oxygen is combined on the one hand with an atom of barium, and on the other hand with the atomic group SO_2 (the radical of sulphuric acid), exactly agreeing with the bodies—

$$\left.\begin{matrix}H_2\\(SO_2)\end{matrix}\right\}O_2=H_2.O_2.(SO_2)\quad\text{and}\quad\left.\begin{matrix}H_2\\Ba\end{matrix}\right\}O_2=H_2.O_2.Ba\ ;$$

that is to say, the acid and the base, by the reaction of which upon each other, and elimination of the equivalents, $H_2=Ba$ or $H_2=SO_2$ is produced. In the same manner, carbonate of lime is represented as—

$$\left.\begin{matrix}Ca\\(CO)\end{matrix}\right\}O_2=Ca.O_2.(CO),$$

and silicate of lime as—

$$\left.\begin{matrix}Ca\\(SiO)\end{matrix}\right\}O_2=Ca.O_2.(SiO_2).$$

Everyone will admit that these views are distinct from those formerly held. Since chemists have adopted these methods of expression, heavy spar cannot be indicated as $BaO.SO_3$, wollastonite not as $CaO.SiO_2$, calc spar not as $CaO.CO_2$; because, whoever would apply these formulæ nowadays in mineralogy, ignores the present advanced state of chemistry altogether. We may say, however, that there is no fear that this will happen, because a mineralogist ought to understand enough of chemistry to know the grounds upon which the changes have been made by chemists. Now and then, we hear it said by mineralogists that it is not yet proved whether silicic acid anhydride is SiO_2 or SiO_3, because the isomorphism of the double fluorides of Si, Ti, Zr, and Sn does not carry to them sufficient proof for SiO_2. How is it that they do not know that the formulæ, $SiCl_3$, $SiFl_3$, do not harmonise with the specific vapour-densities of these bodies? Are not the discoveries of silicide of hydrogen, and of those compounds of Si, H, and O which are analogous to organic compounds, proofs of the equivalence of carbon and silicium? No chemist of repute will, therefore, seriously use the formula SiO_3. Notwithstanding the ready adoption of new facts by many, it appears that it is very difficult for older men, and especially for mineralogists, to give up their old views; but, as well observed by Professor Streng, they are, however reluctantly, compelled to follow and do suit and service to the new facts brought to light by the chemists.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from p. 29.)

Lecture IV.

We had occasion last time to notice the effect of the atmosphere on processes of fermentation in several instances. I mentioned, among other things bearing on that question, an experiment of Gay-Lussac, in which he squeezed some very ripe berries of the grape under mercury, and kept them, with due precautions for the exclusion, as far as he knew, of everything except the grape-juice; he kept this expressed juice for some time quiescent, and then introduced a bubble of air, or a bubble of oxygen, the active substance of air, but he subjected the air or the oxygen, before introducing it into this juice, to various strong influences, which must have destroyed any vital organism in it; and he found that the mere addition of the air to the quiescent juice caused a process of fermentation to commence and a formation of organisms to begin, that they developed themselves, and that the liquid fermented in the usual way. The fact of the fermentation commencing is, if we bear in mind the general results of M. Pasteur's researches, to be attributed to the presence in the mercury or in the grape-juice, or somewhere or other in the substances present, of bodies which, by the mere access of oxygen, were stimulated so as to develop themselves into these little vital cells. It is now known, I may say, that there are in mercury, unless it is purified with extraordinary precautions, always present some such organisms, capable of developing themselves under such influences; and, it is probable, I will not say more than that, for I do not know, that in the grape-juice there may also be similar germs present. The functions of oxygen appear from that experiment—which has since been confirmed by other observers—to be essential, at all events, to the initiation of the process, and there is, in that respect, a remarkable analogy, which I think is interesting to recall to mind, with the action of oxygen on other bodies, as shown by an experiment made by Humboldt many years ago. He got some grains of wheat from Egyptian mummies, which had been so long at rest that they were not inclined to grow; in fact, they could not be got to grow in the ordinary way. However, he stimulated them to activity by immersing them in a little chlorine water. It is well-known to chemists that chlorine in the presence of water does oxidise, or cause the oxygen to separate and pass over to common organic substances capable of combining with it. Humboldt actually stimulated these sleepy wheat grains to life, so that they grew and germinated, and their descendants are still in existence, by the mere action of oxygen developed in that way.

In the processes of wine-making and wine-keeping, the presence of air is one of the most important matters which have to be considered, and there has prevailed, and I ought to say there still prevails, to a certain extent, a difference of opinion regarding the functions of oxygen in these processes. On the one hand, it is known, as a matter of fact, that processes of fermentation are performed under conditions such as that air has access to the substance. No actual wine or beer-making has yet been performed on a large scale on such conditions as to exclude oxygen. On the other hand, the experiments of Gay-Lussac established clearly that it is necessary. In some cases, however, in wine-making, it has been thought desirable to facilitate the access of air to the substance; while other wine-makers think, on the contrary, that in the first process as little air should be present as possible; but there has always been some. The juice first expressed from the grapes has been very carefully examined with regard to the gases contained in it. If air has access to it, it is always necessary to know, in order to judge whether the air acts upon it, whether the air is dissolved by it, and whether, if dissolved by it, it is still to be found in the grape-juice as such, or whether it has undergone combination. Now, every case of the examination of must, or fresh grape-juice, which is not fermented, has shown that it contains a considerable quantity of gas, but no case has been established of free oxygen being present in it. Carbonic acid gas is present in it in a considerable quantity, and also nitrogen, in proof that air had had access to it, but the oxygen which was taken up at the same time with the nitrogen from the air, was not to be got out from that must again. It had been taken up, and it had entered into combination with the substance, so that all the oxygen present was actually combined chemically with it. In that respect a good many observations have been made by

* The Cantor Lectures. Delivered before the Society of Arts.

various chemists, but I ought especially to quote those of M. Pasteur, which are exceedingly careful and valuable. He has shown that this substance not only eats oxygen, but digests it. The oxygen is not to be found in it as such. It is only present in the form of a compound, which is formed by its action on the organic matters there present. Then, when the wine-juice has been expressed, and when it has been allowed to remain some time in a suitable place, so as to undergo fermentation, with a considerable variety of treatment in different places with regard to air, for, in some places it is thought desirable that the fermentation should be allowed to take place in open vessels, or in vessels to which the air can have access as freely as possible, whereas in other cases special care is taken to cover as completely as possible the vessels in which the fermentation is taking place, so that the air may have as little access as possible to the fermenting substance,—and I believe it is impossible to give any one general rule with regard to the best process for all cases of fermentation, because the materials which are subjected to fermentation vary so considerably; they differ from one another in their composition so materially, and there are also other circumstances which are different,—for instance, the temperature, which has an important influence. Not only is the temperature in some localities higher than in others, but other circumstances are also different, and it would not be right to say, because air is found to be perfectly useless in some well-established cases during fermentation, that for that reason it ought to be excluded, or even that it may be excluded, in all other cases of apparently similar fermentation. As far as a general rule can be laid down from present experience, I think it does appear certain that oxygen plays no part in the process after the first expression of the juice. Once the fermentation has commenced, it appears to go on as well if air is excluded from the substance as if air has access to it. There is, however, one circumstance which is considered by persons of considerable experience to be important in this matter, and which I ought, therefore, to mention, viz., that when fermentation takes place at a low temperature—and some fermentations are, with great care, kept at a low temperature—the products are found to be superior if the whole process is carried on, the temperature being kept exceedingly low, and, in those cases, it appears that an open vessel is certainly not in any degree detrimental. It is customary, in fact, to use an open tub when the temperature is low; and, on the contrary, it is usual to use a partially closed vessel, of course allowing for the escape of carbonic acid, when the temperature is comparatively high. When the first vinous fermentation has completed itself, it is customary, in the wine-growing countries, to put the still active liquid into casks, and the slower process of fermentation then goes on, which lasts a considerable time. During this second fermentation, there is very much the same kind of condition present in the first, and there is always formed, in this subsequent fermentation, a considerable quantity of deposit, which is afterwards removed with much care; either the supernatant liquid is carefully decanted, or, in some cases, it is removed by a process of rough filtration. The subsequent treatment of the wine, I mean the keeping of it in casks or cellars, and the subsequent keeping in bottles—and these two processes of keeping it in casks and keeping it in bottles are quite distinct,—are not usually considered as forming part of the process of wine-making. It appears, however, from the investigations of M. Pasteur, that changes take place in the composition and the materials by these processes, which really are as essential to the composition of the product as any other part of it, and that they ought to be considered as later parts of the process of wine-making. In fact, the process of wine-keeping is, in theory, not to be separated from the process of wine-making, the keeping being a process making it more perfect than it was when first turned out of the fermenting vessels. Common experience corroborates that in a very remarkable way. Everybody knows the difference there is between new and old wine, and the changes which takes place when the wine is being kept constitute certainly one of the most important parts of the general subject of wine-making. Wine, when its fermentation has been completed, is found to absorb air with considerable rapidity and avidity, and when endeavours are made to get out from this wine again the air which has been dissolved in it, it is found that some kinds of wine allow it to go, or part with it again, whilst other wines do not; and in this respect, a distinctive test is found between the qualities of the wine; for by observing this difference in the facility with which they give up the air which they have dissolved, and by comparing that with the qualities of wines in each case, a regeneralisation has been arrived at. In this matter I speak upon the authority of others, for I have not confirmed it by my own observations. But all that I do know fully corroborates it. The rule is this, that whereas low-class wines, which people will not pay much for, give up again almost completely the air which they have dissolved, superior kinds of wine do not give it up again, they only give up the nitrogen, and hold the oxygen fast. The oxygen, which is dissolved in both cases, is held firmly, or is digested by the high-class wines; but it is not digested, but simply eaten by low-class wines. Observations have been made in this direction by a great many observers, especially Berthelot and Pasteur, to whom we owe most decisive results in this respect.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

February 2nd, 1871.

Professor Williamson, F.R.S., President, in the Chair.

The following gentlemen were elected Fellows:—R. J. Friswell, R. F. Humiston, M.D., A. H. Mason, J. R. Tustin.

Professor Frankland, F.R.S., read a paper "*On the Development of Fungi in Potable Water.*" He began by alluding to the experiments Dr. Heisch had made some months back with waters contaminated with sewage matter. When to such waters some sugar was added, very soon a kind of fermentation ensued, and a rich fungoid growth made its appearance. Professor Frankland has now repeated and extended those experiments and arrived, with one or two exceptions, at the same results. But, in the course of his researches, he encountered some reactions which appeared at first difficult to explain. During a visit to Mr. Hope's irrigation farm at Romford, on the 26th of November last, he collected a sample of effluent water from one of the drain outfalls. This water consisted of the sewage of Romford, which had percolated to the tile drains through some four or five feet of loose gravelly soil. It was clear,—but that it still contained a considerable proportion of unoxidised sewage was evident from the following results which it yields on analysis:—

100·000 parts contained—

Total solid impurity in solution ..	85·600
Organic carbon	0·844
Organic nitrogen	0·233
Ammonia	0·040
Nitrogen as nitrates and nitrites ..	1·143
Total combined nitrogen	1·419
Chlorine	9·300
Temporary hardness	27·950
Permanent ,, ...	20·600
Total ,, ...	48·550

The proportion of organic carbon was three times, and that of organic nitrogen ten times, as great as that found

in unpolluted water, whilst the comparatively large proportion of ammonia showed that the oxidation of the organic matters was still incomplete. Nevertheless, this water, when mixed with the proper proportion of sugar, and maintained at a temperature of about 70° F., remained perfectly transparent for weeks. The result of the next experiment was still more remarkable. Two samples of the Grand Junction Company's water were mixed with sugar in the usual way. One of them was drawn from a foul uncovered cistern over and within a water-closet; the other from a clean slate cistern, in passing out of which the water filtered through some 30 or 40 lbs. of animal charcoal. The water from the foul cistern remained transparent for weeks, whilst that drawn from the clear cistern through animal charcoal soon became turbid and in three days had produced abundant fungoid growths. Remembering Mr. Heisch's statement that filtration through *well-aired* animal charcoal is effectual in preventing these growths even in foul water, Professor Frankland now passed a rapid current of air through the filter for about fifteen minutes, and then left it exposed to air for six hours. The result, however, was the same as before; Grand Junction water drawn through it immediately after aëration, behaved exactly as before when mixed with sugar. These experiments seem to indicate that the presence of a phosphate was in some way connected with the production of the fungoid growths and other living organisms, for it is known that water dissolves traces of calcic phosphate from animal charcoal, and this supposition was strengthened when Professor Frankland found that the effluent water from the sewage farm at Romford contained no detectable trace of phosphoric acid, the plants and poor soil of this newly cultivated farm having, doubtless, removed all phosphates from the percolating sewage. The hypothesis of the dependence of the fungoid and other growths upon the presence of phosphates was further supported by the results of the following experiments:—

A sample of the Grand Junction Company's water mixed with sugar remaining perfectly clear for twelve days, minute quantities of ammonic nitrate and sodic phosphate were now added; three days later, it swarmed with very active vibrios and cells with bright nuclei, subsequently very luxuriant branched fungoid threads developed themselves, and the mixture emitted a strong ferment odour, which, a few days later, became horribly offensive.

A sample of the Southwark Company's water mixed with sugar remained for nineteen days perfectly clear and transparent; small quantities of ammonic nitrate and sodic phosphate were then added. In a few days it was crowded with vibrios and monads, and later it was found to contain the characteristic mycelial fibres.

The usual proportion of sugar was added to a sample of water collected at the Cerney Springs near Cirencester; it remained clear for eight days, and was then mixed with traces of ammonic nitrate and sodic phosphate; it continued turbid ever afterwards, and soon became filled with swarms of vibrios and fine branching tubular fungoid threads; the water subsequently became brownish and emitted a very offensive odour.

These experiments show that potable waters, which stand the sugar test perfectly, become entirely changed in their behaviour with this test when they are mixed with traces of ammonic nitrate and sodic phosphate; and the following experiments prove that it is the phosphoric salt which alters their behaviour in this respect:—

A sample of the Lambeth Company's water, after admixture with sugar remained perfectly clear for nineteen days; ammonic nitrate was then added; after the lapse of several weeks the clearness of the sample had not been disturbed.

Canterbury deep well-water, softened by Clark's process, remained perfectly clear during twenty-three days after admixture with sugar; traces of nitrate of ammonia were then added, but, after the lapse of two months, it was still perfectly transparent and unchanged.

It is thus evident that the addition of minute traces of a phosphate, either as sodic phosphate, white of egg, or animal charcoal, at once determines these fungoid growths in saccharine water, which before exhibited no tendency to develop them.

An important question now presented itself. Are the germs of these organisms necessarily contained only in the waters which develop fungoid growths, or are they present in the atmosphere? The answer to this question is given by the following experiment:—

Small quantities of potassic chloride, ammonic nitrate, sodic phosphate, and sugar were dissolved in distilled water previously boiled for many hours with caustic soda and potassic permanganate, and afterwards again distilled. Just before solution the solid ingredients were strongly heated in a platinum spoon over the flame of a spirit-lamp; the potassic chloride and sodic phosphate to redness, the ammonia nitrate until a considerable proportion had decomposed into nitrous oxide and water, and the sugar until, after melting, it began to turn brown. This solution was placed in a stoppered bottle, as in all the previous experiments. After a few days' exposure to a temperature varying between 60 and 70° F., a magnificent mycelium of the characteristic description began to grow, and was soon followed by several others. The liquid was also crowded with very minute moving organisms, probably monads. A specimen of real sewer fungus was examined side by side with this and found to be very similar in appearance, but more transparent, and somewhat smaller.

It is thus evident that the purest water which can be obtained in contact with the air yields splendid crops of this sewage mycelium, if it be supplied with the necessary soil, and, further, that the sugar and salts just named contain all the elements necessary for its development. Of these elements, phosphorus is essential, for, in a solution made at the same time, exposed to the same conditions, and containing the same substances, *minus the sodic phosphate*, no trace of mycelium or of any other organism made its appearance during the nine weeks it was under observation.

The presence of germs in a sample of water is therefore insufficient in itself to produce Mr. Heisch's reaction when sugar is added, and then a short (probably a momentary) contact with air is sufficient to impregnate any sample of water with the necessary germs, which develop, on the addition of sugar, only in the presence of a phosphate. The reaction is, in fact, an exceedingly delicate test for phosphoric acid. It would probably defy the powers of the most expert chemist to detect, in 2 ounces of water, the phosphoric acid introduced by the addition of a single drop of a dilute solution of albumen; yet these atmospheric germs find it out, appropriate it, and, by their growth, reveal its presence. Professor Frankland then described some experiments in which water, sugar, sewage matter, and the necessary salts, were kept in dark or obscure places at the temperature of the body. He found that these conditions were favourable for the development of bacteria, vibrios, and similar organisms, but unfavourable for fungoid growth.

From all observations, which were only partially given here, Prof. Frankland drew the following conclusions:—

1. Potable water mixed with sewage, urine, albumen, and certain other matters, or brought into contact with animal charcoal, subsequently develop fungoid growths when small quantities of sugar are dissolved in them and they are exposed to a summer temperature.

2. The germs of these organisms are present in the atmosphere, and every water contains them after momentary contact with the air.

3. The development of these germs cannot take place without the presence of phosphoric acid, or a phosphate or phosphorus in some form of combination. Water, however much contaminated, if free from phosphorus, does not produce them. A German philosopher has said "*ohne Phosphor kein Gedanke.*" The above experiments warrant the alteration of this dictum to "*ohne Phosphor gar kein Leben.*"

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 24th, 1871.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

MR. BROTHERS, F.R.A.S., exhibited a drawing from the fine photograph of the solar corona, taken by him at Syracuse. during the late total eclipse of the sun.

Mr. W. B. JOHNSON, C.E., gave an account of two cases of very narrow escapes from serious accident to railway trains, in consequence of the present faulty system of arrangement of the points or switches.

Dr. JOULE, F.R.S., &c., read the following letter, dated January 21st, 1871, which he had received from Mr. William H. Johnson, of Bowdon.

"Since the last meeting of the Philosophical Society I have made some further experiments on the 'Effect of Cold on the Strength of Iron.'

"In these I have maintained a nearly fixed temperature, and thus avoided to a great extent the error occasioned by the rise in temperature, consequent on sudden torsion.

"January 11th. A piece of a charcoal wire rod, 0·237 of an inch diameter, gave the following results :—

	1st.	2nd.	3rd.
At about 40° F.	20 twists	19 twists	17 twists.
Adjacent 6″ at temperature of melting zinc.	10 twists	9 twists	7½ twists.

	4th.	5th.
Twisted very slowly, surrounded by salt and snow	19¼ twists	16 twists.
Adjacent 6″ at about 40° F.	15 twists	—

"The twisting under salt and snow was performed so slowly, each experiment lasting a quarter of an hour or more, that the temperature cannot have been affected by the torsion. The same care was taken at the temperature of 40° F.

"The great diminution of strength at the melting point of zinc is remarkable.

"I take the liberty of communicating these results to you, as unfortunately I shall be away at the next Meeting, and thus shall not have an opportunity of seeing you."

Mr. BROCKBANK remarked that these experiments did not affect the conclusions stated in his paper read at the last meeting. He believed that the strength of iron under torsion was most affected by the heat developed by the twisting, and that the cooling mixture employed by Mr. Johnson would have the effect of making the wire stand a greater number of twists by counteracting the excessive heat produced by the torsion.

Mr. BROCKBANK, F.G.S., exhibited a drawing of the machine used by him in his experiments on the strength of cast-iron at different temperatures.

NOTICES OF BOOKS.

The Annual Report, for the Year 1870. By CHARLES A. CAMERON, Ph.D., M.D., City Analyst. Dublin: Joseph Dollard. 1871.

ALTHOUGH only a pamphlet of ten octavo pages, this little volume bears ample testimony to the fact that to Ireland's capital strongly applies what is often said of foreign countries here, "they manage things better there;" indeed, the few pages before us contain a mere skeleton of the enormous amount and great diversity of labour performed by Dr. Cameron during the last year in his capacity as analyst to the City of Dublin. The catalogue contains the following headings of subjects:—Specimens of Food Analysed, including milk, bread, butter, flour, sugar, coffee, wine, porter, whisky; Poisonous and Adulterated Confectionery (113 samples in all); Water Analyses; Petroleum Analyses; Reports (14) on various Sanitary Subjects; Sewage Analyses. Acting as Officer of Health during Dr. Mapother's absence in America; inspection of chemical works, manure factories, &c.; attendence at law courts (106); attendance at Public Health Committee (52 times). Meat, fish, fruit, and vegetables condemned during the year. Without entering into further details, it appears that at Dublin they thoroughly understand, value, and carry very efficiently into effect, what is very properly termed *administration de la salubrité et sureté publique*, a branch of that portion of local government which in its entity is so fully treated of by Von Mohl in his celebrated work on "Polizeiwissenschaft." The excellent example given by Dublin deserves to be imitated elsewhere, for the want of a good and efficient system exists in many English towns and not the least so in this metropolis.

CORRESPONDENCE.

CHLORALUM AS A DISINFECTANT.

To the Editor of the Chemical News.

SIR,—The country is fairly steeped in contagion! Small-pox, scarlatina, typhoid, and typhus in man; the lung plague, foot and mouth diseases, and hog cholera in animals, are all raging, and we can scarcely open a newspaper or scientific journal without finding reference to self-propagating maladies, or means suggested for their control. Dr. Budd is meeting with well merited praise. Professor Lister has brought the "antiseptic treatment" into repute, and we hear of news of the antiseptic treatment of fevers. You, sir, can congratulate yourself on the prominence given to the health-preserving powers of carbolic acid, and Dr. Sanderson leads the van in scientific demonstrations of the nature of contagia. It has been recognised that the noxious particles are floating in air and water. Professor Tyndall's electric light has awakened the public mind to dangers surrounding every human being, and a complete revolution in medical and popular opinion has been brought about by Pasteur and his disciples.

Under the circumstances, I do not think the discussion you have permitted in the columns of the CHEMICAL NEWS is out of time or out of place. Our great aim, however, should be to divest the discussion of all that does not properly pertain to it.

Mr. Carter Bell has drawn attention to an article on "Disinfectants" in Ure's "Dictionary," in which the chloride of aluminium is mentioned as an antiseptic. In thanking him for this reference may I ask him to refer me, if possible, to any work in which the therapeutic properties of this agent are described. All I have sought has been knowledge on the subject, and Mr. Bell must not suppose I am specially anxious to lay claim to discoveries. Whether I am the first or the last in a good work matters not to me. My great aim in all that I have done during my professional life has been the extirpation of contagious diseases. I believe, it may be in error, that no part of the work I have accomplished has been of equal importance to the introduction of the chloride of aluminium in medicine. It is now an article of commerce, and one of a very small number, the only one, under many circumstances, available for the effectual destruction of fever poison. My opinion is sure to be confirmed by the "prolonged observations" desired by Dr. Ballard, and no attempts at disparagement nor injudicious advocacy on my part, should I have been guilty of such, can affect the issue.

Mr. Carter Bell has attempted "to remove the smell from alkaline sulphides by chloralum." He used the "yellow sulphide of ammonium" as a substitute for "a

badly smelling midden or cesspool," and would lead us to infer that sulphide of ammonium is the active principle, if I may use the expression, of the fetid contents of such receptacles. I have heard of another gentleman, a physician and a chemist, trying this extraordinary experiment.

Need I, in the columns of the Chemical News, repeat what I have stated elsewhere, that sulphide of ammonium is *not* the enemy we purpose attacking with disinfectants, and that it is not the main principle to counteract in dealing with putrefying organic substances? The chloride of aluminium acting like an acid decomposes the sulphide, and sets free sulphuretted hydrogen.

Disinfectants or disease-germ destroyers should he used promptly to kill the poisons emanating from fever patients. Chloralum does this, and completely arrests putridity so as to prevent the formation of badly smelling compounds, the existence of which rather denotes the decomposition of organic poisons.

It is true that a virus in a partial state of putrefaction may have even greater activity than before decomposition sets in, but complete putresence annihilates the poisonous principle. This I have proved with the virus of pleuro-pneumonia so often kept in a bottle by cowkeepers for purposes of inoculation. When partially putrid, from the absorption of putrid elements into the system of the inoculated animals, simultaneously with what virus is left unchanged, it is the cause of a high mortality. It may be kept so long as to be quite powerless.

Mudie's disinfectant we are told is sulphate of iron. This very useful salt, under its proper name, is available for many purposes in dealing with contagious or other diseases. We know its value; I cannot help thinking that, if Mr. Carter Bell wishes it to be more extensively used than it has been by the sanitarian, it would be well to follow the adage of calling "a spade a spade."

Chloralum is the chloride of aluminium, and it is now made of uniform strength, containing 1500 grains of the salt to the pint of solution; or in the form of powder, mingled with the usual bases, silica and sulphate of lime, forming a 30 per cent disinfecting powder, without odour, and of great efficacy. We know what we are dealing with here; and let us hope that those ill-natured monsters, prejudice and self interest, may not bar the way to an early solution of the problem as to which of the disinfectants may be safely and usefully used under the numerous circumstances calling for their employment.—I am, &c.,

John Gamgee.

1, Great Winchester Street Buildings, E.C.,
February 3, 1871.

THE SEWAGE QUESTION.

To the Editor of the Chemical News.

Sir,—I have not replied to the letter which appeared in the Chemical News from Professor Corfield, because there really is nothing in it for me to reply to. He certainly points out some "mistakes" I have fallen into, but he has also kindly saved me the trouble, by showing afterwards that the statements are in the main true.

In reply to my friend Mr. Hope I must disclaim any intention or desire to "draw" him, and, least of all, into an admission that any thing he had printed in a scientific paper *could* be absurd. As, however, he has made this admission I am bound to say that I never thought he could have been *quite* serious in the *reductio ad absurdum* alluded to. When, however, Mr. Hope says that he has never seen the paper in which my "assertions" about irrigation were made, I must remind him that the paragraph referred to is extracted from my letter to the *Journal of the Society of Arts* (vol. xviii., p. 338). He answered that letter and had also then "the pleasure in contradicting them." For I never said they were not *contradicted*; what I said was that they were not *disproved*. I say so still. Either the distinguished advocate of sewage irrigation must have answered that letter without seeing it, or his memory is not so ever green as his connection with this subject would lead us to expect.

I need not trouble your readers with the question in dispute, as by referring to the *Journal of the Society of Arts*, as quoted above, anyone interested can read Mr. Hope's paper, my letters, and his answer.

Under exceptionally favourable circumstances, irrigation will, no doubt, be a source of profit, and, I am willing to admit that it is yet in its infancy, and that many mistakes have been made in ignorance; but as the universal remedy it is out of the question. Mr. Hope's farm at Romford is, perhaps, the only one where the conditions are thoroughly understood, and he deserves the success which all will wish him; but picture a farm in the west of Scotland of 120,000 acres under the Glasgow sewage during the past three months frost! Nevertheless the British Association Committee look upon it as the only general solution of the sewage difficulty. They are determined to view the question only through a water medium, and a rather muddy one, to thoroughly investigate no other method, and to shun all contact with any professors of opposite views.—I am, &c.,

Edward C. C. Stanford, F.C.S.

Edinbarnet, Dumbartonshire.
February 1st, 1871.

MISCELLANEOUS.

Beet-Root Sugar in California.—The *News Letter* says:—"No one article in all the notices of the California Beet-Sugar Company begins to do the affair justice. We publish a few facts to correct the bungling misstatements made in the dailies during the past week. Two hundred and fifty barrels of A-1 sugar stands credited to shipment No. 1, with many more ready at mill to send over, and beets enough on hand to make two thousand barrels first-class sugar, besides second-class, syrups, &c. The mill, when running to its full capacity, will work 50 tons of beets per day: they are now running through about 40 tons every twenty-four hours. They have one hundred head of cattle under cover to fatten on the pulp. The managers met with no difficulties in reaching the result they have, the parties engaged in the affair knowing their business. They owe no one, and, so far as we can learn, there is no stock for sale, except at a premium."

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 19, 1870.

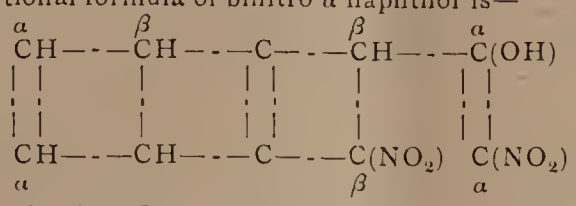
This number contains the following original papers and essays:—

Mononitro α Naphthol.—L. Darmstaedter and R. Nathan.—The introduction to this paper contains a brief review of the labours of Dr. Dusart on a substance obtained by him by the long-continued action of lime and caustic potassa upon nitro-naphthaline, named nitroxy-naphthalinic acid, described in the *Comptes Rendus*, vol. lii., p. 1183. The authors further state that the reason why they repeated the experiments of Dr. Dusart was that, in their opinion, the nitroxy-naphthalinic acid is nothing else than mononitro-naphthol, an opinion which is confirmed by the fact that the formula assigned by Dr. Dusart to the body just named—viz., $C_{20}H_7(NO_4)O,HO$, if written according to the present fashion—

$$\begin{matrix} C_{10}H_8NO_3 \\ OH \\ C_{10}H_6NO_2 \end{matrix}$$

differs only by one atom of hydrogen from the formula of mononitro-naphthol. In order to elucidate this point fully, the authors have tried and succeeded in converting the body alluded to into binitro-naphthol, by means of a process recently introduced by Drs. Wallach and Wichelhaus, thus proving that the nitroxy-naphthalinic acid is simply α mononitro-naphthol.

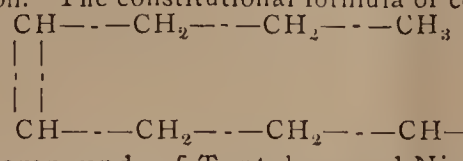
Constitution of Binitro α Naphthol.—Dr. H. Wichelhaus.—The constitutional formula of binitro α naphthol is—



First Synthesis of a Vegetable Alkaloid.—H. Schiff.—The author states that he has succeeded in the synthetical manufacture of coniine, the well-known alkaloid present in the *Conium maculatum*. The author obtained the alkaloid alluded to by the distillation of dibutyraldine, from which the alkaloid is generated according to the formula—

$$\underbrace{C_8H_{17}NO}_{\text{Dibutyraldine.}} = H_2O + \underbrace{C_8H_{15}N}_{\text{Coniine.}}.$$

The artificially-obtained coniine, like that produced by the plant, is a violent poison. The constitutional formula of coniine is—



Native Compounds of Tantalum and Niobium.—Dr. C. Rammelsberg.—This paper treats on fergusonite, a brownish black-coloured mineral found in Greenland, and first described by Dr. Haidinger (*Pogg. Ann.*, vol. v., p. 166). This mineral was formerly taken to be yttrotantalite, and specimens thereof have been analysed several times, since this was first done in Berzelius's laboratory by Dr. Hartwall in 1828. The specific gravity of this mineral has been found different by different authors, varying from 4·89 to 5·838. A somewhat similar mineral, which has been named tyrite, and also bragite, has been found in Norway. The percentage composition of the Greenland mineral, according to Dr. Weber's analysis, is—Nb_2O_5, 44·84; ZrO_2, 6·93; SnO_2, 0·35; YO, 38·61; CeO, 3·05; FeO, 1·33; UO, 0·35. Fergusonite from Ytterby (Norway), analysed by the author of this paper, gave—sp. gr., 5·56; composition, in 100 parts—Ta_2O_5, 8·73; Nb_2O_5, 40·16; SnO_2,WO_3, 0·91; YO, 30·45; CeO and (La,Di)O, 7·80; FeO, 4·09; UO, 1·98; CaO, 3·40; H_2O. Tyrite, in 100 parts—Nb_2O_5, 45·0; Al_2O_3, 1·05; YO, 30·0; CeO, 5·74; (La,Di)O, 3·51; FeO, 1·48; UO, 6·52; CaO, 2·36; H_2O, 4·88.

Law of Avogadro.—J. Thomsen.

Preparation of Chlorine from Hydrochloric Acid.—J. Thomsen.—This paper contains some remarks and observations on Mr. H. Deacon's paper on a new method of the manufacture of chlorine. M. Thomsen begins by stating that his acquaintance with the paper published by Mr. Deacon is only derived from the abstract published in the *Chemisches Centralblatt* (No. 46, p. 723, 1870), and the remarks made only bear upon some purely scientific matters—viz., the calorific value and quantum which is called into play in the reaction described by Mr. Deacon.

Contribution to our Knowledge of the Conjugate Sulphur Acids.—C. W. Blomstrand.—This lengthy essay is, on account of the large number of complex formulæ it contains, not suited for useful abstraction.

Chemical Researches of the Berries of Berberis vulgaris (Barberries) and on the Presence of Acetic Acid therein, according to Dr. Hermbstädt's View.—E. Lenssen.—The result of the author's analysis is that the berries alluded to contain, in 100 parts—Glucose, 3·57; free acid, 6·62; vegetable albumen, 0·51; soluble pectine substances, 1·37; ash, 0·96 (total of matter soluble in water, 13·035); seeds, 8·04; shells and cellulose, 2·56; pectose, 1·69; ash in the matter insoluble in water, 0·37 (total matter insoluble in water, 12·290); water, 74·675;—grand total, 100·0. No acetic acid was found, while the acid alluded to was recognised as malic acid.

Observations on the Products of the Splitting-Up of Albuminous Compounds.—W. Knopp.—This paper contains an account of a series of experiments made by the author with the view of ascertaining the nature of the compounds which are obtained by the action of mixtures of chloroform and sulphuric acid, as well as sulpho-vinic acid, upon dry albumen. The author's experiments on this subject are not as yet quite complete.

Under the title of Correspondence, this number contains the condensed reports of three meetings of the Chemical Society at Züirich (Switzerland). We notice therefrom the following subjects:—

Desoxalic Acid.—H. Brunner.—The author describes the preparation of this acid, the formula of which is $C_6H_8O_9$; that is to say—

$$\underbrace{C_4H_6O_6}_{\text{Uvic acid.}} + \underbrace{C_2H_2O_3}_{\text{Glyoxylic acid.}}.$$

The desoxalates of ammonium, barium, lead, and silver are described; while by far the largest portion of this essay is devoted to an exhaustive discussion, illustrated by lengthy formulæ, on the constitution and mode of origin of desoxalic acid.

Preliminary Notice on Sulphuretted Aniline.—V. Merz and W. Weith.—This paper treats on thionaline, $C_{12}H_{12}N_2S$, and several compounds thereof.

Lactic Acid Anhydride.—Dr. Wislicenus.—The author states that, when lactic acid is kept at the ordinary temperature of the air for a length of time in a vacuum, or even simply in a dry atmosphere, it is gradually converted into the so-called lactic anhydride.

Sitzungsberichte der Königlich Bayerischen Akademie der Wissenschaften zu München, vol. ii., No. 3, 1870.

This number contains the following original papers relating to chemistry and collateral sciences:—

New Locality where Monazite (Turnerite) Occurs.—Dr. G vom Rath.—The author states that, while inspecting the mineralogica collection of M. Handtmann, at Coblentz, the owner called his attention to a small crystal, partly imbedded in another mineral, which crystal, on testing it goniometrically, turned out to be the mineral known as monazite, or turnerite, and found near the locality, well known to tourists—the Laachersee (a deep lake, filled with very deep blue-coloured, nasty tasted water, which never freezes, situated in the neighbourhood of the Rhine, and being, in all likelihood, the crater of an extinct volcano filled with water). The contents of the author's paper on the above-named subject are entirely devoted to the crystallographical description of the rather rare mineral alluded to.

Experiments on the Germination of Seeds.—Dr. Vogel.—This memoir contains the detailed account of a series of experiments made by the author with the view of ascertaining the effect which various substances (as, for instance, aniline, amorphous phosphorus, dilute solution of permanganate of potassa, sulphate of copper, dilute acetic acid, dilute hydrocyanic acid, arsenious acid, illuminating gas, naphthaline, and other substances) have upon the germination of vegetable seeds. Among the substances which thoroughly impede, and even destroy, the power of germination, the author mentions dilute (0·21 per cent) acetic acid and phenylic acid (1 drop to 50 c.c.). It appears that otherwise the germination is not much affected by very dilute aqueous solutions of most of the substances above quoted.

Humate of Ammonia.—Dr. Vogel.—This paper is a treatise on the action of the salt alluded to, as regards its capability of dissolving silica and carrying it into the vegetable material of the plants during their growth.

Synthesis of Substituted Guanidines.—Dr. Erlenmeyer.—The contents of this paper do not agree with the title, since we read here a detailed crystallographical description of a platinum double salt of methyl-uramine.

Acids which are Formed during the Oxidation of the Butyl-Alcohol of Fermentation.—Dr. Erlenmeyer.

Valerianic Acids of Different Origin.—Dr. Erlenmeyer.—Since the author sent these papers to the editor of the *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, we have given an abstract of their contents already (see CHEMICAL NEWS, vol. 23, p. 22).

Physical Geography of the Elevated Portions of Asia.—H. von Schlagintweit-Sakülünski.—An interesting and extensive monograph on this important subject.

Journal für Gasbeleuchtung, December, 1870.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

Supply of Water to Towns.—E. Grahn.—This lengthy memoir is a portion of a lecture given by the author before a meeting of water-works engineers, held at Hamburg last May, and contains, apart from matters strictly relating to the engineering part of water supply, a very excellent account of the origin of water in nature of its various functions in the three kingdoms of nature, and on the requisite qualities of water intended for domestic use.

Description of a Regulator Intended to Keep Up a Constant Temperature when Gas is Used for Heating Purposes in Chemical Laboratories.—T. Schlœsing.—This paper is illustrated by several woodcuts absolutely required for the proper understanding of the contents.

Water Supply of the Town of Kassel.—M. Pfaffen.—The detailed description of the works, area, and mode of supply, as well as of the natural resources whence it is derived.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin (double number), September and October, 1870.

This number does not contain any papers relating to chemistry or allied sciences.

NOTES AND QUERIES.

Gas Analysis.—Is there any recent book published abroad upon gas analysis?

Composition of Borneite.—Can any correspondent oblige me with the composition of the mineral known as borneite.—STIBIUM.

Separation of Gold and Silver.—Mr. Henry Pizcy, the assayer, several years ago treated his chloride of silver with chlorine, to extract the small amount of gold, evaporated the filtrate down, and added protosulphate of iron. Is this process now carried out at all systematically? It seemed to me a very pretty operation.—PER MARE PER TERRAM.

Chemical Agriculture.—I would be much obliged if any of your numerous readers could inform me of a good work on chemical agriculture, and also of one on the manufacture and production of sugar, especially West Indian.—J. T.

Coating Cisterns.—(Reply to R. Binns.)—A mixture of slaked lime (dry) and a solution, moderately strong, of silicate of soda, forms an extremely hard and very watertight cement, but you will find it rather expensive, and you had better apply good hydraulic cement, or, perhaps, better yet, coat the cistern with coal-tar asphalte.

Sulphur Springs.—Can any of your numerous readers give any information respecting sulphur springs, the quantity of sulphuretted hydrogen present in any of these, the affections such waters are recommended for, and, generally speaking, the ailments for which sulphuretted hydrogen, either gaseous or dissolved, is considered to be beneficial?—T. H.

Protochloride of Antimony.—(Reply to "Amateur Chemist.")—There exist only a terchloride and pentachloride of antimony; the former may be prepared, as stated in Watts's "Dictionary of Chemistry," in various ways, among which are the following:—Passing a current of dry chlorine gas over antimony in excess; dissolving the tersulphide of antimony (black sulphide) in hydrochloric acid; or, also, by distilling a mixture of 3 parts of antimony in powder with 8 parts of corrosive sublimate. The compound here alluded to is a preparation kept in chemists' shops, and known vulgarly as butter of antimony.

Atomic Weights of Carbon and Oxygen.—The following are the percentages of carbon:—In acetic anhydride, 46·87; butyric anhydride, 60·67; benzoic anhydride, 74·05. If the molecules of these bodies are represented by the formulæ $C_4H_6O_3$, $C_8H_{14}O_3$, and $C_{14}H_{10}O_3$, respectively, and the atomic weight of hydrogen be assumed as 1, determine the atomic weights of carbon and oxygen by means of the method of least squares. Will some chemist kindly indicate the nature of the method, and its application in the present instance, or name some book in which the explanation may be found, and oblige—A Student.

Soldering Iron and Steel.—(Reply to "George G.")—There are several soldering fluids in use; among these we quote—"A solution of phosphoric acid in alcohol at 80 per cent; the parts to be soldered are moistened with this solution, either by dipping in or with a hair brush. Another solution is that of the double chloride of zinc and ammonium, to be made by dissolving equal parts, by weight, of zinc and sal-ammoniac in strong hydrochloric acid, and evaporation to dryness at a gentle heat; the saline residue is afterwards dissolved in water, so as to make a strong solution. It is obvious that, after soldering, the metal has to be carefully cleaned, so as to remove any excess of adhering soldering salt solution, because both the liquids here alluded to would, if left on the iron or steel, cause it to rust.

MEETINGS FOR THE WEEK.

Monday, 13th.—Medical, 8.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology. (Educational Course.)
—— Royal Geographical, 8.30.
Tuesday, 14th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
—— Photographic, 8. Anniversary.
Wednesday, 15th.—Society of Arts, 8.
—— Meteorological, 7.
—— London Institution, 7. Conversazione. Mr. W. H. Perkin, F.R.S., Sec.C.S., "On Alizarine and other Colouring Matters."
Thursday, 16th.—Royal, 8.30.
—— Royal Society Club, 6.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
Friday, 17th.—Royal Institution, 8. J. N. Douglass, Esq., "On the Wolf-Rock Lighthouse."
—— Geological, 1. Anniversary.
Saturday, 18th.—Royal Institution, 3. Prof. Jowett, "On Socrates."

TO CORRESPONDENTS.

*** Vol. XXII. of The Chemical News, containing a copious index, is now ready, price 11s. 8d., by post, 12s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our office, or, if accompanied by a cloth case, for 1s. Subscribers wishing to complete their sets of volumes are requested to apply to the publisher, who will give them information respecting scarce numbers and volumes. Vol. xxiii. commenced on January 6th, and will be complete in twenty-six numbers. Reading Cases, price 1s. 6d. each post free, may also be obtained at the Office.

T. Morgan.—1. The report of the Deputy-Master of the Mint can be obtained at Hansard's, Great Queen Street, Lincoln's-Inn Fields. 2. You will probably find the paper in the *Philosophical Transactions of the Royal Society.*

Vox.—Consult "Traité des Arts Céramiques," par A. Brongniart; or "Chefs-Dœuvres of the Industrial Arts," by Philippe Burty, published by Chapman and Hall.

Marshall Hall.—Received with thanks.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 586.

ON A MODE OF DISTINGUISHING THE DEPOSIT OBTAINED BY REINSCH'S PROCESS FROM SALTS OF MERCURY.

By JAMES St. CLAIR GRAY, M.B., C.M.,
Assistant to the Professor of Medical Jurisprudence, Glasgow University.

As the deposits obtained on copper-foil by Reinsch's process are possessed of no distinctive characters by which that produced by one metal can be distinguished from that produced by another, it is necessary to have recourse to additional modes of investigation in order to determine to which of the four metals so precipitable the deposit is due; thus, two of these, arsenic and mercury, yield sublimates—the one crystalline, the other consisting of metallic globules—when the coated copper slips, thoroughly dried, are heated to redness in a dry test-tube, and by this property are distinguished from antimony and bismuth; again, two, arsenic and antimony, form with hydrogen, as pointed out by Marsh, gaseous compounds possessed of certain peculiar properties by which they are distinguished alike from mercury and bismuth and from each other. But these, as well as the other differentiating processes, require a very considerable degree of care, neatness, and dexterity of manipulation, when the quantity of the metal present in a large quantity of organic matter is very minute, as it often is in medico-legal investigations; and it has frequently occurred to me that a greater degree of simplicity might yet be attained,

The following is for mercury a test of even greater delicacy than that afforded by sublimation from the coated copper foil, dried, of metallic globules, and is possessed also of the advantage that the result can be seen by the unaided vision while its application is extremely simple. It is founded on the great affinity which exists between mercury and gold.

The following is the best and most simple mode of procedure. One of the copper slips, coated in the ordinary manner by Reinsch's process, is first washed in pure distilled water and then thoroughly dried; when thus prepared it is rubbed with a flattened bead of pure gold, or, should this not be at hand, a flat signet ring will suffice. The result, if the coating be mercurial, is that a portion of the mercury, whose affinity for gold is greater than for copper, is transferred from the copper to the gold, appearing on its surface as a clear, white, shining, metallic crust, this being more conspicuous the more highly coloured the surrounding gold is. This stain is at once removed by pure stong nitric acid. This is of itself perfectly conclusive of the presence of mercury in the metallic coating or deposit on the copper, and is equally applicable should there exist in that deposit a combination of mercury with any or all of the other three metals which yield by Reinsch's process a metallic deposit on copper-foil.

15, Newton Terrace, Glasgow,
February 10, 1871.

OBSERVATIONS ON THE MANUFACTURE OF VERMILLION.

By M. ALSBERG, Ph.D.

In the process of manufacturing vermillion by Martin's method (agitation of quicksilver, sulphur, and alkaline sulphuret), two different stages can be easily distinguished; first, the combination of mercury and sulphur to the black sulphide; and secondly, the conversion of this amorphous black modification into the crystalline red. The chemical combination, as well as the crystallisation, is accompanied by the evolution of considerable heat, which may become so great, especially during the latter stage, and when working in closed vessels, as to give rise to dangerous explosions.

The crystallisation, a consequence of the solubility of the black sulphide in the alkaline sulphuret, and its gradual precipitation in the red form, only takes place when the mass is warm. If it be kept cold it requires a great length of time, often weeks, and even months, until the crystals are formed. If, on the contrary, the crystallisation has once set in (this is indicated by the black mass turning brown), it is generally finished within twenty-four hours. In the case of the slow crystallisation, there are sometimes crystals obtained, which, in colour and form, greatly resemble the native ore; the same crystals are occasionally formed when the solution of alkaline sulphuret containing sulphide of mercury is concentrated. Professor J. P. Cooke, jun., of Cambridge, who examined some crystals of that kind, says:—"They were rhombohedra, approaching a cube, and have the peculiar form of macling, which is so characteristic. They are, doubtless, the primitive rhombohedra observed on the native crystals, R on R 92° 36′. The faces are striated parallel to the basal diagonal of the rhomb face, and the crystals are terminated by the plane of a more obtuse rhombohedron of the same order."

The solubility of the sulphide of mercury in the alkaline sulphurets is considerable, especially when the fluids are concentrated, and then even in the presence of polysulphurets. This solution cannot be diluted without the sulphide of mercury being precipitated in the black modification, but the precipitation may be prevented by the addition of a small quantity of caustic alkali.

Evidently this is the reason that Weber says, that sulphide of mercury will dissolve in alkaline sulphurets only in the presence of free alkali, whereas solutions of the alkaline sulphurets, containing so much sulphur as to look deep red or brown, will readily dissolve enough of the sulphide to give, on dilutions, a considerable black precipitate. The precipitate does not always form immediately, but invariably makes its appearance after a short lapse of time.

The solubility* of the sulphide of mercury constitutes the chief source of loss in the manufacture of vermillion, and, on an average, amounts to from five to eight per cent of the mercury taken.

In spite of all precautions, it will sometimes happen that the mercury "flours," and, as it seems, every minute globule is coated with a thin layer of sulphide. This coat not only prevents the conversion of the metal into the sulphide, but even its solution in nitric acid. If concentrated nitric acid be used, the whole of the sulphide and metal is dissolved, whereas dilute nitric acid has no effect at all. Concentrated hydrochloric acid, especially when boiling, will readily decompose the sulphide.

As is well known, no vermillion will withstand the action of the light; it turns dark and gradually black again. Whether this change is due to the decomposition of the sulphide into the sub-sulphide and free sulphur, or simply to the conversion of the crystalline into the amorphous modification, is doubtful; but it is certain that any impurities increase this tendency, especially the presence of free mercury, which seems to indicate that a decomposition does take place, though always only on the surface. Even the passing of steam or the evaporation of a drop of water over some vermillion will often rapidly effect this change of colour. Of course, the value of the vermillion greatly depends on the stability of colour, and therein the different articles of commerce vary greatly.

* It is generally stated that sulphide of ammonium does not dissolve sulphide of mercury, but, judging from the fact that, according to Liebig and Gautier Bouchard, sulphide of ammonium effects the crystallisation of the black sulphide, it seems probable that this statement is not quite correct.

Some will retain their brightness for several years, while others may be seen to change after a few weeks. If, perhaps, the vermillion obtained by sublimation is a little more stable than that manufactured by the wet process, it certainly, with respect to brightness and fire, does not sustain comparison with the latter; therefore, the latter constantly gains more ground. The different shades, from a deep red almost to an almost light orange, are simply a consequence of the size of the crystals; the larger the crystals, the deeper the colour, and *vice versa*, so that large crystals resemble, in their cochineal colour, the native cinnabar.

Until a few years ago, all the vermillion used in the United States was imported mostly from Europe, and some from China, but now our own manufacturers have defeated foreign competition, when taking into account the high price of labour, &c. The protection by duty is very small, but not only have our manufacturers successfully competed with Europe, but they have even reduced the price considerably, thereby again increasing the use of their product. It may be safe to say, that the annual production in America amounts to 500,000 lbs., and may, before long, reach 1,000,000 lbs. The precarious condition of the California mercury mines may place our manufacturers in a dangerous position, compelling them to obtain their supply of mercury from Spain, at a greatly increased cost. Already, within the last six months, the price of mercury has risen fully 35 per cent, and, as the mercury trade is a monopoly, it may go higher still.—*American Chemist.*

ON GLUCOSE.

By Prof. CHARLES A. JOY.

In the year 1811, Kirchhoff, a celebrated German chemist, discovered that it was possible to convert starch by means of sulphuric acid into sugar. Great expectations were founded upon the announcement of the discovery, as, in consequence of continental wars and the English blockade, sugar had become a very dear article, and it was at first thought that an ample supply could be obtained in this way; but everybody was destined to be grievously disappointed as soon as the subject was more thoroughly investigated, and it was found that the sugar thus produced was of a different character from that to be obtained from the cane and beet. Still, the discovery of Kirchhoff was of great importance, and has led to many practical applications. It was soon found that glucose or grape sugar could be made in several ways, and that it was always the product of the germination of starch grains, and sometimes occurred already formed in nature.

It is probable that both cane and grape sugar are formed from the starch contained in the cellular tissues of the plant, cane sugar being formed first, and then grape sugar, if acids be present. Acidulous fruits contain only grape sugar, whereas cane sugar occurs in those that are free from stronger acids. The chief natural sources of the grape sugar are in the sap of the grape vine, in plums, cherries, figs, honey, in the liver and in diabetic urine; but it would not be economical to prepare it from any of these sources.

One of the latest methods for the preparation of grape sugar is the one proposed by Maubré, and is as follows:—the mixture of dilute sulphuric acid and starch meal is boiled under pressure of six atmospheres. The necessary boilers are similar to those used for high-pressure engines, and are lined with lead and provided in the interior with a perforated lead tube for the passage of steam. The boiler is further furnished with safety valve, stop-cocks, thermometers, &c. In the process of manufacture, 56 pounds of sulphuric acid of 66° B. are diluted with 5600 pounds water, and heated to 212° F. A mixture of the same amount of acid and water is made in a separate wooden vessel, the heat of which is raised to 86° F. Into the second mixture 2240 pounds of starch meal are well stirred and heated to 100° F. This is gradually added to the first mixture, and after heating with open valves for a few minutes to 212° F., the stop-cocks are all closed and the heat raised to 320° F., and continued until all of the starch is converted into sugar, which requires from two to four hours.

The contents of the boiler are then run into a wooden tank, and 168 pounds of pure chalk or carbonate of lime, previously stirred up with 500 pounds of water, are gradually added to neutralise the acid; the gypsum is caught on a filter and the filtrate evaporated to 20° B., and afterward clarified by blood and bone-black and again filtered. In this way the product is obtained pure and free from bitter and empyreumatic taste, and is well suited for any of the purposes to which grape sugar is adapted.

Another way is to convert the starch into sugar by means of malt. For this purpose 10 to 12 pounds of barley malt are well stirred with 400 pounds of water, and to this are added 100 pounds of starch, and the whole is heated to 158° F., and kept at that temperature for several hours under constant agitation. At 158° F. the starch becomes pasty, the grains burst, and at first there are no signs of sugar, but in a quarter of an hour the liquid becomes more fluid and begins to have a sweetish taste. Great care must be observed to retain the heat at the same temperature, not to have it either higher or lower than above indicated; and to insure this several thermometers ought to be put in different parts of the apparatus. After six hours the liquor can be filtered and clarified, and evaporated to a syrup. The sugar prepared in this way always retains the taste of malt, and is only adapted to use in breweries, where this property will not prove deleterious.

Grape sugar, or glucose, can be prepared in open vessels by allowing a mixture of starch and water to flow gradually at a temperature of 130° F. into a vat containing water acidulated with one per cent of sulphuric acid. By keeping it at a boiling point the starch is at once altered, without producing mucilage. The amount of starch taken is usually about one-half the weight of water employed. After all of the starch is added, boil for an hour and decant. The sulphuric acid is neutralised by carbonate of lime as before, and the liquid evaporated to the specific gravity of 1·28, and set aside to crystallise. The molasses is allowed to drain off, and the sugar is dried at a gentle heat in a current of dry air.

In the United States, especially in the West, it is more economical to make grape sugar from corn. There are several large establishments where this business is now extensively prosecuted. The corn is steeped in weak soda-lye to separate the husk and soften the gluten. It is then ground wet and run through revolving sieves, by which the husks and gluten are separated. The starch flows through long ways and troughs, in which are slats against which the solid particles lodge, and thus separate from the water. The wash water is run into a large cistern, where it can be fermented into weak vinegar. The starch is put wet into a mash tub and treated with one per cent sulphuric acid in sufficient water, for three to eight hours. Where it is intended to make sugar, the whole of the starch is converted; but if syrup is sought, then some part of dextrine is left unaltered. The acid liquor is neutralised with chalk as before, and evaporated in vacuum pans, and, after the separation of the gypsum, is run into barrels and allowed to crystallise. For syrup a certain percentage of the dextrine is left in the liquid unconverted, which helps to keep it from cystallising, and in the manufacture of syrup special care must be observed to neutralise all of the acid. The sugar is sometimes cast into blocks six inches square and dried on plaster plates, in a current of dry air, as hot air would be apt to discolour it. It has been found that glucose can be made

from cellulose as well as from starch, but the process is too expensive for practice; it is, however, interesting from a scientific point of view, and ought to be mentioned in this connection.

Two parts of clean linen shreds are gradually added to three parts of sulphuric acid, and they are allowed to stand twenty-four hours. The whole is then largely diluted, and the sulphuric acid neutralised by carbonate of lime or carbonate of baryta. In a similar manner any other kind of cellular tissue, as cotton, wood-shavings, paper, &c., can be converted into grape sugar.

It is a singular fact that, although we can prepare grape sugar from cane by the action of acids, no way is at present known by which glucose can be re-converted into sucrose. It would be a discovery of great importance if we could make cane sugar from glucose, as in that event common sugar could be prepared from a great variety of refuse matters, and would be largely reduced in price.

There was a time when much grape sugar was manufactured in England clandestinely, for the purpose of adulterating muscovado sugar, but this illegitimate business was destroyed as soon as the tariff on sugar was reduced. The price of cane sugar must be very high before manufacturers can afford to make grape sugar for its adulteration.

The starch of potatoes can be converted into glucose by digesting for a few hours with parings of the potato. This operation is largely practised by German farmers in the preparation of food for fattening hogs. The starch is rendered more digestible in this way, and from the glucose some of the larger proprietors manufacture alcohol for which they obtain a high price.

An excellent article of starch sugar can be prepared from Indian corn, which will yield alcohol one-eighth cheaper and quite as pure as that from cane sugar. As by a recent decision of the courts, the manufacturers of vinegar from this source are not distillers within the meaning of the tax levy, the business is not hampered by licences, inspections, or stamp duties, and has thus a great advantage.

In some parts of Europe large quantities of grape sugar are used to add to wine, but in America it is not so much the wine growers as the brewers who make such an extensive use of it as to give rise to its regular importation. This can hardly be justified excepting in times when the price of barley is very high.

We find in the *Zymotechnic News*, of St. Louis, an interesting article on the uses of starch sugar in the manufacture of beer, from which we quote the following paragraphs:—

"Barley contains on an average 57 per cent of starch and cognate substances. These pass into the wort, partly as sugar, partly in the shape of dextrine (gum). The relative proportions of these ingredients vary in accordance with the method of brewing, but experience teaches that, on an average, one bushel of barley yields about 12 pounds of sugar and 15 pounds of dextrine. A portion of the latter substance is further transformed into sugar during fermentation, so that a bushel of barley represents, on an average, 16 pounds of sugar and eleven pounds of dextrine (gum).

"Both dextrine (gum) and sugar are equally essential to the brewing process. The latter furnishes the alcohol, without which no beverage can be called spirituous; while the former constitutes almost the entire extractive matter, or body of the beer, which is one of the chief distinguishing features between beer and wine. Now it is true that all (commercial) starch sugar contains a certain amount of dextrine—the more the poorer the quality; but this portion would be insufficient in case a good article were used, while, in the contrary case, it would be paid for at an extravagant rate.

"Imported potato sugar of good quality, containing some 15 per cent of dextrine (gum), costs about 12 cents per pound at New York. Maize sugar of equal purity can be furnished at 8 cents per pound. Twenty pounds of either article, costing respectively 2·40 dols. and 1·60 dols., would yield 16 pounds of fermentable sugar and three pounds of dextrine (gum), while a bushel of barley will yield not only 16 pounds of sugar, but eleven pounds of dextrine or gum besides. Thus, starch sugar can be added to beer wort only in small quantities, unless when it is desired to impart a vinous character to the beer. When the latter object is not in view the best substitute for barley will always be found in maize or some other cheap grain.

"Not so in the manufacture of wine. For this purpose, good starch sugar, containing not exceeding 15 per cent of dextrine, is decidedly preferable to cane sugar. A pound of the latter, of the quality suitable for wine manufacture, costs at least 15 cents; whereas, as just stated, good starch sugar from maize can be sold at 8 cents. Now, as 5 lbs. of starch sugar are equivalent to 4 lbs. of cane sugar as regards their yield of alcohol, the balance is altogether in favour of maize sugar, to wit:—

	Cents.
4 lbs. cane sugar at 15 cents	60
5 lbs. grape sugar at 8 cents	40

"The 15 per cent of dextrine (gum) contained in the maize sugar will (according to the usual proportion of sugar added to must) increase the amount of 'extract' in wine only by a few per cent, and will tend to give it the 'mouthly' taste (body) which in meagre wines, already fermented, is sought to be produced by the addition of glycerine.

"Enormous quantities of cane sugar are already being consumed in the wine manufacture in this country; so that even as a consideration of national economy it is highly important to supply in maize sugar a partial substitute for imported cane sugar."

In France there is a use for grape sugar arising from the fact that the sugar manufacturers do not prepare molasses ready for the market as they do in this country. The crude molasses is bought up by second parties and the grape sugar is used very largely by them to extend it and give it body. An alkaline solution of grape sugar is converted by heat into a dark brown body, called melassic acid. This acid has a powerful affinity for oxygen, and reduces the CuO to Cu_2O. Some of the tests for grape sugar are founded upon this reaction. One of them, known as Fehling's test, is prepared as follows:—A standard copper solution is made from 1 oz. crystallised sulphate of copper, 3 ozs. cream of tartar, 1½ ozs. pure carbonate of potash, 14 or 16 ozs. of a solution of caustic soda (sp. gr. 1·12) and water until the solution measures 15,160 water grains; 200 measured grains of this solution contain a quantity of copper that would be reduced by 1 grain of sugar, each atom of sugar reducing 10 atoms of the black oxide of copper to the state of suboxide. Cane sugar is converted into grape by boiling with weak sulphuric acid, and it can then be easily tested by the standard solution. It sometimes becomes necessary to test for sugar in diabetic urine. This is accomplished in various ways. One of them, called Trommers's test, is as follows:—Add caustic potash, and filter if necessary; then dilute solution of sulphate of copper in small quantities; the precipitate that first forms dissolves on stirring, and the solution becomes azure-blue, but after standing, a fawn-coloured precipitate of suboxide of copper will be formed. The conditions and precautions to be observed are fully given in medical works and need not be repeated here. The property of grape sugar to reduce metallic salts is made use of for the preparation of silver mirrors. Add to the nitrate of silver a few drops of ammonia and then some grape sugar, and the metal will be precipitated.

Chloride of silver can also be reduced by grape sugar, and this method affords a way for reclaiming photographic wastes, and of preparing pure metallic silver. Take 14 parts of well washed and still moist chloride of

silver, 24 parts of caustic soda, sp. gr. 1·333, 11¼ parts ammonia, sp. gr. 0·925; to this add, with constant agitation in a flask, 7½ parts pure honey, or 9½ parts grape sugar syrup, and let the mixture stand in a warm place until sulphuretted hydrogen affords no sign of silver. Decant and wash out all traces of chlorine. The reduced silver can then be dried and melted in a crucible.

Platinum black, finely divided metallic platinum, can be obtained from the chloride by adding carbonate of soda in excess, and, after adding grape sugar, heating the solution for ten minutes. The precipitate can be collected in a filter and then well washed and dried.

Grape sugar crystallises in warty, cauliflower concretions composed of hard transparent cubes. It is less soluble in water than cane sugar, but more soluble in alcohol. Two and a half parts of glucose are required to produce the same sweetening effect as one part of cane sugar. Sulphuric acid does not decompose it, but forms a definite acid with it, called sulpho-saccharic acid. It forms a double salt with common salt.

$$NaCl,2(C_{12}H_{12}O_{12})+2aq.$$

It also forms definite but unstable combinations with the alkaline bases.

From the foregoing it will be apparent that grape sugar can be easily and cheaply prepared, and that it is capable of many important uses in the arts, if it could be manufactured in adequate quantity and at a reasonable rate.—*Journ. App. Chem.*

ON THE

ESTIMATION OF PHOSPHORUS IN CRUDE PIG-IRON, IN STEEL, AND IN MALLEABLE IRON.

By F. KESSLER.

THE methods at present in use for the separation of phosphorus from iron by the moist way are based either upon the formation in an alkaline fluid of an insoluble combination of one of the two bodies, leaving the other dissolved, or on precipitating the phosphorus in an acid solution; the molybdic acid method belongs to the latter class, and this method, as improved by various authors, is well suited for the requirements of the industrial estimation of phosphorus in iron, although it is troublesome and tedious. This last-named defect induced me to commence experiments with the view to find out whether it would not be possible to find a method of removing iron, and the other metals it is accompanied with, from an acid solution, and to estimate the phosphorus afterwards.

Starting from the well-known reactions of the two salts, commonly known as yellow and red prussiates, containing iron in the two conditions of ferrosum and ferricum, experiments were instituted in this direction, and, after some preliminary investigation, I found that I should be limited to the use of the following reaction:—

$$FeCl_2 + K_4FeCy_6 = 2KCl + K_2Fe_2Cy_6.$$

If it be supposed that 1 per cent of phosphorus in iron is indicated by the production of 1 decigrm. of pyrophosphate of magnesium, and, since that salt contains 27·93 per cent of phosphorus, it is evident that we have to take a quantity of 2·793 grms. of crude iron to experiment upon. As, however, a filtration from a bulky precipitate is required, it is preferable to double the quantity of metal and weigh off 5·6 grms. This quantity of metal, after solution in acid, requires no less than 42 grms. of crystallised ferrocyanide of potassium, and the ensuing precipitate will weigh at least 34 grms. exclusive of chemically combined water.

This fact sufficiently indicates the necessity of a correction in reference to the bulk of this precipitate, and since I could not find any published account on the specific bulk and gravity of this precipitate, I made a series of experiments, taking care to adjust the proportions according to the plan devised and indicated above. I dissolved 11 grms. of soft iron wire in hydrochloric acid, and 84 grms. of ferrocyanide of potassium in water, taking care to dilute each of these solutions to 500 c.c.; the specific gravity of the iron solution was equal to 1·055, while that of the ferrocyanide solution was found to be equal to 1·093. 250 c.c. of each solution having been taken were mixed in a flask holding 500 c.c., and the joint bulk found to be equal to 505 c.c. the liquid was filtered, care being taken *not* to wet the filter previously; the specific gravity of the filtrate was found equal to 1·033. From this the bulk of the liquid is calculated according to the following equation:—

$$250\ (1{\cdot}055 + 1{\cdot}093) - 34 = 503 \text{ grms.}$$

and the bulk of the precipitate—

$$505 - \frac{503}{1{\cdot}033} = 18 \text{ c.c.}$$

It hence follows that, in order to take 250 c.c. filtrate as the half of the total, the liquid mixed with the precipitate has to be previously diluted to 518 c.c. But this quantity may be varied from 517·5 to 518·5 according to the variable quantity of pure metal present in 5·6 grms. of crude iron as compared with the quantity of pure iron present in the 5·5 grms, of iron-wire solution just alluded to. Since I employed in my experiments the large crystals of the ferrocyanide of potassium as met with in commerce, it became necessary to ascertain the quantity of phosphates or other impurities present in the salt, and the amount thereof in the quantity constantly to be used, so as to eliminate this source of error altogether, or to take it into account; this was done in the following manner:—A measured quantity of chloride of iron, containing 5·58 grms. of iron and absolutely free from any phosphorus, was mixed with the ferrocyanide solution and further treated (the full description of my method I will give presently), for phosphorus by the pyrophosphate of magnesium method; instead of obtaining, however, that salt, I got a small quantity of a yellow-coloured flocculent substance, which, including the ash of the filter, left, after ignition, a quantity of 1 milligrm. and, consequently, instead of taking the quantity of the filter ash at 0·0005 grm. I took it during my experiments at 0·0010 grm.

Take 56 decigrms. of crude iron, previously suitably broken up—for cast-iron the substance should be passed through a sieve the meshes of which are 0·5 m.m. apart—put the metal into a porcelain crucible 11 centims. diameter at the top, and a depth of 13 centims., and provided with a handle and spout (a so-called casserole) and add to the metal 60 c.c. nitric acid, sp. gr. 1·2, taking care to cover the vessel with a large sized watch-glass. After the first violent action has subsided, the crucible is gently heated so as to cause the slow evaporation of the nitric acid; when, after the lapse of about one and a half hours, the materials contained in the crucible begin to get rather thick, the vessel is uncovered, and heated more strongly over an argand burner, care being taken to prevent spirting by the moving about of the crucible. As soon as the mass is dry, the handle of the crucible is fastened in a suitable manner so as to admit of the crucible and contents being heated to very bright redness (if a muffle is at hand it answers the purpose best); after the cooling of the vessel its contents are transferred to a platinum crucible. After the ignition of the materials in the platinum crucible to oxidise the carbon they are again transferred to the porcelain crucible and therein treated at a gentle heat with 35 c.c. of hydrochloric acid, sp. gr. equal 1·19, until the whole of the substance in the vessel, excepting some perfectly colourless silica, is dissolved. The method of dissolving the metal here described does away with the separate treatment of any phosphorus-containing residue in the case of nitrohydrochloric acid being employed for the solution of

the metal. The acid solution obtained, as just mentioned, is poured, without any previous filtration, into a flask of at least 518 c.c. capacity, and diluted to about 200 c.c. A caoutchouc plug is next placed into the neck of the flask, care being taken to have the plug perforated with two holes, through one of which a glass rod is passed, so as to admit of being used as a valve, while through the other hole, a glass tube is passed, which is let down through the neck of the flask just until it reaches the wider part of the flask; the tube is, by a suitable means, put into communication with a self-acting sulphuretted hydrogen apparatus. The sulphuretted hydrogen gas having been turned on, the operator opens the valve by removing the glass rod from the plug and shakes the flask, taking care not to spill any liquid; this operation is continued until the reduction of the iron and precipitation of copper, or any other metals precipitable by sulphuretted hydrogen, become apparent, as shown by the running together of the sulphur to small globules and the brownish colour imparted to the liquid. This method of reduction is more rapidly and readily effected than the reduction by means of a sulphide.

The next operation consists in pouring into the flask 200 c.c. of an aqueous solution of ferrocyanide of potassium, prepared so as to contain 210 grms. of the crystallised salt in 1 litre. The contents of the flask are next diluted to 518 c.c., and the liquid filtered through ordinary dry filtering-paper, folded as usual—the diameter of the filter being 27 centims., the funnel to be covered with a glass plate. The filter is at once filled to the rim, so as to moisten the paper thoroughly. The turbid liquid which at first passes through (about 20—30 c.c.) are separately collected, and after the turbidity subsides the filtrate is collected into the 250 c.c. flask.

The filtration is relatively the most tedious portion of the entire process, since it takes nearly an hour and a half. When a quantity of 250 c.c. of the liquid has been collected in the flask, its contents are poured into a beaker-glass, into which are previously poured 10 c.c. of a solution of sulphate of magnesium, of a strength of 200 grms. to the litre, and strong liquid ammonia in excess. For the proper separation of the ammonio-phosphate of magnesium, the liquid is left standing at least over night. The operator will observe that the salt just alluded to is always covered with a greyish coloured flocculent substance; but, without troubling about the nature of this impurity, which is completely removed by a subsequent operation, the manipulator brings the ammonio-phosphate of magnesium (as it does not adhere to the sides and bottom of the beaker-glass), and the body just alluded to, on a small filter, washes it somewhat with liquid ammonia of 0·98 sp. gr., and next dissolves the salt adhering to the sides of the beaker, and also that contained on the filter in nitric acid of 1·035 sp. gr. There will remain on the filter an insoluble and blue-coloured cyanogen compound. To the nitric acid solution, liquid ammonia in excess is added, and, after the complete separation of the pure ammonio-phosphate of magnesium, it is collected on a small filter, washed with liquid ammonia of 0·98 sp. gr., until, on testing, no more chlorine is detected, it is next dried and ignited.

After deduction of the weight of the ash of the filter and the impurity above alluded to (in the author's mode of operation, amounting to 0·0010 grm.), every decigramme of ignited pyrophosphate of magnesium amounts to 1 per cent, or, more correctly, to 0·9975 per cent, of phosphorus.

The author and inventor of this method states that a lengthy series of comparative experiments made by him have proved that the method just described is in every respect correct, and suitable for all practical purposes, and applicable to the proper estimation of phosphorus in crude pig-iron, steel, and malleable iron. He considers it to be preferable to the molybdic acid method.—*Journal für Praktische Chemie.*

THE PRODUCTION OF IODINE AND BROMINE.

By W. H. CHANDLER.

To Scheele is the world indebted for the first intimation of the elementary existence of fluorine and chlorine, he having in 1771 referred the action of sulphuric acid upon fluor-spar to the freeing of a distinct acid from the mineral, though whether fluorine has, even up to the present day, been isolated, is a matter of great doubt. In 1774 the same chemist isolated chlorine. In 1811 Courtois separated iodine from the waste liquor from the manufacture of soda-ash from seaweed, followed by the discovery of bromine in the bittern of sea-weed by Balard in 1826. The isolation of these four closely-allied elements from their compounds is thus included in a century, and the application of them to economical purposes, to any extent, was accomplished since the beginning of the present century. Their close relationship, their physical properties, and their chemical affinities, which are nearly in an inverse proportion to their chemical equivalents, induce one to the supposition that they are modifications of the same element.

The isolation of chlorine, bromine, and iodine from their compounds with the alkalies, is accomplished with equal facility. But the abundant store of the former in the enormous deposits of common salt throughout the world and in solution in the ocean and inland seas, forms a striking contrast to the rarity of the two latter halogens. In combination with silver, bromine and iodine are found in some rare ores in Mexico and South America. Chatin claims to have detected iodine in rain water, though in very minute quantities, and even in the atmosphere. In sea-water traces of it have been uniformly detected, though not in quantities sufficient for quantitative estimation. Bromine exists in slightly larger quantities, and, associated with iodine and chlorine, is found in the ocean and inland seas, the mineral and saline springs, and salt deposits, throughout the world.

According to Von Bibra, the amount of bromine in the Atlantic Ocean, in one United States gallon, is 24 grains; in the Dead Sea, examined by Herapath, 121·5 grains; in the dried residue of the Mediterranean, 1·15 per cent; in the mineral spring of Kreusnach, Ure found 10·8 grains; in Kissingen water, determined by Kastner, 0·44 grains; at Tenbury, in Worcestershire, examined by Dr. Ure, as high as 12·5 grains; and at Arnstadt, according to Hartung, 13·6 grains. Iodine occurs in far less quantities, from mere traces to 2·2 grains per gallon, this latter quantity being found in the iodine spring at Halle.

In the United States both bromine and iodine have been detected in the various saline and mineral springs. Iodine was first detected in America, in the Saratoga Spring waters, by Drs. Usher and Steel, in 1830, and bromine in the same waters by Dr. A. A. Hayes, and in the salines of Onondaga by Professor B. Silliman, in the same year. The quantity of bromine in the spring waters of Saratoga county, determined by Professor Chandler, reaches 3·63 grains per gallon in the water of the Artesian wells, the largest amount of iodine found being 0·2 grain; but in America, as in Europe, it is in the salines that the quantity of these substances becomes of economical importance, and in a brine of the Saginaw Valley, Dr. Chilton found 7·65 grains of bromine; at Tarentum, Pa., 6 grains bromine and 4 grains iodine were reported by Stieren; in the salina brine analysed by Professor Goessman, however, only 1·36 grains of bromine per gallon are reported.

Besides these various sources, iodine has been detected in the soda deposits of Peru, in the ashes of the Spanish barilla plants. Cod-liver oil is said to owe some of its medicinal properties to a trace of iodine. Though the distribution of bromine and iodine is thus very general, yet owing to their existence in such comparative minute quantities, the sources of our commercial supply are much more restricted.

Up to the beginning of this century the alkalies of commerce were derived from the ashes of plants, and the burning of sea-weeds was an important industry, especially in Great Britain and Ireland.

The amount of ashes of sea-weed, the so-called kelp, reached its maximum production in 1800, when 20,000 tons were collected. To produce this, 400,000 tons of wet weed were burned. From this time, owing to the removal of the import duty and to the introduction of the manufacture of soda-ash from common salt, the trade declined. But the discovery of iodine in the mother-liquors of kelp salts somewhat revived the manufacture—and it is to this source alone that the total supply of iodine in commerce is due. The high price stimulated the business, and in America, in a few places in New England, iodine factories were established. These latter, however, were soon abandoned, the weed upon the coast being of poor quality. The process of separating the iodine is exceedingly simple, being nearly analogous to that for the isolation of chlorine. The ashes are leached with water, and the various crystallisable salts of potash and soda are separated by concentration. Carbonates, sulphates, and chlorides of potash and soda are thus removed, leaving in solution sulphite, hyposulphite, and some carbonate of soda, together with the iodides and bromides. By the addition of sulphuric acid the first three salts are decomposed, and the sulphate produced is removed by crystallisation. The concentrated mother-liquor is acidulated with sulphuric acid, and after the addition of binoxide of manganese, the iodine and bromine distilled off. The reaction may be represented thus:—

$$NaI + MnO_2 + 2(HO,SO_3) = NaO,SO_3 + MnO,SO_3 + I.$$

The bromine of commerce was derived mostly from salines until the salt mines of Stassfurt were opened; the Schoenebeck salt springs, near Madgeburg, producing the greater part of the supply for Germany. The method of manufacture is similar to that followed in the separation of iodine.

Upon opening the mines of Stassfurt, bromine was found in the mother-liquors in considerable quantities, and at present the principal part of the European product is derived from this source. As high as 300 grains per gallon has been obtained from these mother-liquors. Although but two or three of the manufactories at this place have economised this substance, the price of bromine has greatly decreased during the last five years. This decrease hae been hastened by the large production of bromine in the United States.

Although the amount of bromines in the Saratoga waters is considerable, yet the comparatively limited flow of water, and the large consumption of those waters for medicinal purposes, precludes the manufacture. But from the strong salines our supply is derived in large quantities. At Tarentum, Sligo, and Natrona, in Western Pennsylvania, Pomeroy, Ohio, and Kanawha, West Virginia, the manufacture of bromine has become of considerable importance. The production of 1870 will reach 125,000 pounds, a quantity probably in excess of the consumption. In 1867 the Stassfurt product of bromine was nearly 20,000 pounds.

The total product of iodine in Great Britain and France is about 200,000 pounds annually, and outside these two countries very little is produced. As the average product of iodine is about ten pounds to the ton of kelp, and it requires twenty tons of wet weed to produce one ton of kelp, this total product represents the burning of 400,000 tons of sea-weed. At the present price, the iodine produced is of more value than the alkaline salts, which were the original object of the industry.

As previously stated, iodine is not produced in the United States. Since its use was first established here the price has fallen from 16 dollars to 5 dollars per pound. At present, bromide is furnished for less than 1½ dollars per pound.

The chief consumption of iodine and bromine is for medicinal purposes in the form of iodides and bromides of potash, soda, or ammonium. A small proportion is consumed in photography. Bromine has been proposed as a discharge in calico printing, and during the late American war was to some extent employed as a disinfectant. As yet but a small proportion of the bromine of the saline mother-liquors is economised, but should manufacturers turn their attention to this important substance, the consequent reduction in price will render its economical employment in other directions possible.—*American Chemist.*

NOTE ON THE ZIRCONS OF MUDGEE, NEW SOUTH WALES.

By Professor A. H. CHURCH, M.A.

I RECENTLY obtained a few rounded pebbles, each weighing about 2 grammes, for examination; they came from Mudgee, New South Wales. Although the specimens did not present the usual lustre of worn surfaces of zircon pebbles, yet their obviously high density, and the traces they retained of their pyramidal form, nearly sufficed to identify them with the zircon; this idea was amply confirmed by the results of experiment.

The Mudgee zircons present the exact tint known as hyacinthine; indeed, the true hyacinth or jacinth, about which such constant mistakes are made by jewellers, lapidaries, gem-collectors, and even mineralogists, is now an attainable luxury. The engraved gems and the cut stones commonly called jacinths (even by Dana—*vide* his "Mineralogy," 5th ed., p. 275) are invariably, so far as my experience goes, nothing but the hyacinthine garnet, a comparatively common stone possessed of far less interesting properties than the true hyacinth. The Mudgee zircons are rather dark; the colour is distributed somewhat irregularly in and upon many of the specimens. When cut, facetted, and polished, this Australian zircon, if not too deep in colour and too large in size, presents a rich soft red colour tinctured with orange-brown. I was fortunate enough to secure one pale-coloured specimen, which, now that it has been judiciously cut, has turned out a stone of surpassing brilliancy and beauty.

The density of one of the Mudgee stones was 4·704. After heating, it was found to have become quite colourless, although its density remained virtually unchanged, namely, 4·699. In these and most other particulars the Mudgee specimens resemble those of similar colour from Expailly, in Auvergne.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

February 2nd, 1871.

Professor WILLIAMSON, F.R.S., President, in the Chair.

Discussion on Professor Frankland's Paper "On the Development of Fungi in Potable Water."

THE PRESIDENT, having expressed the thanks of the Society for the interesting communication, asked whether the phosphoric acid present in the sugar had been taken into account?

Professor FRANKLAND thought that, through the crystallisation of the sugar, the phosphorus would have been excluded.

Dr. HEISCH was glad to hear that his statements had been confirmed by Dr. Frankland's experiments, but in two important points these were diametrically opposed to his own results. The first point was the statement that water which had been filtered through animal charcoal still retained organic germs. Now he (Dr. Heisch) has

been investigating, week after week for the last three years, large quantities of water which had passed through charcoal filters, and not on one single occasion had he found any fungoid growth in such water. The other point of divergence is, that whilst Dr. Frankland declares the cellular formations obtained from white of egg in a sugar solution to be very similar to the sewage fungi, he (Dr. Heisch) finds the two kinds of cells very easily to distinguish from one another. The sewage fungus is very small, perfectly spherical, excessively transparent, and mostly grouped in grape-like bunches. The development and decay of these cells is very often a rapid one; about six hours after the introduction of the sewage matter into the sugar solution the spherical cells appear, which, after six more hours, have grown into mycelia, and a short time after the whole vegetation has disappeared. But the special property which marks the whole growth of the sewage fungi is that it is accompanied by the emission of the odour of butyric acid. This circumstance is wanting in the development of organisms derived from white of egg; the cells, too, in the latter case, have a different look from the sewage fungi. Dr. Heisch then enumerated some pathological cases, which went to show that the noxious germs in potable waters are mostly contained in the same, and not derived from the atmosphere.

Dr. RUSSELL said, that soon after the reading of Mr. Heisch's paper, in June last, he had undertaken a series of experiments in the same direction, and he arrived at the same results. He also had investigated a sample of the Romford Farm effluent water by means of the sugar test, and met, like Dr. Frankland, with a negative result.

Mr. BELL remarked that the samples which Dr. Russell examined had, perhaps, been kept for some time, in which case they would have purified themselves by a process of natural decay. He procured seven samples from Dr. Frankland, four of which had been obtained in 1869; these when submitted to the sugar test did not become turbid, while three which had been taken in October and November last became turbid. When Dr. Heisch read his paper it occurred to him (Mr. Bell) that the investigation of the subject had not been exhausted. He had some doubt as to the cause of the turbidity, which is rarely produced in sugar solutions by fungoid development accompanied by the production of acid only. Shortly after Dr. Heisch read his paper, a gentleman brought a sample of water taken from a well in Drury Lane to the laboratory for examination. Mr. Lewin added some sugar to a portion of the water and, in about twelve hours, the sample became turbid, and when examined microscopically, was found to be alive with those little creatures which Mr. Bell had been in the habit of seeing in various vegetable extracts, and he at once inferred that these organisms caused the turbidity and not the fungoid development. He made various experiments with phosphates, and found that, when calcic phosphate is present, a large development of Bacteria is produced in solutions. He also made various experiments with water to which animal charcoal had been added, and with water passed through animal charcoal, and in every instance the water produced bacterian bodies on the addition of sugar. He further found that pure water into which ignited charcoal had been introduced might be kept a considerable length of time without developing any organisms on the addition of sugar, thus showing that the charcoal itself forms a suitable soil for organic germs.

Dr. VOELCKER confirmed the fact that sewage easily undergoes alteration. A jar of sewage left standing loosely covered for some months, lost almost all its ammonia, whilst its amount of nitric acid had increased. Referring to the insufficiency of charcoal for filtering purposes, Dr. Voelcker stated that iron sponge surpasses it very much in that respect. Water filtered through this medium could perfectly stand Mr. Heisch's test. Spongy iron is obtained by calcining with charcoal the residues from burnt copper pyrites.

Dr. THUDICHUM declared that a great deal more is talked about fungi than is really known, and that the whole fungus theory is a mere surmise and has no real standing ground.

Mr. WARINGTON explained that the purifying action of the spongy iron consisted probably in the removal of the phosphates in the water, since these would be retained by the hydrated ferric oxide wherewith the sponge is largely crusted. Mr. Warington further called attention to the fact that fresh animal charcoal gives up some phosphates to the percolating water, whereas charcoal that had for some time been in use does not, and he thought this may help to understand the difference in the respective results arrived at by Dr. Frankland and Dr. Heisch.

Dr. DUPRE asked whether Dr. Frankland had boiled the sugar solutions? Whenever he (Dr. Dupré) had done it, he obtained no fungoid vegetation..

Dr. FRANKLAND said in reply—

(To Dr. Dupré)—Usually they were not boiled, but in experiment No. 9 the sugar was burnt to caramel, the water previously treated with caustic soda and potassic permanganate, and all the salts added to it were heated to a considerably high temperature,—and in that experiment more splendid fungi were obtained than anywhere else.

(To Dr. Heisch)—The discrepancy in the observations as to the efficiency of charcoal seems to be explained by Mr. Warington's remark. As to the comparison of the two kind of cells, he did not consider them identical, but still rather similar to one another. Whether during the development of the sewage fungi the odour of butyric acid was perceptible or not, to this he had not paid attention.

(To Mr. Bell)—The samples of effluent water were taken into examination a day or two after their collection.

(To Dr. Voelcker)—The quick disappearance of the ammonia in sewage and sewage water had very frequently been noticed by himself.

(To Dr. Thudichum)—As to the fungus or germ theory of disease, he (Dr. Frankland) had nowise committed himself to it.

GLASGOW PHILOSOPHICAL SOCIETY:

(CHEMICAL SECTION).

A MEETING was held in the Society's rooms, Corporation Buildings, on Monday evening, the 30th ult., Alexander Whitelaw, Esq., in the chair.

Mr. JOHN SUTHERLAND read a note "*On the Pumping of Hot Liquids.*

Mr. E. C. C. STANFORD, F.C.S., referring to a paper read before the British Pharmaceutical Conference at Liverpool, exhibited a stuffed specimen of the fulmar (*Fulmarus glacialis*) from the Island of St. Kilda, and of the oil vomited by this bird.

In the parish of Harris, Inverness, nearly 200 miles fron Inverness, and about forty-five miles west of the nearest point in North Uist, 57° 50′ N. lat. and 8° 35′ W. long., is situated perhaps the most remarkable, certainly the loneliest, little village in Great Britain.

"St. Kilda's lonely isle" is seldom reached by strangers, and I therefore record briefly some impressions derived from a recent visit. There are several islands, one of which only, the south or main island, is inhabited.

Seen from North Uist on a very clear day, the main island has exactly the appearance of an enormous whale on the horizon, and the north islands look like a huge sea-fortress with a tower on each side.

The north island, or island of Boreray, with its surroundings is, perhaps, the boldest. We sighted this island in the early morning looming through a dense mist, and quite close to the vessel. As the mist suddenly cleared away, a startling scene presented itself. A perpendicular rock, some 1000 feet in height, sheer out of the Atlantic, towered above our heads. Its face, covered with lichens of every variety of colour, was gorgeously illuminated by the rising sun. It was flanked

by two enormous rocky pillars 800 to 900 feet in height, one of which is beyond the perpendicular. This rock is an extraordinary sight; it is perfectly white from sea-line to summit, being completely covered with the white eggs, droppings, and feathers of an innumerable family of Solan geese, which are said to keep this rock entirely to themselves. We astonished the winged inhabitants by a cannon-shot, but they repaid the astonishment with interest, for we were unprepared for the extraordinary effect of the myriads of wings which immediately hovered over and around us, and turned day into night.

St. Kilda proper lies about three miles south of Boreray. It may be described as a precipitous mountain ridge, which, in some parts falls sheer into the sea, with enormous precipices some 1500 feet high. It is three miles long and two miles in its extreme breadth. It lies 50¼ miles due west of Schillay Island in the Sound of Harris. The formation is marked in Nicholl's geological map as greenstone with syenite veins. On the south-east side the hill slopes down to a small open bay much exposed, and the landing is difficult. On this side of the hill the village is situated. About thirty houses, well built and better roofed than is usual in the West Highlands, are occupied by about seventy inhabitants. The population does not increase, the infant mortality is large, and said to be peculiar to the island.

It is somewhat remarkable that the inhabitants are not fishermen, but are all farmers and birdcatchers. Until quite recently none of the men knew how to fish. The staple food of the island is a bird called the fulmar, which forms the subject of this notice.

This bird is a species of petrel, the *Fulmarus glacialis*, or *Procellaria glacialis* of Linnæus, the Fulmar petrel of Buffon, belonging to the family of *Procellariæ*, order *Natatores*. At a distance the bird might be mistaken for a gull, which it resembles in size and colour; it is more nearly allied, however, to the albatross, which it resembles in its remarkable bill and its vomiting oil when attacked. The head, neck, and lower parts are pure white, the wings and back bluish-ash, and the bill bright yellow. The bill is stout and thick; the upper mandible considerably hooked at the tip, where it is also dilated and sulcated; the lower mandible is straight and slightly truncated. The nostrils are united in a single tube. A sharp claw on the legs takes the place of a hind toe. The flight of the bird is very beautiful, and it has a remarkably graceful movement of the head. The fulmar inhabits Polar regions, and, so far as I can ascertain, is unknown in any other of the outer Hebrides, and is found only on St. Kilda. It breeds enormously there in the rocks, laying a single large white egg, and the young are fed by the oily matter disgorged by the parents.

The strong bill enables them to extract oily matter, by perforating the skin of dead seals or whales.

In Newfoundland they feed largely on the codfish offal, and probably they are experienced fishers everywhere.

The method of catching these birds is peculiar to St. Kilda; the men may well call themselves birdcatchers, for assuredly there are none like them. The process seems simple enough, but the awful danger must be seen to be appreciated; indeed, the climbing propensities of these men would astonish any member of the Alpine Club.

Hanging on a rope (often made of horse-hair) the bird-catcher descends the fearful precipices, armed with a sort of fishing-rod, having a slip noose at the end. This he dextrously throws over the head of the bird, which is sitting on the ledge of the rock beneath him, and hauls him up. He then dips the bird's beak into a small leather bag suspended to his waist, and there the oil is vomited. The bird is then killed and eaten as food, the feathers and the oil forming the two articles of export. Beds made of the feathers are said never to harbour insects, but it is alleged also that they are difficult to keep dry.

The oil is a good deal mixed with a rougher sort from Solan geese, and realises a poor price as an ordinary rough fish-oil. The sample I exhibit is genuine. It is of a clear, dark, slightly reddish sherry-colour, and has a powerful aad peculiar odour—an odour of which the whole island and all the inhabitants smell. It is certainly a fish-oil, and it possesses nearly all the properties of cod-liver oil.

Its specific gravity is midway between cod-liver and sperm.

Fulmar oil, specific gravity	0·902
Cod-liver, light ,, ,, ...	0·924
,, brown ,, ,, ...	0·929
Sperm oil ,, ,, ...	0·875

It is soluble in ether. Cold alcohol dissolves less than 1 per cent, and hot alcohol 3 per cent.

Treated with a drop of oil of vitriol, it produces precisely the same coloured reactions as cod-liver oil, which, if the generally-received views be correct, would show it to be a liver-oil.

It contains a very faint trace of iodine.

Heated with oil of vitriol and excess of potash, it gives off a strong odour of oil of rue.

Saponified with soda, the soap retains the peculiar odour, and yields a tolerably fluid fatty acid on acidifying the solution.

I shall be glad if this short notice of fulmar oil will induce any one to experiment with it for medicinal purposes. I have no doubt a good deal might be obtained, and a good market would be a boon to that isolated people.

The specimen of fulmar exhibited is one of two that I had stuffed after keeping them alive for about a fortnight. It has suffered a little in appearance from its captivity.

Since the publication of this paper in the *Pharmaceutical Journal*, Mr. Norman McRaild, the factor of St. Kilda, writes me that the most of the birds are caught in August when young, and when they are very fat. Each bird throws up a small glassful of oil, perhaps about 1 oz., and 8000 to 9000 are killed every year, yielding about 50 gallons of oil. At one time the people lived entirely on the flesh of these birds. Now they make a good deal of a rougher oil from the salted bird. 675 gallons of this oil were sold in 1869 by the factor in Skye for smearing purposes, at 2s. 6d. a gallon. This is a lighter and thicker oil than the true vomit; but has the same chemical character.

Mr. P. L. Simmonds, the editor of the *Journal of Applied Science*, has sent me a specimen of Penguin oil from the Falkland Islands, in reference to which I quote an article in the *Journal of Applied Science*, for November, 1870, by B. Delfino.

The Slaughter of Penguins and Seals.—During the whole of August and beginning of September, large and countless flocks of penguins come from all directions to the Falkland Islands, and where they alight the ground is literally covered with them. This periodical migration is for purposes of reproduction. The people who make a business of killing these birds for their oil, proceed about this time in schooners capable of weathering the storms that are so common at this season. Besides a small crew, these schooners have on board a "copotar," with a gang of from twelve to fifteen men. Their only arm is a short stick. On the island to which they repair, they find a rough kind of furnace that has been used the previous year, and which seems to heat one or more iron boilers, each of which is capable of holding as much as 250 gallons of oil. These islands are leased from the Colonial Government for five years at a small rent, and every exporting house has several rookeries, which are respected by the rest. The penguin hunters are generally at their post before the arrival of their intended victims, and when these arrive and drop on the ground by millions, the men go among them and commit great havoc upon the tired birds, heaped together, whose wings are intended more as helps to swim than to fly. After the lapse of five or six hours of incessant slaughter, the "copotar" and his five

men generally have got enough of birds for one night's boiling. Each man immediately picks up a certain number of the dead birds and begins to skin them. This operation is done by making a cut in the belly, and, with a peculiar knack, the whole skin, with feathers and all, comes off the bird at one pull. I have seen one man, after five or six hours' work, present to the copotar about nine hundred penguins ready for the boiler. A certain quantity of fatty matter adheres to the skins, which are generally used to feed the fire. The furnace where the boiling-down operation is carried on is kept burning all the time the men reside in the islands, and serves beside to cook their food and protect them from the intense cold of these regions. The fat of eleven birds gives about one gallon of oil, and it is calculated that a gang of fourteen men, with a copotar, returns after a month and a half's campaign with a quantity of oil varying from twenty-five to thirty thousand gallons. With every flock of penguins, that is, with every three, four, or five thousand, there comes a bird larger and of a different plumage. The penguin's feathers are of a light ash colour, with brown spots, while those of this bird are white, with a yellow tuft on the head resembling a crown, and two rings made of yellow feathers, also on each side of the head, looking something like the ornament worn by ladies in their ears. The English call him the "king of the penguins," and he is seldom killed, perhaps owing to the fact of his being as remarkable for his leanness as the penguins are for their fat.

This oil, which appears to be a large article of commerce, is a dark brown oil, with the same general character as that from the fulmar.

It is remarkable that though these oils are obtained from birds, they have all the characters of fish oils, and all give the colour test with oil of vitriol.

I append comparative specific gravity and melting-points of the fatty acids from the soaps.

	Sp. gr.	Melting-point of fatty acids.
Fulmar oil, true vomit	0·902	70° F.
,, from flesh of bird ..	0·910	82° F.
Penguin oil	0·922	102° F.
Cod-liver oil, light	0·924	—
,, ,, brown	0·929	—
Sperm oil	0·875	—

The colour test has always been held to be due to the presence of cholic acid or other biliary matters; so that either these birds must feed partly on these substances or the test is illusory.

An interesting article appeared in the *Journal of Applied Science* (October, 1870) on the mutton bird of Australasia (*Procellaria obscura*), which appears to be closely allied to the fulmar in its character and uses.

The author also exhibited specimens of white and pink Tyree marble, the analyses of which have been already published in the Chemical News. As there was some doubt thrown on his former statement that the dark green crystals imbedded in the pink matrix consisted of hornblende, these had been since submitted to analysis, with the following result:—

Silica	60·00
Lime	11·64
Magnesia	3·32
Peroxide of iron	3·42
Alumina	22·18
	100·56

with a slight trace of manganese. This analysis corresponds with some already published of aluminous hornblende.

The author also gave a verbal communication of some experiments that were in progress, to show in a complete form the actual loss of nitrogen when fresh meat is mixed with various kinds of charcoal; in continuation of a former paper on "*The Retention of Organic Nitrogen by Charcoal*," already published in the Chemical News. The experiments had been some months in progress, and the results were promised, when completed, in a future paper.

NOTICES OF BOOKS.

Handbuch der Angewandten, Pharmacentisch-und Technisch Chemischen Analyse als Anleitung zur Prüfung Chemischer Arzneimittel und zur Visitation der Apotheken, wie als Wegweiser zur Untersuchung und Beurtheilung von der Pharmacie, den Künsten, den Gewerben und der Landwirthschaft angehörenden chemischen Präparaten und Fabrikaten. Unter Berüchsichtigung der älteren und neueren Pharmakopöen Dentschlands, Oesterreichs, der Schweiz, Englands, Frenckreichs, und Russlands, wie der Ergebnisse der nenesten Forschungen im Gebiete der Technischen Chemie, in vierter. Auflage neu bearbeitet von Adolf Duflos, Dr. der Medicin und der Philosophie, Königlichem Geheimen Regierungsrathe und Professor. Mit erläuternden Abbildungen nach R. Brodengeyers, Zeichnungen in Holzschnitt ausgeführt. Ein Ergänzungs-Band zu den ver schiedenen Ausgaben von des Verfassers Werk, Chemisches Apothekerbuch. Ferdinand Hirt, Königliche Universitäts und Verlags-Buchhandlung. Breslau, 1871.

Handbook of Applied Pharmaceutical and Technical Chemical Analysis; intended as well for a Guide in the Testing of those substances (Chemical Compounds) employed in Medicine as for a Guide to the Inspection of Chemists' Shops and the Testing of Materials employed in Arts, Manufactures, Agriculture, and collateral branches of industry. Written with reference to the older, as well as the more recent Pharmacopœæ of Germany, Austria, Switzerland, Great Britain, France, and Russia, and including also the most recent researches in the domain of Technical Chemistry. By Dr. Adolf Duflos. Breslau: F. Hirt. 1871.

Under the above title, quoted in full, we have received for review a copy of a work which deserves the attention of all who are interested in the study and practice of chemistry. The author is a Frenchman by birth and education, and for the last forty years he has been well and deservedly known for his scientific attainments all over Germany, and wherever the German language is read and understood.

The work before us is a handbook of chemical analysis, more especially applied to the pursuits of the pharmaceutist, but almost equally so to the requirements of the technologist, agricultural chemist, and manufacturing chemist; while its author intends it to be considered as a supplement to a larger work, entitled "Chemisches-Apothekerbuch"—a comprehensive work on chemistry, wherein such preparations as are employed in pharmacy are treated more fully than is the case in ordinary treatises on chemistry. As regards the work under review, this edition is the fourth, but the work is intrinsically a new one, having been re-written and enlarged, so as to embrace subjects beyond the scope of pharmacy; the result being a book unique of its kind, and got up with minute care and with the embodiment of all the latest researches published up to the time of going to press.

It is impossible to give even a faint idea of the amount of carefully digested and clearly explained matter condensed in some 400 pages, and embracing, as may be gathered from the title, all the chemical preparations used in pharmacy, embodying the older and more recent pharmacopœæ of Germany, Austria, Switzerland, England ("British Pharmacopæia"), France, and Russia. Not only are the commoner preparations described, as regards their characters and the proper modes of testing their purity, but also the rarer substances. In order to give our readers some idea of the manner of treatment, we take as instance—*Acetum (vinegar)*: Characters of that liquid; quantitative tests for the determination of its strength—viz., the quantity or real acetic acid it contains; tests for the detection and estimation of alcohol and aldehyde present in the vinegar; qualitative tests for the presence of sulphuric

acid; quantitative estimation of that acid; tests for hydrochloric acid; tests for nitric acid; tests for metals. Among the rarer substances, we quote as examples—*Amygdalinum* (the names are given in Latin and German), $C_{40}H_{27}NO_{22}=457$: Characters of this body; tests for its purity, and especially for the detection therein of salicine. Under the heading—*Anilinum*—

$$NC_{12}H_7, \text{ or } NC_{12}\left.\begin{matrix}H_2\\H_5\end{matrix}\right\}=93:$$

The author treats first of the pure substance, describing at length its properties and the tests for its purity; he next treats of the salts, and then of aniline oil (commercial aniline), the aniline colours, and the best means and methods of testing these for arsenic. *Liquor Hollandicus; æthylinum chloratum, chloride of ethylen*, $C_4H_4,Cl_2=99$: Description of characteristic properties and means of testing for its purity. *Carbonium bichloratum sulfurosum*, CCl_2,SO_2; or *trichlormethylium chloratum bisulfurosum* (bisulphited terchlorinated methylic chloride)—

$$C_2Cl_3,Cl+2SO_2=218:$$

Description of characteristic properties. *Natrum choleinicum* (a mixture of glycocholate and taurocholate of soda): Description of characteristic properties.

We could, without difficulty, add hundreds of examples, naming among them a large number of substances which are common articles of commerce—such as petroleum, phosgen, solar oil, petroleum ether, petroleum benzine, paraffin—not particularly belonging to the domain of pharmacy and manufacturing chemistry. In the appendix, we meet first with a concise description of the manures of commerce, and the modes of testing them qualitatively and quantitatively; and secondly, the author has added a brief, but excellent, chapter on reagents and on qualitative testing. The work is copiously illustrated with excellent wood engravings, contains an exhaustive index in Latin and German (each separate), is illustrated with a portrait of the author, and is well printed on good paper. The names of the paper maker, the printer, and the xylographic artist, are specially mentioned on the reverse side of the title-page.

OBITUARY.

JAMES SHERIDAN MUSPRATT.

We regret to have to record the death of Professor James Sheridan Muspratt, M.D., F.R.S.E., &c., which took place at West Derby, Liverpool, on the 3rd inst. The deceased was born at Dublin on the 8th of March, 1821, and was the son of the well-known founder of extensive chemical works established near Liverpool. The Professor was a pupil of the late Mr. Graham, first at Anderson's University, Glasgow, and afterwards in London, and also studied under Baron von Liebig at Giessen. The deceased was the founder of a College of Chemistry at Liverpool, and was well and widely known in the scientific world by a variety of scientific publications bearing upon chemistry in its widest sense; among the works edited by him, that styled "Chemistry, Theoretical, Practical, and Analytical, as Applied to Arts and Manufactures" is most widely known; but the English work, although containing undoubtedly a very great deal of valuable information, has been immensely improved upon by Dr. F. Stohmann, who has edited a German translation of this work, which is in every respect superior to the original, although divested of the large number of steel plate engravings representing the portraits of a series of scientific men. The deceased was a member of a large number of scientific societies, and many of his later researches have been published in the Chemical News.

CORRESPONDENCE.

DR. FRANKLAND'S PAPER ON THE DEVELOPMENT OF FUNGI IN POTABLE WATER.

To the Editor of the Chemical News.

Sir,—I have read with the greatest possible interest your account of Professor Frankland's paper on this subject, as his ingenious experiments really seem to have brought us a long way nearer to a comprehension of "the physical basis of life" than we were even so recently as when Professor Huxley delivered his opening address to the British Association at Liverpool. A firm believer in the existence of a "physical basis" of infection, I would suggest a third rendering of the dictum of the German philosopher which he quotes, namely, "*Ohne Phosphor gar kein Austeckung.*" This certainly seems to me exceedingly *probable* at all events, and perhaps "Heisch's test" may supply that test for infection, or even for the germs of non-infectious disease, which ordinary methods of chemical analysis do not afford; and Dr. Frankland may now have got an unfailing test for the dangerousness or harmlessness of any given "previous sewage contamination."

I ought to add that with regard to the general impurity of the sample of effluent water which Dr. Frankland took at my Romford farm in November last, I found, after the sample was taken, but before leaving the farm, as I think I mentioned to Mr. Smith, the Secretary of the Rivers' Commission, that, owing to some new drains having been only just filled in and some others being altered, some sewage was escaping into the outfall without passing as usual through 5 or 6 feet of *consolidated* soil. Yet I hope that further researches will show that even this imperfectly purified water was not dangerous.—I am, &c.,

W. Hope.

Parsloes, Feb. 12, 1871.

MISCELLANEOUS.

London International Exhibition, 1871.—The buildings in which this will be held are now finished, and ready to receive the exhibits. They will accommodate altogether 50,000 persons. The object of the Commissioners in holding the Exhibition may be summed up as follows:—They propose, in the first place, to make an International Exhibition a permanent institution of the country, giving to industrial art the same opportunity that is afforded to fine art by the annual exhibitions of the Royal Academy. In the second place, they produce the area over which the exhibition shall spread itself, by reducing the various industries into groups, and taking certain of these each year, bring the entire industry of the country under review every seven or eight years, fine art being a standing division of the programme. And, in the third place, to restrict the conditions under which exhibits have hitherto been received, by making all articles undergo a preliminary sifting, through appointed committees of selection, thus excluding all works that do not possess sufficient artistic merit to warrant their exhibition, and by the further exclusion of mere masses of natural products. The manufactures exhibited this year will be woollens and pottery, in addition to fine art of every description. H.R.H. the Prince of Wales is the President of Her Majesty's Commissioners for the Exhibition; Messrs. Spiers and Pond are to be the refreshment contractors; Messrs. Chaplin and Horne the carriers; and Her Majesty's Commissioners have entered into arrangements for the printing and publication of the official catalogues by Messrs. J. M. Johnson and Sons, of Castle Street, Holborn, London.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Journal für Praktische Chemie, Nos. 19 and 20 (double number), 1870.

This number contains the following original papers and essays:—

Some Derivatives from Oxysulpho-Benzide.—J. Annaheim.—(Preliminary notice.)—After briefly referring to some of his former researches, the author states that, by treating oxysulpho-benzide with iodide of methyl and iodide of amyl, he has obtained the following compounds:—

$$\underbrace{\left.\begin{matrix}C_6H_4OCH_3\\C_6H_4OCH_3\end{matrix}\right\}SO_2}_{\text{Methyl-oxysulpho-benzide.}} \quad \text{and} \quad \underbrace{\left.\begin{matrix}C_6H_4OC_5H_{11}\\C_6H_4OC_5H_{11}\end{matrix}\right\}SO_2}_{\text{Amyl-oxysulpho-benzide.}}.$$

Chemical Constitution of Diglycolic Acid and some Compounds allied therewith.—Dr. H. Kolbe.—This essay is not well suited for useful abstraction, since it would require the reproduction of a large number of lengthy and complex formulæ.

Composition of the Oil contained in Kernels of the Avoira Elais.—Dr. A. C. Oudemans, jun.—This excellent essay contains a detailed account of the author's researches on that kind of orange-coloured oil known in the trade as palm oil. The oil alluded to consists chiefly of the glycerides of laurinic, palmitinic, stearic, and oleic acids, mixed with small quantities of tricaprine, tricapryline, and tricaproine. In 100 parts the substance contains, in round numbers—Tri-oleine, 27·0; tri-stearine, 31·7; tri-laurine, 41·3.

Benzol-Disulpho Acid Chloride, and on Thioresorcine.—Dr. O. Pazschke.—(Preliminary notice.)—The chloride of the benzol-disulpho acid is readily obtained by the action of pentachloride of phosphorus upon dry benzol-disulphate of soda. The resulting body is solid; crystalline; soluble in ether; fuses at 62°; when acted upon by tin and hydrochloric acid, a violent reaction takes place, and a new substance is obtained which fuses at 27°, boils at 243°, and possesses a penetrating odour somewhat akin to benzol-sulpho-hydrate. On analysis, this body was found to consist of $C_6H_6S_2$; this compound should be viewed as resorcine, the O of which has been replaced by S. The author continues his researches, desirous to obtain compounds isomeric with thioresorcine.

Periodides of the Alkaloids.—Dr. S. M. Jörgensen.—This monograph, of which only the first portion is here published, cannot be abstracted so as to give even the least idea of the immense amount of labour and scientific ingenuity the author has brought to bear upon this subject.

New Chloride of Platinum.—S. A. Norton.—(Preliminary notice.)—When 1 molecule of chloride of platinum ($PtCl_4$) in aqueous solution is mixed with 2 molecules of nitrate of silver, the result is the formation of salt, $PtCl_2+AgCl$ (described in *Comptes Rendus*, vol. lxiii., p. 553). The yellowish coloured filtrate from this precipitate does not contain any silver nor nitric acid, but simply chlorine and platinum; the body obtained on evaporation crystallises, does not deliquesce, and its concentrated aqueous solution is not precipitated by ammonia. The author thinks that this compound may probably be—

$$PtO_2.4HCl + aq. \quad \text{or} \quad (PtCl_2)O.2HCl+aq.$$

American Journal of Pharmacy, February, 1871.

This number contains the following original papers relating to chemistry and collateral sciences:—

Decomposition of Acetate of Morphia in Solution.—J. M. Maisch.—After referring briefly to some well-known spontaneous decompositions observed to take place in the aqueous solutions, not simply of alkaloids (as, for instance, sulphates of quinine and morphia), but also in some neutral solutions of certain inorganic salts, among which are neutral acetate, and succinate of ammonia (first observed by M. Horst, see *Archiv. der Pharm.*, 1823), the author proceeds to say that Dr. W. T. Taylor, of Philadelphia, U.S., informed him that, while always keeping in stock, for external use, a solution of acetate of morphia, he (Dr. Taylor) had repeatedly observed the separation in the liquid of some few crystals which, on examination, proved to be pure morphia, entirely free from acetic or other acid, and yielding the usual characteristic reactions. At the same time the liquid had deposited a considerable quantity of a brown matter, while the fluid was of a pale brownish colour. The fluid was neutral to test-paper; became reddish coloured with chloride of iron, which tint disappeared on the addition of hydrochloric acid. The liquid, after having been acidulated with nitric acid, was rendered turbid by the addition of iodohydrargyrate of potassium; evidently some minute portion of acetate of morphia remained in solution. The author observes that the acetic acid contained in ordinary vinegar becomes decomposed, and that the changes noticed in the solution of acetate of morphia are, perhaps, of a similar nature; while the question is raised concerning the utility or disadvantage of the addition of acetic acid to some pharmaceutical preparations.

Precipitation of Quinine by Iodide of Potassium from Acid Solutions.—J. M. Maisch.—A prescription was ordered for preparation to consist mainly of 15 grains of sulphate of quinine, 1 drachm of iodide of potassium, and the same quantity of tincture of chloride of iron (of course according to the strength prescribed in the United States Pharmacopœia). The alkaloid salt was dissolved in the tincture of iron, and the iodide of potassium in water. The solutions having been mixed, a brown precipitate was at once formed, whereupon a fresh portion of sulphate of quinine was dissolved in water with the aid of a little sulphuric acid; the iodide of potassium was next added, and after this was dissolved the tincture of iron, when the same result was observed. The purity of the iodide of potassium having been duly ascertained, a series of experiments were instituted with reference to a statement made by Dr. Righini (*Journ. de Chim. Méd.*, vol. xiii., No. 116), to the effect that bisulphate of quinine produces, with iodide of potassium, a red pulverulent precipitate. The examination of the precipitate obtained in compounding the above prescription, after washing with water, exhibits, when dry, a brown-coloured powder, emitting a slight odour of iodine, which is slowly evolved. When the precipitate is treated with ammonia, it changes to a dull cinnabar-red; dissolved in acids, it yields a copious precipitate with iodo-hydrargyrate of potassium; heated upon platinum-foil, it is decomposed, leaving, first a bulky charcoal, which, on being burned off with some difficulty, leaves no residue; so that the precipitate only contains iodine in addition to the elements of quinine.

Emulsion of Almonds.—H. P. Reynolds.—The following prescription may be of extensive utility, and for that reason we depart for this time from our general rule of not entering upon the direct territory of pharmacy. Take sweet almonds (blanched), sugar, and glycerine ("C. P."), of each 1 ounce; powdered gum arabic, 1 drachm; water, 2 ounces. Rub to a uniform paste, strain through muslin, and evaporate, by a heat not exceeding 150° F., to the consistence of a fresh solid extract; preserve in wide-mouthed bottles, of size convenient for use; may be flavoured to suit taste. The author prefers orange-flower water and oil of almonds. When an emulsion of almonds is prescribed, as is now often the case, as a vehicle for chloral hydrate, it is readily prepared as follows:—Take concentrated emulsion, 2 drams; water, sufficient to make 1 ounce of mixture; mix thoroughly. The above emulsion is preserved, or rather condensed, milk of almonds, and may be useful for dietetic and culinary purposes—as, for instance, to prepare readily orgeade. The syrop d'orgeade does not, as is well known, keep for any length of time without fermenting, and thereby spoiling.

Morphiometric Process for the Pharmacopœia.—W. Procter, jun.—An excellent essay on the assaying of opium to determine its morphia strength.

On Glycyrrhizin.—J. M. Hirsch.—A memoir answering the question as to the easiest method of isolating glycyrrhizin, and on its property and mode of action of masking bitterness.

Artificial Preparation of Mannite.—J. M. Hirsch.—This paper contains the account of some experiments on the preparation of mannite by artificial means, more especially for the production of that material to replace the drug known as manna, which has become very scarce. These experiments, not being quite successful, are to be followed by others.

Sitzungsberichte der Königlich Bayerischen Akademie der Wissenschaften zu München, vol. ii., No. 4, 1870.

This number does not contain any papers or memoirs relating to chemical or allied sciences, but contains a lithographic reproduction, accompanied by a learned essay, on the oldest map known to be in existence—viz., a map of the Nubian gold mines, the facsimile being reproduced from the specimen belonging to the collection of Egyptian antiquities in the Museum at Turin.

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt, No. 3, 1870.

This number contains three lengthy essays relating particularly to the geognosy and paleontology of the Austro-Hungarian empire, while a fourth essay is devoted to the geology of the Eastern portion of European Turkey. As usual, these essays are illustrated by magnificently-executed lithographs, and, as regards the last-named essay, by a large-sized coloured geological map.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin November, 1870.

This number does not contain any papers relating to chemistry or collateral sciences.

NOTES AND QUERIES.

Gas Analysis.—Bunsen's work on this subject is equal to every requirement.

Colouring of Confectionery.—Would you kindly inform the undersigned which is the best innocuous yellow colouring matter for confectionery purposes? Also as to the toxicological properties of carmine of cochineal, indigo, and ultramarine?—INNOCUUS.

Mineral Oil.—Can you or any of your readers say the reason why pale mineral lubricating oil (stored in iron tanks), should go darker coloured the longer it is kept ?—B. S.

Artificial Mineral Waters.—Will you kindly recommend me a book containing the best and latest information on the manufacture of artificial mineral waters, either in English or French.—S. C.

Benzole.—The trade reports quote for quality "30 per cent,"—this must refer to English make. Is it 30 per cent distillate to 100° C.? if so, what relative value has Scotch benzole distilling 45—50 per cent to 100°, and 90 per cent to 120° C.? It appears English benzole is richest in colouring matter.—Carbon.

Sulphur Springs.—T. H. will find full details in Pereira's "Materia Medica," vol i., pp. 298, 299; Longmans, 1849. I will copy the particulars for him if he will send his address.—G. A. Keyworth, Hastings.

Sulphur Springs.—(Reply to T. H.)—You will find in vol. 5 of the "Handwörterbuch der Reinen und Angewandten Chemie," a series of about 200 analyses of various mineral waters, among which are several containing sulphur in some form or other. As regards the therapeutic application of sulphur-containing mineral waters, and mineral waters in general, including the composition of the chief active constituents, we refer you to Pereira's "Elements of Materia Medica and Therapeutics," vol. i. of the 4th, or perhaps later edition, under the heading "Inorganic Bodies—Water," where you will find all the information you desire. Both books here quoted may be inspected at the Library of the Commissioners of Patents.

Composition of Borneite.—(Reply to "Stibium.")—On page 44 of the 5th Edition of Dana's celebrated work on "Mineralogy," the composition of no less than twenty-five samples of bornite (so it is there written), also known as variegated copper ore, purple copper ore, poikilo pyrites, is quoted. It appears that the mineral varies in composition, but essentially contains sulphur, copper, and iron, mixed with more or less gangue. A crystalline sample from Redruth, Cornwall, contained in 100 parts:—Sulphur, 26·84; copper, 57·89; iron, 14·94; gangue, 0·04; total, 99·71.

Precipitating Sulphur.—Have you or any of your readers a good and cheap method of extracting or precipitating sulphur which is held in solution in caustic soda liquor, carbonate soda liquor, and soda-ash *vat* liquors? I am aware that, if any of the liquors are boiled, or otherwise concentrated, to a salt or solid, and are then subjected to heat in a reverberatory furnace, that the said heat, if hot enough, will *burn* out all the sulphur. I am also aware that sulphate of lead will precipitate a quantity of the sulphur; but, both processes being costly for the purpose I require, I write asking if you have or know any cheaper or better method.—Chemicus.

MEETINGS FOR THE WEEK.

Monday, 20th.—Medical, 8.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology." (Educational Course.)
Tuesday, 21st.—Zoological, 9.
—— Civil Engineers, 8.
—— Ethnological, 8.
Wednesday, 22nd.—Society of Arts, 8.
—— Geological, 8.
Thursday, 23rd.—Royal, 8.30.
—— London Institution, 7.30. Mr. F. S. Barff, M.A., F.C.S., "On the Action, Nature, and Detection of Poisons."
Friday, 24th.—Royal Institution, 8. W. Mattieu Williams, "On Rumford's Scientific Discoveries."
—— Quekett Microscopical Club, 8.

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THE CHEMICAL and TOXICOLOGICAL CLASS will meet as usual every Monday and Thursday evening, at 8 p.m., commencing October 3rd.

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THE CHEMICAL NEWS.

VOL. XXIII. No. 587.

AMERICAN ECLIPSE EXPEDITION.

WE owe to the courtesy of the editors of the *Journal of the Franklin Institute* early proofs of a long communication from Professor Young, dated Jerez, December 23rd, 1870. From it we make the following extracts:—

Our party of twelve concentrated at Jerez, some thirty miles (by rail) north of Cadiz, about ten days before the eclipse, and although much interrupted by unfavourable weather, our instruments were in position and adjustment in good season. Mr. Dean, assisted by Capt. Ernst and Mr. Gannett, had in charge the determination of our geographical position, using a 46-inch transit, chronometers fitted with galvanic break circuit apparatus, a chronograph, sextant, &c. The photographic apparatus comprised two telescopes equatorially mounted with clockwork, one of 8 inches aperture, and the other of 6, besides a horizontal telescope of 5 inches aperture, and about 30 feet focus, with a plane unsilvered mirror of glass to reflect the sun's rays into the tube. Messrs. Willard, Mahoney, and Gannett attended to this department.

Professor Pickering observed for the polarisation of the corona with apparatus which, I presume, he has described in his letter to you. He was assisted by Mr. Ross. Professor Langley also observed for the same, and for the general features and structure of the corona.

We had four spectroscopes in the field. Professor Winlock used an instrument with two prisms. This was attached to an equatorial of 5½ inches aperture, which formed a distinct image of the object observed on the slit of the collimator. Mr. Clark assisted him at the finder.

My own instrument was the spectroscope figured and described in a late number of the CHEMICAL NEWS (See vol. xxii., p. 277). It was attached to the Dartmouth College Equatorial of 6½ inches aperture, and, like Professor Winlock's, received a distinct image on the slit of the collimator. The observations previous to totality were made with the whole dispersive power, equivalent to 13 prisms. During totality I used 7. As the observations of Professor Pickering last year had made it a matter of interest to observe the general spectrum produced by the total light coming from the moon's immediate neighbourhood during totality, without any limitations such as are introduced by throwing an image upon the slit, two instruments were mounted for this purpose. The first consisted of the collimator and telescope of my large 9-prism instrument, with two of its prisms. The telescopes have an aperture of 2¼ inches, and a focal length of about 17, and the prisms are of proportionate size, having, however, an angle of only 45°.

This was mounted upon a board in such a manner that it could have the slit of its collimator readily pointed toward the sun. As its angle of aperture was about 70°, it required no great exactness of direction and no movement during the totality. This instrument was put in charge of Mr. Abbay, a member of one of the English parties, who, at his own request, was kindly detailed to assist us in the matter, as they had no suitable instrument, and we no spare observer. He brought with him an induction coil and a Geissler tube, prepared under Mr. Lockyer's supervision, which gave the combined spectra of hydrogen, mercury, magnesium, and sodium. This spectrum, thrown into a field of view in the ordinary way, formed the scale of reference. Another smaller instrument was fitted up from a star spectroscope of one prism of very dense glass, with telescope and collimator of the usual size—about 1 inch aperture and 7 focal length. To increase the light I placed in front of its slit a small telescope, magnifying about 2¼ times, and having a field of 3½°. This was adjusted carefully for distinct vision on a remote object, and its effect was, *not to form an image on the slit*, and thus to limit the locality from which the observed spectrum was derived to some small point, but merely *to increase the virtual angular diameter of the sun and the total amount of light received on the slit.* This instrument was put in charge of Mr. Pye, a young Englishman who was spending the winter at Jerez, for the benefit of his health. He had had some experience in spectroscopy, and used his instrument very skilfully and effectively. To all these instruments, excepting Mr. Abbay's, was adapted Professor Winlock's beautiful arrangement for recording the positions of the lines observed. A little chisel is brought down by the simple pressure of the finger, upon a siver plate which moves with the telescope, and thus a fine line is made which permanently marks the position of every line which may be upon the cross-wires of the instrument; these plates are preserved, and afterwards read off at leisure by a suitable micrometer.

The day and night previous to the eclipse were very fine, but early in the morning it clouded up. When we arose the prospect was very gloomy—it even rained from time to time.

Just before totality the clouds became much thicker and we nearly gave up hope, but at the needed time, almost by the direct interposition of Providence, it would seem, a small rift in the now heavy clouds passed over the sun, and permitted us to see and observe the sublime phenomenon, if not in all the beauty and grandeur of last year, yet satisfactorily and most gratefully.

During the totality, one good photograph of the corona was obtained with the 6-inch glass, with an exposure of one and a half minutes. It is, of course, by no means so good as it would have been had the sky been truly clear, but it shows a great deal of detail, curved filaments and radial shadings far better than any before obtained. The picture obtained with the 8-inch glass was nearly injured by not being removed until the sun came out.

The details of its structure were not seen this year nearly as well as last by the telescopic observers. The thin cloud shaded out and obliterated definition, without, however, cutting out much light. I think, if anything, it appeared more extensive than then, but much less definite in its outline. Others, however, thought it much less extensive. Its form was, as usual, roughly quadrangular—nearly square; but it is a curious fact that its longest diagonal was neither equatorial nor polar, but made an angle of nearly 45° with the hour circle, lying from the N.W. to the S.E. This was carefully determined by Professor Langley.

In respect to the polarisation observations, the appearances in the instruments were much complicated by the cloud and haze, but I believe Professors Pickering and Langley both agree that the corona certainly has a considerable proportion of its light radially polarised.

Our spectroscopic results completely confirm those of last year, except that the two faint lines which I saw between D and E last year, and suspected to be corona lines, as well as 1474, were not seen at all this time. 1474 was traced by Professor Winlock to a distance of near 20′ from the sun's limb. I traced it 16′ on the west, 12′ on the north, 14′ on the east, and about 10′ on the south. The principal chromosphere lines were also visible in the corona to a distance of 3′ or 4′. But Professor Winlock and myself both agree in attributing these to the reflection of the haze around the sun. I am the more confident as to this, because last year in a clear atmosphere the C line was certainly sharply terminated at the upper limit of the chromosphere or prominence under observation. Mr. Abbay, in his spectroscope, saw only the 1474 line and the F line—the former considerably the brighter of the two. He saw no continuous spectrum.

Mr. Pye saw C (8·5); D_3 (5·5); 1474 (10) and F (3). The numbers appended represent the relative brightness

of the lines. All of us, except Mr. Abbay, saw a faint continuous spectrum, but without any traces of dark lines. Still, as far as I am concerned, I should not dare to affirm that there might not have been dark lines without my seeing them, since on account of my using so great dispersive power this spectrum was very faint, except when I divided the slit considerably. With the instruments of Professor Winlock and Mr. Pye the case is different. I do not think, if dark lines existed, they could have escaped them.

No particular examination was made of the chromosphere spectrum, but I noticed it sufficiently to ascertain that there were probably in it, at least in the particular portion under observation, no lines which I had not already seen under ordinary circumstances. I saw C, D_1, D_2, D_3, 1474, two or three of the iron lines near E, the four lines of *b*, the barium lines between *b* and F, and one of the iron lines, often seen there, F, and 2185, which Mr. Lockyer considers a magnesium line. I did not examine the spectrum above this point, nor below C. This was on the eastern limb of the sun, near the point of last internal contact.

Perhaps I may be allowed to interpolate here, that on the previous day I saw in the spectrum a very bright, but small, prominence on the N.W. limb of the sun, the line below C, which I have seen twice before, but have so often looked for in vain that I had more than half concluded myself to be deceived. It is the reversal of the dark line, 656, of Kirchhoff's map. Also, in the same prominence, and for the first time, I believe, the three *chromium lines* just below *b*. Of course, D and D_2, and all the lines of *b*, were very bright in it, as well as many of the iron lines seldom seen.

But the most interesting spectroscopic observation of the eclipse appears to me to be the ascertaining, at the base of the chromosphere, and, of course, in immediate contact with the photosphere, of a thin layer, in whose spectrum *the dark lines of the ordinary solar spectrum are all reversed.*

Just previous to totality, I had carefully adjusted the slit tangential to the sun's limb, at the point where the second contact would take place, and was watching the gradual brightening of 1474 and the magnesium lines. As the crescent grew narrower I noticed a fading out, so to speak, of all the dark lines in the field of view, but was not at all prepared for the beautiful phenomenon which presented itself when the moon finally covered the whole photosphere. Then the whole field was at once filled with brilliant lines, which suddenly flashed into brightness and then gradually faded away, until, in less than two seconds, nothing remained but the lines I had been watching. The slit was very close, and the definition perfect. Of course, I cannot positively assert that all the bright lines held exactly the same position that had been occupied by dark ones previously, but I feel very sure of it, as I particularly noticed several groups, and the whole arrangement and relative intensity of the lines struck me as perfectly familiar. Mr. Pye saw the same thing for an instant only.

This observation is a confirmation of "Secchi's continuous spectrum" at the edge of the sun, and, I think, tends to make tenable the original theory of Kirchhoff as to the constitution of the sun and the origin of the dark lines in the ordinary solar spectrum.

AN EXAMINATION OF THE DOCTRINE OF ATOMICITIES.*

By FRANK WIGGLESWORTH CLARKE, S.B.

DURING the past few years the columns of the CHEMICAL NEWS have contained many papers upon the subject of atomicities. These papers have expressed the widest variety of views upon the doctrine, but have all (or all that I have seen) agreed in one important particular.

They take the body of the theory for granted; and, assuming that the various elements have distinct, definable atomicities, reason from this assumption as from an established fact. The greatest confusion has necessarily resulted. Thus, we have papers proving that sodium is tri- or sept-atomic; or that chlorine is a triad, or that some other element or radical has been incorrectly classified; but none to show whether the regularities and analogies upon which the doctrine of atomicities was first founded were sufficient to form an adequate basis for it. Very naturally, as a consequence of this manner of treating the subject, the theory has grown shadowy and indefinite, it has become difficult to explain coherently, and much of the discussion concerning it has, for want of a well-established starting-point, been utterly fruitless. It is my purpose now to examine briefly the various forms which the theory has assumed, or may assume, and, by comparing them with one another, to endeavour to show how far the main body of it is warrantable.

Now the question to be settled is, how far are we justified in supposing the various elements to possess, inherently and as a positive property of their own, definite atom-fixing powers; and, if such powers exist, how far have we any knowledge of them? If, for the sake of argument, we assume that such powers really belong to the elements, we shall find there are three essentially different modes of regarding them; and a careful comparison of these modes will afford us, not only the best means of learning their value relative to one another, but also the soundest criterion of the worth of the central theory itself.

First, we may suppose each element to have a definite, invariable atomicity. Second, it may be held that the atomicity of a substance varies within certain known or ascertainable limits. And third, it may be said that the atomicity of each element can vary to any extent below a certain definite maximum, whether we are able to attach any value to such a maximum or not.

The first form of the theory is one which, once held extensively, has of late been quite generally abandoned; although still it is accepted by a few authorities. And yet it is perhaps the most typical of the three, and has much higher claims to scientific precision of statement than the more popular second form. But, unfortunately, it is overthrown by the fact that two elements may combine in several different proportions. However, since this fact is sometimes evaded, or, rather, since some chemists attempt to evade it by means of an extra theory which is tacked on to the original doctrine, it becomes necessary for me to consider the nature of the evasion, and its scientific validity.

Let us take the familiar case of the chlorides of iodine, ICl and ICl_3. The two elements forming these compounds being both regarded as monatomic, it is said that in the first compound they saturate each other, the higher chloride being formed by a molecular union of the first with two atoms of chlorine. But, as has been objected by most authorities, if a saturated compound can unite with chlorine, a new force must be called into action, and of the existence of such a force we have no evidence whatever. Suppose, however, that such in addition to the original doctrine should be admitted; then comes forward another compound of chlorine and iodine, ICl_4,* and to account for this we must either invent a third force, or else admit the variable atomicity of one or the other of the elements involved. And a fourth compound of the two elements would call up a fourth force, and so on indefinitely. Over and above these objections it may be shown, I think, that to admit any notion of "molecular union" into the classifying of the higher compounds of any two elements would destroy all certainty in the theory of atomicities. For instance, carbon is called tetratomic. But suppose CH_2

* Read at the Troy Meeting of the American Association for the Advancement of Science.

* A crystalline solid, discovered by Kammerer.

should be isolated, would we not be justified in saying that CH_4 was merely a *molecular* compound of methylene with two atoms of hydrogen, and in looking upon carbon as a dyad? So, then, without stopping to consider any of the other well-known compounds analogous to the trichloride of iodine, we may lay this form of the doctrine of atomicities aside as untenable. But before passing further, it is worth while to notice an important distinctive feature of the view we have just been examining—viz., that a definite standard of comparison exists to form a basis for them. This standard is hydrogen, which, being looked upon as a monad, affords a starting-point for fixing the atomicities of all the other elements. The moment that we admit a variability of atom-fixing power, it is plain that this standard is gone; for we cannot say that the atomicity of every other element varies, while that of hydrogen alone remains stationary.

If we turn now to the second form of the theory, which teaches that the atomicity of a substance may vary within certain known limits, we find the elements commonly divided into two classes, according as their atomicities are represented by even or odd numbers.

Upon this view a monad can become either triad or pentad, but never dyad nor tetrad; while, on the other hand, a diatomic element may be also tetra- or hexatomic, though never tri- or pent-atomic.

But here again we come into collision with certain facts which cannot be set aside. Take, for instance, the chloride of iodine already cited; we have ICl_3 and ICl_4—is the atomicity of iodine even or odd? Vanadium forms three chlorides—VCl_2, VCl_3, and VCl_4; to which class does it belong? How shall we classify the biniodide of phosphorus, and how dispose of plumbo-triethyl and plumbotetrethyl? And how shall we account for the fact that, although manganese and chlorine belong in different classes, the permanganate and perchlorate of potassium are not only similar in formula, but are actually isomorphous? Many other examples might be cited in addition to these, but these seem to me sufficient to prove conclusively that no line can be rationally drawn dividing the elements into artiads and perissads.

Now, upon taking all these facts into consideration, it becomes doubtful whether we can ascribe a definite value to the atomicity of any element, and the whole subject grows vague and uncertain; for we have, as I have already suggested, no standard unit of comparison. If we look at any simple compound consisting of two elements, we have to make at least one, and sometimes two, pure assumptions, in order to decide upon the atomicity of either element contained in it. First, we must assume the atomicity of one to ascertain that of the other; and, secondly, in many cases we must take it for granted as to whether the compound may be called "saturated" or not. Take water as an example. If oxygen may have any one of half a dozen different atomicities, and hydrogen any one of as many more, then, in order to decide upon the value of the first element in water, we must arbitrarily select one of several values as the precise one which hydrogen possesses in that particular compound. Or, rather, we have to select two; for water contains two atoms of hydrogen, and we have no possible means of knowing whether these two atoms may not have two different values. And if, to simplify the selection of possibly appropriate atomicities for the two elements, we assert that water is a saturated compound, we make another pure assumption —we contradict the fact that water enters into union with other substances, and we obtain results which are of no more value than mere guess-work. And if we find such difficulties in considering so simple a substance as water, how much more troublesome it would be to write out the atomicities for so complicated a body as one of the silico-tungstates of aluminium, which contains (if formulated empirically) 126 atoms of oxygen and 87 of water of crystallisation.* And some of the so-called "indefinite" compounds would be even more; for instance, such a compound (a veritable *compound*) as Ordway has described, having the formula, $Fe_2Cl_6+23Fe_2O_3$. Such compounds seem to me absolutely inexplicable by any of the ordinary forms of the theory of atomicities; in fact, it seems as if theoretical chemists had actually run away from, or ignored them. They have not yet been firmly grappled with, and cannot be under the restriction imposed by current theories. Many of these compounds bid utter defiance even to the so-called "law of multiple proportion," which law, by the way, has been called in question by no less an authority than Grove.

If what I have thus far put forward be admitted, and we are unable to attach any definite known value to the atomicity of any element, then we are forced to fall back upon the third possible form of the doctrine, that every element possesses a certain maximum value, below which, as far as we know, it may combine in any proportion. But this idea of a maximum, as will at once be seen, is in its essence a necessity; since we cannot conceive that a single atom of one element may unite with an infinite number of atoms of some other. There must be a limit somewhere, although we may not be able to say where it is, and although it may possibly vary with circumstances of temperature and pressure. If we accept the atomic theory, we must believe in atom-fixing powers, and that belief takes shape most naturally and consistently in the idea of a maximum value; and if we reject the conception of atoms, although that rejection would be fatal to the first two forms of the doctrine of atomicities, the notion of a maximum still remains as the simplest expression of a necessary law of chemical union. For under no circumstances whatever can we conceive that a limited quantity of one species of matter can unite definitely with an unlimited quantity of another. So, then, although the first two forms of the doctrine may be objected to on account of their dependence upon another unproved theory, the third form is not essentially affected by any such sources of doubt. The words of the statement may be faulty, while its meaning is not. But, as I have already stated, although we know that maxima of combining power must exist, we cannot in any case establish the value of such maxima, for very obvious reasons. Thus, we cannot say that in CH_4 carbon is saturated, or, in other words, exerts its greatest atom-fixing or combining power, for we have no positive knowledge but that there may yet be discovered a CH_5, or even a CH_{10}. Such future discoveries may be improbable, but we have no adequate reasons for asserting them to be impossible. And we cannot even decide with certainty whether the different elements have one common value, or several different values, for their maxima, although we may have good reasons for surmising differences.

But, after all, although we may be able to ascribe definite atomicities to the elements, what is to be done with the regularities which so plainly connect the elements with one another? Hydrogen, oxygen, nitrogen, and carbon certainly unite together in such ratios that, as a rule, we can call them respectively mono-, di-, tri-, and tetr-atomic, and many other elements follow their lead. There must be a law somewhere to account for these regularities, and that law must do what the theory of atomicities does not, also account for the exceptions to them. The natural laws governing chemical union must certainly apply to all chemical compounds. Yet at present it seems to me that we know little more than that such a law, or laws, must exist. True, we may say that similar elements form similar compounds—a statement which is really the foundation of the doctrine of atomicities. Only that doctrine, as usually held, goes further than the facts warrant, ignores important compounds, and tries to impose limitations where it is by no means certain that any exist. Even this plain rule of similarity is not absolutely invariable; for every chemist knows that nitric acid and its compounds are more closely akin to chloric acid and chlorates than to phosphoric acid and phosphates. So, then, beyond the necessary law of maxima of combining

* See Watts's "Dictionary" upon silico-tungstates

power, and the faint light it sheds, we know no rule which is applicable to all chemical compounds. We are familiar with many regularities and analogies proving that substances are often simply related to one another, but the general law which shall bind them all together in a symmetrical whole is yet hidden in the future. For the present, we may look upon the doctrine of atomicities as a convenient, but faulty, system of mnemonics, which, properly used, enables us to grasp and handle large masses of facts, but which, when misunderstood and regarded as a law, only leads us into confusion. It does not even represent any law, it does not explain anything, it has many faults; but, after all, it is of great value for purely-memorising purposes, although it is liable at any moment to be supplanted by the invention or discovery of something better.

JACOBI'S THEORY OF ELECTRO-MOTORS.

By the Rev. H. HIGHTON, M.A.

(1). SOME time ago I promised an examination of Jacobi's classical paper in *Ann. de Chimie et Phys.*, vol. xxxiv., and I will now carry out my promise as briefly as the subject will allow.

M. Jacobi, as is well-known, was supplied with funds by the Emperor Nicholas of Russia, to construct an electro-dynamic engine, which he did, and applied it to work a vessel on the Neva. His engine was a failure in point both of economy and power; and he wrote to shew that this arose from the necessity of natural laws, and, consequently, that no one else could succeed.

His mathematical investigation of the subject led him to the conclusion that the power evolved by a battery was as the heat and as the expenditure of zinc. This chimed in exactly with the fashionable theory of the age, nearly put a stop to experiments on the subject by really scientific men, and left it to mere empirics. In fact, a successful electro-dynamic engine has been relegated almost to the category of perpetual motion and the philosopher's stone.

I hope to be able to show that Jacobi's conclusions are neither more nor less than a *reductio ad absurdum* of the doctrine of a mechanical equivalent of heat and chemical change.

(2). He shows clearly enough that when a magnet does work it diminishes the intensity of the currents and the consumption of zinc. This he attributes to the production of counter-currents, and investigates mathematically the supposed laws of these counter-currents; and from his premises he works out, in a mathematical shape, all the formulæ connected with the subject, the mathematical expressions for the maximum of work done, for the expense, for the connection between the work and heat, for the most efficient arrangement of the battery, &c. Grant the premises, and his conclusions follow. But it is also quite as clear that, if his conclusions are absurd, his premises and everything drawn from them by mathematical reasoning must be false.

(3). I set out, then, by asserting that his conclusions are absurd. For what is his main conclusion, on which all else is made to depend? It is his formula for the maximum work of an electro-dynamic engine.

This is—

$$T_0 = \frac{n_2 k_2}{4\rho\kappa}$$

in which T_0 = maximum of work; n, number of cells in battery; k, electro-motive force of the elements used; ρ, the total resistance of the circuit; κ, a coefficient depending on the inertia of the iron used in the magnets. Now I say that this great leading formula, on which all his other deductions are based, is utterly absurd, and, therefore, by the *reductio ad absurdum*, disproves his premises and all the conclusions based upon them. For, it will be observed, that, other things being equal, the maximum work varies inversely as the total resistance of the circuit. Now, since the total resistance of the circuit is least when the coils of wire are shortest and thickest, it follows that the shorter and thicker the wires of the coils the more work the engine does, and that it does most work when there are no coils at all! If this is not a *reductio ad absurdum* I do not know what can be. M. Jacobi seems half conscious of this, for he draws attention to it and comments on it; but the absurdity and impossibility of it seems never to have struck him. Well may M. de la Rive call this a remarkable conclusion, that (as he expresses it) the maximum work of an engine should be entirely independent of the number and size of coils; but the absurdity of it seems never to have struck him either. The fact is that the mathematical expression of physical phenomena, unless checked by the utmost care about the premises, is like a powerful but self-willed steed which takes the bit into its mouth and rushes with its rider into a slough of absurdity. Let me add that κ will be found, on examining the reasoning, to be inversely as the inertia of the magnets, and, consequently, there is this second absurdity that the greater the inertia of the magnets the greater the work done by them. But this is a trifle compared with the first absurdity. I should say that in one place his formula is printed—

$$T_0 = \frac{n_2 k_2}{\rho_2}$$

but this seems to be a misprint. Even if correct, it would not affect my main argument—but I think there is no doubt it is a misprint.

(4). I am not bound to show where the error or errors in the premises lie, but I will point out several possible sources of these errors. (1). He assumes the power of a magnet to be represented by $l^2 \times i^2$, l being length of wire in coils, and i the intensity of current. This in actual practice is very seldom, if ever, true. (2). He assumes, without proof, that the diminution of the intensity is due simply to counter-currents. (3). He assumes that the intensity of these counter-currents is expressed by—

$$\frac{\kappa m \beta v}{\rho}$$

where κ is a co-efficient depending on the inertia of the magnets, m the magnetic power, β the length of the wire of the bobbins, v the velocity of motion, ρ the total resistance of circuit. (4). The nearness to which the magnets approach each other in their motion never enters into any of his formulæ, so that the power would appear to be the same if they never approached nearer than an infinite distance. But, above all, he never checked his mathematical conclusions by his practical experiments to see if they agreed. He expressly tells us he did not do so.

(5). He admits that when M. Botto, of Turin, assisted by a committee of *savans*, tested the matter practically, they found that the force did not vary as the consumption of zinc or as the heat, but he sets aside these conclusions by remarking that M. Botto measured the work performed by the number of foot-pounds raised; but that he ought to have taken as his measure the number of foot-pounds multiplied by the velocity with which they were raised, which I need not say is a wholly erroneous view. A foot-pound raised from a state of rest to a state of rest, is a measure of the same total amount of power, with whatever velocity it be raised. The only difference is that in one case a higher force is exerted for a shorter time.

(6). Let me draw the conclusion, then, that having cleared the way by showing the falsity of our ordinarily accepted theories, it remains for us to do what has never yet been done, namely, to establish by a very large number of properly devised experiments, carried out by scientific men combining for that purpose, the true and full laws which regulate the connection between chemical change

in a galvanic battery and magnetic power; and by constructing, as has never yet been done, an electro-dynamic engine, built on really sound principles, to ascertain whether electro-dynamic power can be used conveniently and economically. It is asserted, on apparently good authority, that the practical problem has been most satisfactorily solved in America. Once clear away the fallacious theories now prevailing, and time will soon bring us to sound practical results.

Putney, February 15, 1871.

ON THE

PROPERTIES OF IRON AND STEEL AS APPLIED TO THE ROLLING STOCK OF RAILWAYS.*

By Sir WILLIAM FAIRBAIRN, Bart., LL.D., F.R.S., &c.

Dr. Joule communicated to me the discussion which took place at the last meeting of the Society, on the question of the effects of intense cold on steel tires. This enables me to refer to a series of experiments which had for its object the effects of various degrees of temperature on wrought-iron. These inquiries are, to some extent, analogous to the cause of the recent accident which occurred on the Great Northern Railway, near Hatfield, by the breaking of a steel tire, which caused the death of a number of persons.

It has been asserted, in evidence given at the coroner's inquest, that the breaking of the steel tire was occasioned by the intensity of the frost, which is supposed to render the metal brittle, and of which this particular tire was composed. This is the opinion of most persons, but, judging from my own experience, such is not the fact, and provided we are to depend on actual experiment, it would appear that temperature has little or nothing to do with it.

Some years since I endeavoured to settle this question by a long and careful series of experiments on wrought-iron, from which it was proved that the resistance to a tensile chain was as great at the temperature of zero as it was at 60° or upwards, until it attained a scarcely visible red heat. To show that this was the case, and taking, for example, the experiments at 60°, it will be found that the mean breaking weight, in tons, per square inch, was in the ratio of 19 930 to 21·879, or as 1 : 1·098 in favour of the specimens broken at the temperature of zero.

The generally received opinion is, however, against these facts, and it is roundly asserted that the strength of iron and steel is greatly reduced in strength at a temperature below freezing. The contrary was proved to be the case in wrought-iron plates, and assuming that steel follows the same law, it appears evident that we must look for some other cause than change of temperature for the late fracture of the tire on the wheel of the break-van of the Great Northern Railway.

In our attempt to investigate the cause of the failure it may be interesting to show how the experiments on wrought-iron to which we have referred were obtained at various temperatures, and subsequently to give the results as found in the summary.

The immense number of purposes to which both iron and steel are applied, and the changes of temperature to which they are exposed, renders the inquiry not only interesting in a scientific point of view, but absolutely necessary to a knowledge of their security under the various influences of those changes, and when it is known that most of our metal constructions are exposed to a range of temperatures varying from the extreme cold of winter to the intense heat of summer, it is assuredly desirable to ascertain the effects produced by those causes on material from which we derive so many benefits, and on the security of which the safety of the public frequently depends. It was for these reasons that the experiments in question were undertaken, and the summary of results are sufficiently conclusive to show that changes of temperature are not always the cause of failure, as that which occurred near Hatfield on the Great Northern Railway.

That such is the fact, I may adduce several accidents of broken tires, all of which occurred during the spring and summer months when the temperature was high. One of them occurred on the Lancashire and Yorkshire Railway in the summer of last year when the temperature was 50° to 60° above freezing. I could enumerate others in which the winter frosts had nothing to do with the fractures which ensued.

It might have been satisfactory to have shown the process by which the following results were obtained; suffice it to observe that all the specimens were torn asunder with and across the fibre in oil- and water-baths, and those under the freezing-point were made in a snow bath reduced to zero. The summary of the results is as follows:—

Summary of Results.

No. of the experiments.	Temperature, ° Fahr.	Breakage weight per square inch in lbs.	Breakage weight per square inch in tons.	Remarks. Duration of strain in regard to fibre.
1	0	49·009	21·879	With
2	60	40·357	18·001	Across.
3	60	43·405	19·377	Across.
4	60	50·219	22·414	With.
5	110	44·160	19·714	Across.
6	112	42·088	18·789	With.
7	120	40·625	18·136	With.
8	212	39·935	17·828	With.
9	212	45·689	20·392	Across.
10	212	49·500	22·098	With.
11	270	44·020	19·651	With.
12	340	49·968	22·307	With.
13	340	42·088	18·789	Across.
14	395	46·086	20·574	With.
15	Scarcely red	38·032	16·978	Across.
16	Dull red	30·513	13·621	Across.

From the above it will be seen that the plates from which these results are obtained are much stronger in the direction of the fibre than across it.

The above experiments are quite conclusive as regards the strength of wrought-iron plates till they approach red heat. At that temperature nearly one-half the strength is lost; it becomes exceedingly ductile, and may be drawn to a considerable extent in the direction of the fibres before it breaks.

Another series of experiments were made on wrought-iron bars, which indicated somewhat different results. In these experiments the specimens from the same works attained the maximum of strength, and gave at the temperature of 415° a resistance of 39·072 tons per square inch, and at zero and 60° there were little or no differences, excepting in the case of temperature, when the resistance was increased from 28·419 at zero and 60, to 39 tons per square inch at 415°. This may, however, be accounted for from the increased manipulation of rolling, where the fibre is drawn and elongated to a much greater extent than in plates. This does not, however, affect to any great extent the ratio of compression and extension as regards the effects of temperature, although I should be inclined to take the experiments on plates before that on bars, as analogous to the broken tire, which, it must be borne in mind, is without weld and perfectly homogeneous.

The danger arising from broken tires does not, according to my opinion, arise so much from changes of temperature as from the practice of heating them to a dull red heat, and shrinking them on to the rim of the wheels. This, I believe, is the general practice, and the unequal, and in some cases, the severe, strains to which they are subject has a direct tendency to break the tires.

* Read before the Manchester Literary and Philosophical Society, January 10th, 1871.

To show how easily this may be effected, let us suppose that a tire, two feet six inches or three feet diameter, is shrunk on to a wheel one-tenth of an inch larger than the tire, it then follows that the tire in cooling must be elongated to that extent, with a strain equivalent to the force of the shrinkage, and calculated to produce that amount of molecular disturbance. It may be more or it may be less, but supposing the strain to be one-half or three-fourths of that which would break the tire, it then follows that the constant action of its irregular motion on the rails must ultimately lead to fracture.*

I am not surprised that this should be the case, as most, if not the whole, of railway tires, excepting those on engines and tenders, are not turned but selected by hand, heated and shrunk upon the wheels with every degree of tension, as suits the convenience of the workmen. So long as this process is pursued, the public will be exposed to the risk of broken tires.

What is required in this description of manufacture is, that the rim of the wheel and the inside of the tire should be *turned to a standard gauge*, accurately calculated to give the required amount of tightness with a larger margin of strength, and this done we should attain greatly increased security to the public, and a great saving in wear and tear—to say nothing of the large sums expended by companies in the shape of compensation for injuries and loss of life.

ON FERMENTATION.†

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from p. 67).

In the process of making wine, there are a considerable number of operations which are occcasionally considered rather extraneous to normal wine-making, and are by many persons classed amongst frauds. Materials are sometimes used in aid of the natural constituents of the grape-juice, materials which contribute to the quality of the product; some of them by adding materials to it, but others simply removing from the substances bodies which are not wanted in it. And I must say that it does appear to me a great error to object to the introduction of any new conditions which may be found to effect an improvement in the product. I do not think it is reasonable to suppose, because wine is only known by the vulgar as fermented grape-juice, that for that reason nothing but grape-juice ought ever to be used in the manufacture. I think it would be desirable—in fact, it ought to be almost compulsory—that persons should state what materials are present in substances which they sell to the public; but, I think, with that safeguard, it would be right to leave manufacturers perfectly free to employ whatever materials they might find most conducive to the elaboration of their products. In some countries, grape-juice is exceedingly rich in acid and poor in sugar (and I think a good deal of wine is rather of that class), and wine-makers in such districts find that their stuff is more drinkable if some of the acid present in it can be removed before it is sent out. They therefore put into the must, in fermenting the wine, some chalk, and the lime which is present in the chalk combines with the tartaric acid and takes it out of the liquid. Thus, the sour liquid is rendered less sour, and certainly that is not, in any degree, or to any extent whatever, a fraudulent admixture. Nothing is added, but only an unpleasant substance is taken from it. It also happens in precisely the same districts, that from the paucity of sugar which is present in the grape-juice, the wine is too weak in alcohol; and that to meet the requirements of consumers, many wine-makers now add sugar in the process. Now sugar is one of the natural and proper constituents of grape-juice, and if the grapes contain too little of it, it does seem quite proper and desirable that more should be added. However, in the subsequent making of wine, there are several other processes which are less natural than these, and about which some greater difference of opinion may possibly prevail; and one of the commonest, not only amongst wine-makers, but also amongst wine-consumers, is the process of fining. In order to establish the effect and the meaning of this process, I think we must trace back the history of wine from the time in which it is first put into casks by those who produce it to the time at which it gets into the hands of consumers. It is customary—I cannot say whether it is universal or not, but I believe it to be so almost—to put new wine into new casks; and in the better districts oak casks are used. New wood is far more porous than old wood when used for such a purpose. And of course the wine, when put into the cask, sinks into the wood, so that the outer surface is moistened, and allows some of the water and alcohol, and the various volatile materials to evaporate. In fact, the wine diminishes during the first year of keeping in wood very rapidly, by a process of evaporation. But this is not all. Whilst the water and alcohols are evaporating from the outer surface, air is dissolved by the liquid which is in the wood. Air actually diffuses itself through the wet wood into the body of the wine in the cask; and what is more than this, the water and alcohol which go out are replaced by something. The cask does not collapse, nor is there a vacuum produced above the liquid. The wood is always sufficiently leaky for air to come into it, and there is always a space left above the wine. Wine-makers are, therefore, in the habit of filling up their wine-casks periodically. In some districts in France, they are filled up in the first year three times, at three different periods; and, in the second year, they are filled up only twice, but only at perfectly definite periods or seasons, which have been found, for those particular wines, to be most advantageous. But each time the wine, if examined carefully, is found to have undergone, not only what we chemists should call a process of concentration, the solid substances dissolved in the liquid of course always remaining behind, the proportion of liquid being diminished, but, at the same time, it has undergone other changes, that is, there is a deposit formed from it. Some of the bodies present in it, either by themselves or by forming compounds with others added to them, form a sediment, and in the wine-growing districts it is customary, and I have no doubt necessary, to decant the wine and pour it off carefully from the deposit many times, for the presence of the deposit, if continued in the wine, would be injurious to the future changes which it has to undergo. When this comes into the hands of the consumer, there is suspended in the substance of the wine some of this deposit,—some solid particles which might be got to settle down, but which could not easily be removed completely by any process of mere subsidence, and the processes of fining, which are exceedingly various, have for their object the more complete removal of these solid particles by forming compounds with them. In some cases, the process consists in forming what I might call a sort of mordant, or something like a process of dyeing, in which a gelatinous compound is formed in the body of the liquid, which carries down with it a good deal of colouring matter, which it encloses, and which does, while going down, take with it a number of little filaments and cells which were floating in the liquid, and which were so exceedingly light that they would not have settled and could not have been removed otherwise. This point is particularly important in relation to a process which I shall presently mention. In some cases, it has been thought the wine contained too much albuminous matter.

* From long continued action under strain, it has been proved that it is only a question of time when rupture takes place, as repeated increased and diminished changes with the same load ultimately leads to fracture.

† The Cantor Lectures. Delivered before the Society of Arts.

The theory of fermentation which was held for a long time, and which we considered at one of our previous meetings, consisted in attributing the process to the decomposition of the albuminous matter which is present in the fermenting liquid. It was supposed that there was too much of this albuminous matter present, and that it remained and was inclined to do further work. One process which has been adopted to a considerable extent in the champagne districts, where that was supposed to occur, consisted in adding tannin, a substance which I have already spoken of, which carries down a good many albuminous bodies, forming a precipitate with them, and with these no doubt carries down the solids which may be in suspension. Then another process, which really bears a considerable resemblance to this one in principle, although not in form, is that of sulphuring, using sulphur in the casks, which, of course, you would understand at cnce, exerts an antiseptic action. It is, in fact, a process which consists in producing a material which is, in plain English, a poison to any germs which may happen to be present, whose action must consist, as far as it goes, in arresting the vitality,—in stopping any work which they were doing. M. Berthelot, who has made many accurate experiments regarding the composition of wine and the changes which it undergoes, subjected some wine to the action of a known quantity of air, and by examining the wine afterwards he was led to the conclusion that air is an unmixed evil to wine when once it is fully made. There are certainly many general observations which everybody must have had occasion to make which agree with that. If we open a bottle of wine and use half of it, especially if we leave a bottle of light wine open for some little time, everybody knows that it deteriorates in quality, and becomes flat, or even sour. In a great many cases, it is found that there is a development on the surface of the wine, and if you were to examine it carefully you would easily see, especially in light French or German wines, a pellicle—in fact, the vinegar cells; and their presence must have the effect of promoting the oxidation of the wine. M. Berthelot's experiments confirm the general obervation, which everybody makes more or less definitely, that air is noxious to wine when present in any quantity. But M. Pasteur has arrived at precisely the opposite result. I do not mean to say that he says air cannot do harm, but that what is hurtful in air is the excess of it, or the too rapid rate of its action. He lays down the principle that every ripening of wine, or the process by which young and crude wine is changed into good old wine, consists in a process of slow oxidation; that is its very essence, and that without that, a crude young wine cannot be mellowed or transformed into a good old wine. The evidence which he gives for his conclusion is exceedingly simple, and I must say it appears to me exceedingly conclusive. He has, for the purpose of keeping wine with air, and for the purpose of keeping it without air, resorted to appliances which are far more effectual than those generally resorted to in common life. You may be aware that a cork, even what we should consider a good cork, does not completely prevent the communication of external air with liquids in a bottle. I do not suppose many people can know how much air passes in and through a cork, but the quantity is very great. M. Pasteur sealed up some young green wine, by putting it into a glass vessel, and then he melted up the neck, so that he had no air present with it. He then kept it for a considerable time, and he found that this wine, even after years' keeping, was as green and as young as at first; that wine kept under conditions such as that air could have no access to it did not undergo, to any extent, the change which was wanting, and that it did not improve by keeping. He then sealed up, in a similar vessel, some wine with air, and he subjected the wine, with a known quantity of air, to various influences which were calculated to accelerate the action of the air upon it, and amongst these I ought specially to mention that of light. He took some small vessels made of perfectly clear glass, and sealed up his wine, various qualities of it, in these little vessels with air, and then exposed them to the sunshine in the south of France. He found that the oxygen of the air was totally dissolved, and that, when he examined the air, the oxygen had gone, but he found that his wine then did pass over rapidly into a state exceedingly like that into which it passes by the ordinary process of keeping in bottle. It lost its harshness, and became like old wine, which it resembled very greatly in its quality, and also in its composition the older kinds of wines. At the same time he found that there was formed in such quality of wine a considerable amount of deposit, and his explanation of the way in which oxygen acts so as to improve the quality of the wine is this, that it serves gradually to take away from the wine various substances which are present in it, and that the deposit is due to an oxidation of the colouring matters present, which have an unpleasant, astringent, harsh taste, and it also consists in acting upon the alcohol of the wine and upon the various organic liquids in it in a similar manner. This result is certainly one of very great importance; for if the process of improving wine requires the action of oxygen, and if, on the other hand, the action of oxygen may do much harm—I mean if all the good has to come from the oxygen, and if all the worst evils come from oxygen,—and that really is the position in which the question stands upon our present evidence, it must be of the greatest importance to ascertain what are the conditions under which the beneficial action can be exercised, and what are those under which its detrimental influence occurs. In that respect, both of the observers I have mentioned, and others also, have established some remarkable facts, but in order to appreciate them duly, it will be necessary for you to know something of the general character of compounds to which I must now make allusion.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

February 16th, 1871.

Professor Williamson, F.R.S., President, in the Chair.

The following gentlemen were elected Fellows:—W. D. Herman, W. W. Holder, G. Lockyer, jun., W. J. Lockyer, J. E. Mayer, R. Meldola, M. M. P. Muir, W. J. Reynolds, W. Smith, T. E. Thorpe.

The following papers were read:—

"*On the Production of Wood-Spirit*," by E. T. Chapman.—The author began by remarking that the difficulties of obtaining correct information regarding this production are very great: the manufacturers are too jealous to disclose their secrets.

The first points which have to be considered in establishing a distillery are, an abundant supply of wood, and, even more important than this, a ready market within reasonable distance for the sale of charcoal. Charcoal is a bulky article, deteriorates easily by travelling, and, as a rule, has a rather high tariff fixed upon it by railway companies and carriers. An abundant supply of water is also needed. The author then proceeded:—

I would first consider the various kinds of wood employed, and the kind of charcoal produced from it; then the ovens, or retorts, in which it is charred; the condensers; and, lastly, the various methods of separating the wood distillate into its proximate constituents, and rendering it saleable.

In this country, the woods preferred for distilling are oak, beech, birch, thorn, crab, or apple; many other woods are, however, in use, such as hazel, alder, ash, and maple. Excepting under very special circumstances, poplar, elm, and nearly the whole tribe of coniferous woods are not distilled in this country. Holly and yew are highly esteemed for distilling, but are, of course, not sufficiently common to form a serious item in the wood-distillers' consumption.

Oak.—The oak employed is almost invariably either the "lop"—that is the lesser branches of large trees—or the saplings specially grown for their bark. In either case. the greater part of the bark is removed before it comes into the wood-distillers' hands. This peeled oak, as it is called, would probably be used to the exclusion of every other wood, could it be obtained in sufficient quantity and at the same price as other woods; the only objection to it being that its charcoal is brittle, and, therefore, in wood-distillers' phrase, does not measure well. Charcoal *buyers* make no objection to it.

Beech.—I have, personally, no experience of the use of beech-wood, excepting under circumstances which gave me no clue to its value; however, it is reported to be a particularly good wood. It is, I believe, the only wood frequently employed in England in the form of large trees. Such wood is frequently blasted to pieces with gunpowder before use. It is first sawn into such lengths as will go into the ovens; then holes are drilled in the logs, sometimes in the end, sometimes in the side, a charge of powder introduced, and the log blasted. Beech-wood furnishes much tar—it is said to give a particularly large yield of naphtha, and to be the best source of creosote; its charcoal also is good.

Birch.—This wood is, beyond all doubt, one of the very best woods for wood-distilling purposes, if the object be to prepare pure acetic acid, or a pure acetate; the same remark applies to thorn and apple; but it is by no means always the object of the wood-distiller to prepare a particularly pure acetate—consequently, the estimation in which these woods are held varies with the object which the distiller has in view, as will be explained more fully bye and bye.

Coppice woods, generally consisting of a mixture of hazel, alder, ash, and maple, together with smaller quantities of birch, beech, and oak, are very extensively used by wood-distillers. These woods vary in value greatly according to their size; they are, perhaps, of most value when of from fourteen to eighteen years' growth. They are generally cut much too early in this country, sometimes at the end of six years; according to German experience, the greatest yield of wood is not attained until much later.

I think it may be taken as a general rule, hardly liable to exception, that the larger the wood is that is supplied to the wood-distiller the better for him, provided it be not so large that it cannot be charred completely in the twenty-two hours or so during which it is in the ovens.

Roots of trees are occasionally distilled, provided they are not decayed, and are broken up into moderate-sized pieces; they generally furnish a very valuable distillate, and not bad charcoal. The chief objection to them is the great labour required in breaking them up; they are sometimes put into the ovens whole, and heated for forty-eight hours. Decayed woods of all kinds are to be avoided, especially large logs a part of which is decayed; or, at least, great caution must be employed in using them, as the charcoal from them generally proves to be spontaneously combustible. Holly-wood also yields a charcoal which is very liable to spontaneous combustion. The spontaneous combustion of charcoal appears seldom to take place unless perfectly dry charcoal, fresh from the oven (though quite cold and without fire in it), be heaped together in such a manner that the air cannot play freely upon it—put into sacks, for example. It then gets gradually hotter and hotter, and finally catches fire. After being freely exposed to the air for twenty-four hours, the danger appears to be completely averted. Of course, were the combustion due to a spark left in the charcoal, this free exposure to air would certainly cause it to blaze up.

Ovens.—The ovens, or retorts, employed in wood distilling are very various in form; those employed in this country are always constructed of iron, either cast or wrought, and it is a disputed point which is the best material. According to the experience of one wood-distiller, the wrought-iron ovens will hardly last thirty charges; according to another, they have lasted five or six years. The truth, I take it, is that, properly protected by brickwork, and with careful firing, wrought-iron quite equals the cast in endurance; it is, of course, much less liable to crack, and may be made very much thinner than cast-iron. Its disadvantages are, that it easily loses its shape by over-heating, and is somewhat apt to leak at its joints. Wrought-iron ovens are always furnished with cast-iron exit-pipes and doors, as wrought-iron is rapidly attacked by the acid vapours, unless it be sufficiently hot to prevent any condensation upon it. I am not aware that wrought-iron has ever been employed for the construction of any but cylindrical ovens.

Cast-iron is most commonly employed—its advantages are obvious; thus the whole oven may be, and frequently is, cast in one piece, so that there is no chance of leakage from it. Its disadvantages are, that it is apt to crack with the fire, and when cracked it is difficult to repair it satisfactorily; this is especially the case with cylindrical ovens cast in one piece. The ovens we employ are cast-iron; they are square in section, and consist of plates of cast-iron fastened together with bolts and nails.

The following are the dimensions of various ovens that I have either seen and measured myself, or have obtained measurements or drawings of:—

	Description of Oven.	Length.	Diameter.	Depth.	Breadth.	Area.
		feet.	feet.	feet.	feet.	Cubic feet.
1.	Wrought-iron Cylindrical ..	7·0	5·00	—	—	137·50
2.		9·5	3·50	—	—	91·39
3.	Cast-iron .. Cylindrical ..	10·0	3·50	—	—	96·00
4.		10·0	3·10	—	—	115·40
5.	— —	4·5	3·00	—	—	31·77
6.	— —	7·0	2·50	—	—	34·30
7.	Rectangular ..	7·0	—	4·50	3·66	115·29
8.	— —	9·5	—	5·00	3·66	174·00
9.	— —	10·0	—	4·33	4·33	187·76

Of these ovens, Nos. 1, 3, and 4 have been reported upon very favourably. No. 5 is an old pattern not likely to be ever again constructed, though still in use. No. 6 is a form still used for special purposes, but not now, I believe, excepting for the preparation of special kinds of charcoal; and in one establishment with which I am acquainted two such ovens are retained for burning heavy timber from ship-breakers' yards. No. 7 is not, I believe, an approved pattern. No. 8 is considered to be a very good oven; I believe some constructed on this pattern have been in use nearly twenty years. No. 9 are those employed at our own works; they are the largest with which I am acquainted. I have not mentioned the places where these various ovens are, because in two cases I have been requested not to do so, and in others I have no permission to publish the data. No matter what ovens are employed, they all require to be properly protected with brick-work from the direct action of the fire. No. 1 is protected for two-thirds of its circumference; one fire heats two of these ovens. No. 2 is similarly protected, but each oven has an independent fire. Nos. 3 and 4 are only protected at the bottom; they have a separate fire to each. I have seen ovens of the diameter of No. 4 arranged similarly to No. 1—that is, one fire to two ovens. No. 5 had an independent fire; No. 6 the same. All rectangular ovens have an independent fire for each. As a rule, rectangular ovens are only protected with brick-work as to their bottom plates. The exact arrangement of flues it is not necessary

to describe, further than to say, in general terms, that it is such as to heat the oven as equally as possible.

The author next gave a detailed description of the various kinds of retorts, the mode of charging them, and then continued—

The temperature at which wood is distilled makes a marked difference in the nature of the products obtained from it. Thus I have, in a series of experiments made on oak-sawdust, obtained from 13 to 27 per cent of the sawdust in the form of charcoal, simply by varying the temperature at which the charring was effected. The higher the temperature, the smaller the yield of charcoal; but if the heat be very gradually increased, so that the charring is effected at a low temperature, and the charcoal produced subjected to a high temperature, the yield is invariably only slightly smaller than when the wood has been charred at a permanently low temperature; so that the large yield in the case of the low temperature is not due to imperfect charring. When the wood is very rapidly charred, the amount of liquid which distils from it is always smaller than when it is charred slowly, and the amount of uncondensable gas greatly larger. The amount of acetic acid produced is also larger at a low temperature than a high one.

The maximum amount of naphtha is obtained at a temperature considerably higher than the maximum amount of acetic acid; that is to say, in laboratory experiments where the condensation is of the most perfect kind. But the large volume of permanent gas produced at a high temperature must tend to carry away much of the naphtha with it; still it is generally believed that more naphtha is obtained in a wood distillery, when the ovens are small and highly heated, than in one where the reverse conditions obtain. And, on the other hand, it is, I think, admitted that more acetic acid is obtained with large ovens at a low temperature.

I will now proceed to describe the general process of wood distilling. The vapours which are generated by the action of heat on the wood in the ovens are generally taken off by a pipe from either the upper part of the end, or from the top, and conducted direct into the condensers. Here water, acetic acid, naphtha, and tar are more or less perfectly condensed, but a large volume of permanent gas passes through the condensers. In this country, it is generally allowed to escape; abroad, it is almost invariably burnt under the ovens as fuel. The composition of this gas varies greatly with the time that the wood has been in the oven, and with the heat employed; if a sample of it be collected soon after the decomposition of the wood has commenced, it will be found to be colourless, have scarcely a perceptible smell, and to burn with a blue flame. It consists of a mixture of carbonic acid, hydrogen, and a hydrocarbon, apparently marsh-gas. As the temperature rises, the escaping gas becomes clouded with a tarry smoke; it now burns with a yellow smoky flame. If a sample of it be collected and allowed to stand for two or three days in a bottle, it deposits tar and becomes colourless; and if the temperature of the ovens has been kept very low, it burns with a yellow, slightly luminous flame. It contains carbonic acid and gases absorbable by bromine. After the carbonic acid and the gases absorbable by bromine have been removed, the gas burns with a blue flame like that of alcohol, and consists of a mixture of carbonic oxide and hydrocarbon gases—probably chiefly marsh gas. Towards the end of the operation, the gases are richer in illuminating power, not derived from gases absorbed by bromine. Even right up to the end of the operation, however, the gas, when purified by long standing, treatment with bromine, washing with alcohol, and then with aqueous potash, has very little illuminating power. It does not appear, therefore, that any hydrocarbons of the marsh-gas series of at all complex formula are produced. Carbonic oxide increases in quantity as the operation proceeds; and towards its close, especially if the temperature be allowed to rise rather high, its quantity is very considerable. I have had neither time nor opportunity to make a full and careful examination of the gas, but the foregoing sketch will give a general idea of its nature. I I think I may say that it consists essentially of carbonic acid, marsh gas, and carbonic oxide, with varying quantities of olefines, probably ethylene. The carbonic oxide increases in quantity throughout the whole operation till its close. The illuminating power of the gas, after standing and washing in alcohol, is greatest somewhat before the close; but if the temperature be kept low in the ovens, the illuminating power in the washed gas is never high. Of course the gas is charged with volatile liquids produced during the distillation, therefore it has considerable illuminating power as it leaves the condensers.

The liquid products of distillation, which form two layers—the upper aqueous, the lower tarry—are differently treated according to the object the manufacturer has in view. After having given an exhaustive account, which does not conveniently allow an abbreviation, of the products obtained in wood distilling, the author went on to say that some easier and more exact methods of estimating the commercial values of the various products ought to be introduced.

"*On the Effects of Pressure on the Absorption of Gases by Charcoal*," by John Hunter. Very numerous experiments lead to the observations—first, that the amount of absorption increases with the pressure to which the gas is exposed; and secondly, the same change of pressure produces about the same amount of increase in the quantity of each gas absorbed.

"*On the Solubility of the Phosphates of Bone-Ash in Water holding Carbonic Acid*, by R. Warington.

NOTICES OF BOOKS.

Victoria.—Abstracts of Specifications of Patents applied for from 1854 *to* 1866, *Ac to Bu.* Illustrated by copies taken from the original drawings. Prepared in the Patent Office attached to the Registrar-General's Department, Melbourne. By William Henry Archer, Registrar General of Victoria. Melbourne: by authority, John Ferres, Government Printer. 1870.

The author of this work states that, in selecting the plan of preparation adopted in the present volume, and proposed to be continued in the succeeding volumes of abstracts, two objects have been kept especially in view; firstly, to produce the work in so cheap a form that the mere question of price would not exclude it from the library of an inventor of moderate means; and secondly, to omit no essential item of information contained in the original descriptions. The arrangement of the various divisions is alphabetical (this volume begins accordingly with Accidents—prevention of, and ends with Buoys), but in consequence of the importance of mining as a national industry, it is intended in the second volume, now in course of preparation, to describe all patents relating to the extraction of metals, without waiting till the publication of the letter "M" in ordinary course. The abridgments of the specifications were prepared by Mr. T. Harrison, of this department. We congratulate the people of Victoria, not only on the zeal shown by their officers of the Registrar General's Department in preparing this excellent volume, but also for having carried the matter out with a very decided improvement, viz., the addition of a large number of beautiful photo-lithographic plates exhibiting copies of original drawings added to the specifications: in this respect the people of Victoria are ahead of their brethren in the mother country, and have taken a good hint from the practice so effectually carried out at the Patent Office of the United States. As far as we are able to judge, the abridgments are very well made, and clearly give the essence of the subject matter; the typographical execution of this and of other works issued by this office is highly commendable.

MISCELLANEOUS.

The Abbé Moigno.—From a letter just received from the Abbé Moigno, dated Paris, February 15, 1871, we understand that this distinguished *savant* had a narrow escape during the bombardment. A shell exploded in his bedroom and destroyed more than a thousand valuable books, but he escaped uninjured. *Les Mondes*, the publication of which was suspended last September, will reappear as soon as communications are open. M. Ch. Girard has, we regret to say, received serious injury from the fall of a shell, but our readers will be glad to hear that he is now convalescent.

Magnetic Motive Power.—If we mistake not we are in the dawn of a new and economic motive power. We have long had an instinctive expectation of its approach. Now, by the scientific exposition of the possibility of an infinitive development of magnetic power by an apparently inadequate initial force, as argued by Mr. Highton in the Chemical News of Dec. 2nd, 1870 (vol. xxii., p. 205), and the apparent product of it before our senses, seemingly verifying the argument, we are strongly induced to hail magnetism as the coming worker for millions of men and for purposes innumerable. Some will smile at this. It is right they should. The idea has been discouraged by electric writers as visionary and impracticable. They have asserted the impossibility of any such economic use of material for the production of magnetic power as could ever justify the hope of its substitution for steam. They were right so long as the battery was regarded as the source of power instead of a mere initiative, such as results now seem to prove it to be. Our theories of electro-motive force may require to be re-examined, and, perhaps, changed. The axiom that a given magnetic force is the exact product of a given consumption of zinc or chemicals must now be challenged and put to proof. We confront now this proposition that, although the electro-motive force may be in the battery, yet that the magnetic power which follows its application is capable of indefinite enlargement without increase of the initiative agent. We are brought face to face also with the fact that when a magnet is performing its maximum work the battery which started the magnetic power is most at rest! In other words, that the magnetic power is not proportioned to the size or consumption of the elements of a battery, although dependent upon it as an initial force. We do not pretend to explain this problem, but we can tell what we have seen. It seems to corroborate the recent position taken by Mr. Highton, of England, another of whose articles we reproduce to-day from that excellent paper the London Chemical News, and to prove that we are on the borderland of a new and wonderful series of development of an economic, safe, and efficient motive power. It may prove that our assumed data as to the power resident in our battery materials has been underrated, and their productivity misunderstood. Let us now state what we have seen. A few days ago we accompanied, on invitation, several gentlemen to the works of Mr. H. M. Payne, of Newark, N.J. (The editor here gives a description of a powerful magnetic saw-mill which he saw in action). This rapid and effective action has been watched nine consecutive hours by investigating parties, without any perceptible decline of power, and with a consumption of less than half a pound of zinc, at a cost of less than half a cent per hour. The power developed was rated at two-horse, and can be maintained for twenty-four hours without intermission at a maximum cost of ten cents. Such at least is the statement made to us by Mr. Payne, and confirmed by a well-known gentleman, who thoroughly examined it. By increase of diameter and width, or by multiplication of wheels, and the number of magnets, the power can be largely increased, so we were assured, *by the same number of cells*. This was proven by the fact that, by the addition of wire in the circuit, of sufficient length to surround another set of magnets, no diminution of power was apparent, although the action of the battery was necessarily less; thus another wheel with similar power could have been added. The four cells we saw were stated as capable of maintaining the speed and power produced in our presence for sixty hours without renewal, at the cost of about a single stage-fare on Broadway per day. In this machine, so utterly simple as to challenge the scrutiny of the most ordinary mind, we see the dawn of a new power, capable of endless application at a minimum cost, and destitute of the usual elements of danger. It occurs to us as very strange that what is just being proposed as a possible status of facts by a learned divine in England, should prove the self-same theory which an American citizen has been privately and persistently developing in actual practice for years. To what it may give rise we have no prophet's ken to tell. If the premises demanded are proved to be correct, its application is infinite. We may yet see the Atlantic crossed by huge vessels, propelled without an ounce of coal, by a power the initiative of which the captain may place beside his writing desk in his cabin, which a child can apply, and the littlest finger may stop. The begrimed furnace-man may then come out from these lower hells and walk the deck as clean as the passenger, and the blazing fires be put out. And it may be that in the mysterious workings of the Almighty, these electric forces which are on every hand developing themselves as the life of the world, quickening its pulses from pole to pole, the cause of growth and the cardinal element of a power the limit of which is yet unknown, may be ordained to remove from man part of the curse of toil, unbending the labourer's back, and making him to stand erect as at the first.—*New York Telegraph Journal.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Zeitschrift für Chemie von Beilstein, No. 1, 1871.

This number contains the following original papers and memoirs:—

Electro-Thermic Methods of Analysis and Synthesis.—Dr E. Mulder and F. C. E. van Embden.—The contents of this paper and of the following—

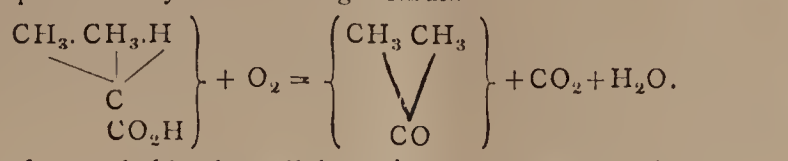
New Method of Quantitative Analysis and Synthesis—Dr. E. Mulder—Cannot be properly understood without the reproduction of the woodcuts illustrating them.

Oxidation of Isobutyric Acid.—A. Popoff.—According to the researches made by MM. Chapman, Markownikoff, and Butlerow on the oxidation of some of the oxy-acids, it appears that the law holds good that those carbon atoms are first and most readily attacked which are already partly combined with oxygen—for instance—

$$\left.\begin{matrix} RROH \\ \backslash\,|\,/ \\ C \\ CO_2H \end{matrix}\right\} + O = \left\{\begin{matrix} RR \\ \vee \\ CO \end{matrix}\right\} + CO_2 + H_2O;$$

since there exist iso-acids (more correctly, para-isoacids) of the fatty series, the constitution of which is quite analogous to the oxy-acids just alluded to, but contain H instead of HO, presumably, these acids will behave with oxidising materials in the same manner, but will also be more difficultly oxidisable, since none of the carbon atoms they contain are rendered more readily attackable by being partly already combined. This presumption has been proved correct by a direct experiment made by the author with isobutyric acid, prepared by Markownikoff's method, and treated at boiling heat with a mixture of potassium bichromate and dilute sulphuric acid; but, when isobutyric acid in excess is heated in a sealed tube to 140°—150° with an aqueous solution of chromic acid for about twenty hours, and the contents of the tube are next submitted to distillation, and the distillate

saturated with potassa, there is obtained an oily body, boiling at from 55° to 60°, and exhibiting all the properties of aceton. The reaction may be represented by the following formula :—



It is therefore probable that all iso-acids constituted as above mentioned yield, when oxidised, the keton corresponding thereto.

Contribution to our Knowledge of Chlor-Acetamide and Iod-Acetamide.—N. Menschutkin and M. Jermolajew.—This paper treats on monochloracetamide, $C_2H_2ClO.NH_2$, prepared by mixing one volume of pure chloracetic ether with two and a half volumes of concentrated ammonia. The result is the formation of a body crystallising in prismatic crystals; readily soluble in water and boiling alcohol; fusing at 119·5°; subliming without decomposition. Combining with mercury, it forms a compound, $2(C_2H_2ClO.HN).Hg$, crystallising in small acicular crystals, difficultly soluble in boiling water; this solution is not precipitated by sulphuretted hydrogen, and decomposes at 170° with great violence. Iod-acetamide is obtained by treating an alcoholic solution of chloramide (*sic*) with solid iodide of potassium; the resulting body is readily soluble in warm water, from which solution the substance crystallises in prismatically-shaped, large sized, colourless crystals.

Sulpho-Acids of Ortho-Bromtoluol.—E. Wroblevsky.—After referring to a former paper on this subject, the author treats of the salts of the α, β, and γ sulpho-acids of ortho-bromtoluol, and on a nitrited β ortho-bromtoluol meta-sulpho acid. As regards the preparation of ortho-bromtoluol, the author observes that the sulphate of bromdiazotoluol should be decomposed with absolute alcohol, whereby about two-thirds of the theoretically-required quantity of $O.C_7H_7Br$ is obtained.

Pharmaceutische Zeitschrift für Russland, No. 19, 1870.

This number contains no original papers relating to chemistry.

No. 20, 1870.

Contains an original paper on the—

Dissemination of Arsenic in Nature considered in a Forensic Chemical View.—Dr. F. L. Sonnenschein.—This memoir treats chiefly on the way arsenic may become, to a certain extent, the constituent of human bones, and be also found in the soil of cemeteries, and on the method of detecting arsenic in very small quantity in portions of the bones of human skeletons. The broken-up bones are placed in a glass tube, one end of which is sealed up, and which, for a length of 1 meter, has a diameter of 10 m.m. On the bones, perfectly pure and concentrated hydrochloric acid is poured, so that one-third of the length of the tube is left empty. The tube is next gently heated in a water-bath until the carbonic acid is driven off; and when this has taken place, the open end of the tube is sealed before the blowpipe, and next heated in a water-bath to 100° for several days, until the contents are converted into a gelatinous mass. By the sealing of the tube, any volatilisation of chloride of arsenic is prevented. When the contents are thus far dissolved, the top of the tube is cut off with a file, and chlorate of potassa added for the purpose of destroying all organic substance. The rest of the operation is performed in the usual way for the detection of arsenic.

No. 21, 1870.

Contains an original paper on the—

Testing of Essential Oil of Bitter Almonds and Essential Oil of Cloves for Adulterations.—F. A. Flückiger.—As regards the substance chiefly now used for the adulteration of essential oil of bitter almonds, this is the well-known nitrobenzol, or so-called mirbane oil. After referring to a number of tests, devised by various authors, for detecting this admixture, the author states that he employs the following method :—Dilute sulphuric acid (sp. gr., 1·11) is added to granulated zinc; and then the essential oil of bitter almonds to be tested, and the mixture, which is frequently stirred with a glass rod, is, after two hours, poured on a previously thoroughly moistened filter. The filtrate is colourless, provided the heating of the mixture is prevented, which heating is the more readily avoided, since the addition of the essential oil of bitter almonds greatly lessens the evolution of hydrogen. The salt of aniline present in the filtrate (of course, only when the oil was adulterated with nitrobenzol) may be at once detected by the addition of one of the several suitable oxidising agents; the best is chlorate of potassa, which, though not acting immediately, calls forth, even in very dilute solutions, a beautiful rose colour. This reaction is quite sure, even when only 2 grms. of the oil, adulterated with only 1 per cent of nitrobenzol, are tested. These are mixed with 10 grms. of granulated zinc, and 10 grms. of dilute sulphuric acid, and left standing for two hours, care being taken to stir the mixture frequently. As regards the essential oil of cloves, after referring to some of the more common and well-known adulterations of that substance, the author gives an account of some experiments made with the view to try whether it would be possible to detect, by chemical reagents, the adulteration of oil of cloves with carbolic acid; this is done in the following manner :—From 2 to 10 grms. of the oil are shaken up for a considerable length of time with from 50 to 100 times its bulk of hot water. After cooling, the latter is decanted carefully; and, where great accuracy is desired, the aqueous solution is concentrated by evaporation at a gentle heat. To a few c.c. of this liquid a drop of ammonia is added, and next a small pinch of good bleaching powder. If the essential oil were adulterated with even a comparatively small quantity of phenol, the liquid, after having been well stirred up, will assume first a greenish tinge, which afterwards becomes blue, and remains so for several days. Carbolic acid, dissolved in 100 parts of water, assumes, upon the addition of chloride of iron, a beautiful violet colour, which does not appear at all, or not with the proper hue, when essential oil of cloves is present.

No. 22, 1870.

Contains an original essay on the—

Different Kinds of Manna met with in Commerce.—Dr. A. Haussknecht.—This excellent essay contains an account of the various substances designated as manna, and, among these, the author also quotes the manna mentioned in the Pentateuch, and gives some valuable suggestions and information respecting that substance.

No. 23, 1870.

Contains an original essay on the—

Adulteration of some Chemical Preparations Applied in Pharmacy.—Dr. J. F. Martenson.—Among the substances analysed and named by the author we quote the following :—*Acid. phosphor. glaciale purissimum* was found to be perfectly soluble in water; but this solution became turbid by the addition of alcohol, and contained no less than 14·41 per cent of soda, equal to 47·4 per cent of phosphate of soda, and, moreover, alumina and 2·5 per cent of nitric acid. A sample of *acid. nitric. concentr. crudum* contained 1·6 per cent of hydrochloric acid and 5·5 per cent of sulphate of soda. *Tartarus emeticus pulv.* was mixed with 5·5 per cent of cream of tartar and 0·5 per cent of pulverised metallic antimony.

Purified Honey (Mel Despumatum).—Dr. E. Thorcy.—The author dissolves the white honey of commerce in its weight of water, and adds to every 2 pounds of this solution 1 drachm of carbonate of magnesia. This mixture is thoroughly shaken up, and, after an hour's rest, is filtered and evaporated to the consistency of a syrup on a water-bath. By this mode of treatment the peculiar flavour (bouquet) of the honey is preserved, and its colour is not impaired as is the case in boiling it. The author also states that direct experiments, made by him with the view to ascertain whether, by the boiling of honey in perfectly clean copper vessels, any of that metal was dissolved and imparted to the honey, prove that such is not the case, while the alteration of the colour of honey by boiling is due to the peculiar colouring matters present in that substance becoming decomposed.

NOTES AND QUERIES.

Artificial Mineral Waters.—(Reply to S. C.)—See Richardson and Watts's "Chemical Technology," vol. i., part 4, pp. 233—266.

Chlorate of Copper.—Will any of your readers be kind enough to inform me if chlorate of copper is used for fireworks, and where it may be obtained?—DELTA.

Precipitating Sulphur.—(Reply to "Chemicus.")—Refer to articles on this subject which were published in the CHEMICAL NEWS, vol. xviii., p. 157; and vol. xix., p. 234.

Colouring of Confectionery.—(Reply to "Innocuus.")—The best yellow for confectionery is saffron. Carmine of cochineal is inert. Indigo is antispasmodic, but weak. Ultramarine is probably inert. —G. A. K.

Colouring of Confectionery.—(Reply to "Innocuus.")—Saffron, anatto, and also the yellow colouring matter contained in safflower and eliminable from it by washing it with tepid water, are innocuous yellow colours; carmine of cochineal, indigo, and ultramarine are also innocuous substances.

Sewage Fungi.—Will any gentleman learned in such matters inform me as to what power is necessary for the detection and identification of sewage fungi under the microscope, and also of other low forms of life in vegetable and other extracts?—PER MARE PER TERRAM.

Manufacture of Alcohol.—Can you inform me if there is any Act of Parliament permitting manufacturing chemists to make their own alcohol from damaged grain or other substances, when it is to be used absolutely in the manufacture of other chemicals or compounds?—R. FISHER.

Testing Gold.—I would feel obliged if any of the numerous readers of the CHEMICAL NEWS could give me a test whereby I could tell the quality of some articles of jewellery, whether they were, say, 9, 12, or 15 carat. I am aware that nitric acid does not act on standard gold, but affects all qualities under that.—INQUIRER.

Mineral Oil.—(Reply to B. S.)—The change alluded to is due to action of the oxygen of the air upon some of the constituents of the oil, which, like many similar and various other organic substances, even though perfectly pure, are by this action, aided by sunlight and temperature, changed in composition, and thereby become dark coloured.

Tar Distilling.—I shall be obliged to any reader of the CHEMICAL NEWS who can suggest a safe and simple plan to prevent my tar stills boiling over. I may say that I never fill them more than two-thirds full, and, no matter how slowly or cautiously they are heated up, they at times boil over without giving the least notice. This occurs oftenest before any naphtha has been given off.—TAR DISTILLER.

Laughing Gas.—Could any of your readers give me full particulars how the liquid gas is made? Fourteen years ago I had it administered in Edinburgh; one or two teaspoonfuls poured into a bladder with a small tube in the neck which, in all cases, was most effective. I have made the dry gas, but it has not the strength, and quite a weak taste compared with what I speak of. Can it be purchased ready made, and where?—READER.

MEETINGS FOR THE WEEK.

MONDAY, 27th.—Medical, 8.
—— Royal Geographical, 8.30.
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology." (Educational Course.)
TUESDAY, 28th.—Royal Institution, 3. Dr. Foster, "On Nutrition."
—— Civil Engineers, 8.
WEDNESDAY, March 1st.—Society of Arts, 8. Mr. A. V. Newton, "On the Patent Laws and their Administration, with a view to the Adoption of Practical Amendments."
—— London Institution, 4. Prof. Huxley, LL.D., F.R.S., "On the First Principles of Biology." (Educational Course.)
—— Pharmaceutical, 8. Dr. Carpenter, F.R.S., "On the Microscope and its Revelations."
—— Microscopical, 8.
THURSDAY, 2nd.—Royal, 8.30.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— London Institution, 7.30. Prof. J. C. Thorold Rogers, "On the Colonial Question."
—— Chemical, 8.
—— Royal Society Club, 6.
FRIDAY, 3rd.—Royal Institution, 9. Capt. Noble, "On Pressure of Fired Gunpowder."
—— Geologists' Association, 8.
SATURDAY, 4th.—Royal Institution, 3. Prof. Jowett, "On Socrates."

TO CORRESPONDENTS.

ERRATUM.—In Mr. Hope's letter of last week for "*Austeckung*" read "*Ansteckung*."

T. Morgan.—1. The report of the Deputy-Master of the Mint can be obtained at Hansard's, Great Queen Street, Lincoln's-Inn Fields. 2. You will probably find the paper in the *Philosophical Transactions of the Royal Society.*

Vox.—Consult "Traité des Arts Céramiques," par A. Brongniart; or "Chefs-Dœuvres of the Industrial Arts," by Philippe Burty, published by Chapman and Hall.

W. Crossley.—Your request shall be complied with.

Cement.—Consult the article on "Cement" in Spon's "Dictionary of Engineering," No. 30, pp. 931-939; or "A Practical Treatise on Concrete, and How to Make it, with Observations on the uses of Cements, Limes, and Mortars," by H. Reid, C.E.; published by Messrs. Spon.

J. Sutherland.—We are obliged for your communication; it shall receive early attention.

Dr. A. Bauer (Vienna).—Received with thanks.

C. T. Kingzett.—You will probably find the information in some of the following books all of which are at the Library of the Commissioners of Patents:—Ure's "Dictionary of Arts, Manufactures, and Mines," 6th edition; Tomlinson's "Cyclopœdia of Useful Arts;" Dr. Muspratt's "Theoretical, Practical, and Analytical Chemistry."

Sydney Gibbons (Melbourne).—We are obliged for your letter and enclosure. Your suggestions shall receive attention.

Dr. C. A. Cameron.—We are glad to have an additional proof of the wide circulation of the CHEMICAL NEWS.

Dr. F. Hurter.—We do not know the address of the editor; the journal is published by Wilhelm Dümmler, Berlin. We shall be pleased to have the paper you name.

Domeier and Co.—Received too late for insertion this week.

Chemical Technology, or Chemistry in its Applications to the Arts and Manufactures. By THOMAS RICHARDSON and HENRY WATTS. Second Edition, illustrated with numerous Wood Engravings.

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THE CHEMICAL NEWS.

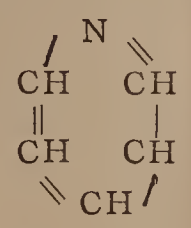
VOL. XXIII. No. 588.

ON THE

CONSTITUTION OF THE PYRIDINE AND CHINOLINE SERIES OF BASES.

By C. R. A. WRIGHT, D.Sc.,

MR. JAMES DEWAR, in a paper read before the Royal Society of Edinburgh, and reprinted in the CHEMICAL NEWS of January 27th (vol. xxiii., p. 38), considers the bases of the pyridine series to be derived from a nitrogenised benzol nucleus—



or benzol where one of the triatomic groups, CH, is replaced by nitrogen. It may be of interest to notice that other modes of representation are equally in accordance with the facts known at present.

First, this series of bases may be regarded as having for their nucleus the four-carbon benzol (unknown as yet) many of whose derivatives are well known—*e.g.*, pyrrol, tetraphenol, pyromucic alcohol, pyromucic acid, carbo-pyrrolic acid, are, respectively, the aniline, phenol, saligenin, salicylic acid, and amido-benzoic acid of the series, thus:—

Pyrrol. $CH{-}C.NH_2$; $CH{-}CH$

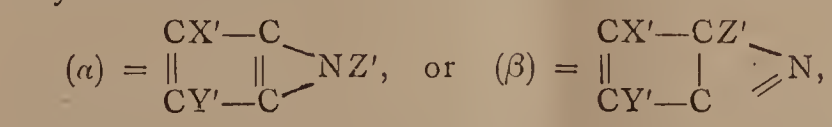
Tetraphenol. $CH{-}C.OH$; $CH{-}CH$

Pyromucic alcohol. $CH{-}C.OH$; $CH{-}C.CH_2.OH$

Pyromucic acid. $CH{-}C.OH$; $CH{-}C.CO.OH$

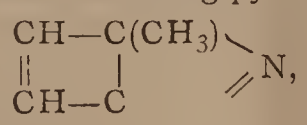
Carbo-pyrrolic acid. $CH{-}C.NH_2$; $CH{-}C.CO.OH$

Taking X, Y, Z, to represent radicals of the C_nH_{2n+1} series, the general form for bases of the pyridine series may be either—

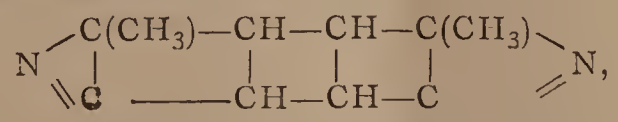


of which, perhaps, the second is the most probable. From either of these general formulæ, numerous cases of isomerism in the higher members of the series may be predicted.

The duplication of the molecule of pyridine is readily accounted for thus:—Assuming pyridine to be—

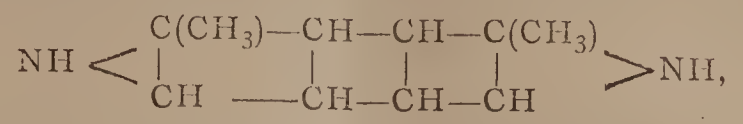


dipyridine would be—

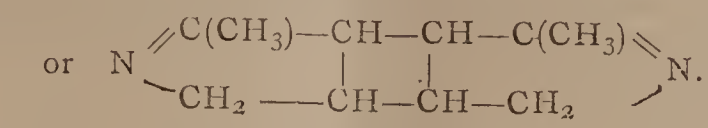


and so on for the other members of the series.

In connection with this, it is observable that the acid, $C_5H_5NO_2$, got by Hübner by the oxidation of nicotine, is either identical or isomeric with carbo-pyrrolic acid, the isomerism (if that be the case) being easily accounted for by supposing a difference in the positions of the NH_2 and CO.OH groups in the two compounds respectively; hence, on this view the following compositions might be attributed to nicotine:—



or

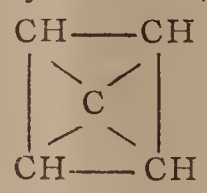


The chinoline series of bases bear to the pyridine series a relation similar to that which the derivatives of diphenyl do to the analogous derivatives of benzol, *e.g.*—

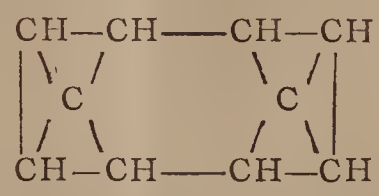
	Aniline.	Amido-diphenyl.
	$C_6H_5(NH_2)$.	$C_6H_4(C_6H_5)(NH_2)$.
	Pyridine.	Chinoline.
(α)	$C_4H(CH_3)(NH)''$.	$C_4(CH_3)(C_4H_3)(NH)''$ }
(β)	$C_4H_2(CH_3).N'''$.	$C_4H(CH_3)(C_4H_3).N'''$ }

The isomerism between the two series of bases derived from coal-tar and cinchona respectively, may be accounted for either by supposing different relative positions, &c., in the groups replacing H in the primary hydrocarbon, C_4H_4, or by assuming that one series is of the form (α) and the other of the form (β).

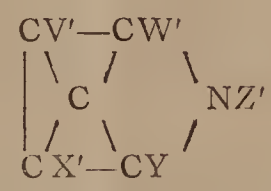
Again, the pyridine series of bases may be considered as being derived from a hydrocarbon, C_5H_4—



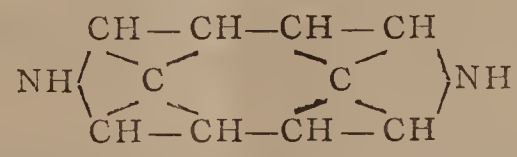
the duplication of whose formula gives rise to naphthaline—



Taking V, W, X, Y, and Z to represent radicals of the series C_nH_{2n+1}, the general formula of the pyridine series will be—



from which very numerous cases of isomerism may be predicted. In support of this hypothesis may be adduced Perkin's production of pyridine from a naphthaline derivative. The duplication of the molecule of pyridine is easily accounted for; thus, dipyridine may be—



As before, the chinoline series may be considered as derived from the pyridine series, by substitution of the four-carbon benzol residue, C_4H_3, for H.

It does not appear from Mr. Dewar's paper that the picoline employed by him was wholly freed from higher homologues, especially lutidine. Probably the dicarbo-pyridenic acid—

$$C_5H_3N\begin{cases}COOH\\COOH\end{cases}$$

obtained by this gentleman was derived from lutidine; if so, the coal-tar lutidine (α lutidine) would be a dimethylised body, probably derived from the hydrocarbon, C_5H_4; for, were it an ethyl-methyl derivative of C_4H_4, the resulting acid would be—

$$C_4HN\begin{cases}COOH\\COOH\end{cases}$$

whilst a trimethyl derivative might be expected to yield—

$$C_4.N\begin{cases}COOH\\COOH\\COOH\end{cases}$$

It is, however, possible that only two methyl-atoms are oxidised by Mr. Dewar's method of treatment, thus yielding—

$$C_4N\begin{cases}CH_3\\CO.OH\\CO.OH\end{cases}$$

Should this be the case, the different resistances of the three methyl groups to oxidation would perhaps indicate a base of the formula (α) rather than of (β); *i.e.*, the third methyl atom would be in association rather with the N, than with the C_4 residue.

It would be of interest to examine the oxidation products of the cinchona lutidine (β lutidine); for, according as an acid isomeric with dicarbo-pyridenic, or an acid $C_5H_4N.CO.OH$ should result, so it would appear that the relations of α and β lutidine are analogous to those of dimethyl-benzol and isoxylol, or to those of dimethyl-benzol and ethyl-benzol, respectively; the difference, in the first case, being simply one of relative position among the groups, replacing H in the primary hydrocarbon, and in the second being a difference in the nature and number of the radicals thus replacing H.

Yet another reason for considering C_5H_4 as the root of the pyridine series is the fact that the lowest member of the series yet obtained contains five atoms of carbon; whilst, had C_4H_4 been the nucleus, a base, C_4H_3N, lower than pyridine, should exist.

ON A REGISTERING SPECTROSCOPE.*

By WILLIAM HUGGINS, LL.D., D.C.L., F.R.S.

The short duration of the totality of the solar eclipse of December last led me to seek some method by which the positions of lines observed in the spectrum of the corona might be instantly registered without removing the eye from the instrument, so as to avoid the loss of time and fatigue to the eye of reading a micrometer-head, or the distraction of the attention and other inconveniences of an illuminated scale.

After consultation with the optician, Mr. Grubb, it seemed that this object could be satisfactorily accomplished by fixing in the eye-piece of the spectroscope a pointer, which could be moved along the spectrum by a quick-motion screw, together with some arrangement by which the position of this pointer, when brought into coincidence with a line, could be instantly registered.

I was furnished by Mr. Grubb with an instrument fulfilling these conditions, and also with a similar instrument with some modifications by Mr. Ladd, in time for the observation of the eclipse.

Unfortunately, at my station at Oran, heavy clouds at the time of totality prevented their use on the corona, but they were found so convenient for the rapid registration of spectra, that it appears probable that similar instruments might be of service for other spectrum observations.

In these instruments the small telescope of the spectroscope is fixed, and at its focus is a pointer which can be brought rapidly upon any part of the spectrum by a screw-head outside the telescope. The spectrum and pointer are viewed by a positive eye-piece which slides in front of the telescope, so that the part of the spectrum under observation can always be brought to the middle of the field of view. The arm carrying the pointer is connected by a lever with a second arm, to the end of which are attached two needles, so that these move over about 2 inches when the pointer is made to traverse the spectrum from the red to the violet. Under the extremity of the arm fitted with the needles is a frame containing a card, firmly held in it by two pins which pierce the card. This frame containing the card can be moved forward so as to bring in succession five different portions of the card under the points of the needles; on each of these portions of the card a spectrum can be registered.

The mode of using the instrument is obvious. By means of the screw-head at the side of the telescope, the pointer can be brought into coincidence with a line; a finger of the other hand is then pressed upon one of the needles at the end of the arm which traverses the card, and the position of the line is instantly recorded by a minute prick on the card. A bright line is distinguished from a dark line by pressing the finger on both needles, by which a second prick is made immediately below the other. In all cases, the position of the line is registered by the same needle, the second needle being used to denote that the line recorded is a bright one.

It was found that from ten to twelve Fraunhofer lines could be registered in about twelve seconds, and that when the same lines were recorded five times in succession on the same card, no sensible difference of position could be detected between the pricks registering the same line in the several spectra.

It is obvious that, by registering the spectra of different substances on the card, a ready method is obtained of comparing the relative positions of the lines of thin spectra.

The compound prisms employed in both spectroscopes were made by Mr. Grubb, and gave a dispersion equal to about two prisms of dense glass with a refracting angle of 60°.

PS.—I have just learned that in a spectroscope contrived by Professor Winlock for observing the eclipse of December 22nd, 1870, the positions of the observing-telescope are registered by marks made upon a plate of silvered copper. —February 3rd, 1870.

* Read before the Royal Society.

EXPERIMENTS ON THE OXIDATION OF IRON.*

By Professor F. CRACE CALVERT, Ph.D., F.R.S., &c.

Some two years since, Sir Charles Fox inquired of me if I could give him the exact composition of iron rust, viz., the oxidation found on the surface of metallic iron. I replied that it was admitted by all chemists to be the hydrate of the sesquioxide of iron, containing a trace of ammonia; to this he answered that he had read several books on the subject, in which the statements referring to it differed, and from recent observations he had made he doubted the correctness of the acknowledged composition of iron rust. He further stated that if he took a bar of rusted wrought-iron, and put it in violent vibrations, by applying at one end the fall of a hammer, scales would be separated which did not appear to him to be the substance I had described.

This conversation induced me to commence a series of experiments which I shall now detail. I first carefully analysed some specimens of iron rust, which were procured, as far as possible, from any source of contamination. Thus, one of these samples was supplied to me by Sir Charles Fox, as taken from the outside of Conway Bridge; the other secured by myself at Llangollen, North Wales. These specimens gave the following results when submitted to analysis:—

	Conway Bridge.	Llangollen.
Sesquioxide of iron	93·094	92·900
Protoxide of iron	5·810	6·177
Carbonate of iron	0·900	0·617
Silica	0·196	0·121
Ammonia	trace	trace
Carbonate of lime	—	0·295

* Read before the Manchester Literary and Philosophical Society, January 24th, 1871.

These results clearly show the correctness of Sir Charles Fox's foresight, that the composition of the rust of iron is far more complicated than is stated in our text-books. Therefore the question may be asked. Is the oxidation of iron due to the direct action of the oxygen of the atmosphere, or to the decomposition of its aqueous vapour; or does the very small quantity of carbonic acid which it contains determine or intensify the oxidation of metallic iron? To reply to it I have made a long series of experiments, extending over two years, and which I hope will throw some light on this very important question.

Perfectly cleaned blades of steel and iron having a gutta-percha mass at one end, were introduced in tubes which were placed over a mercury trough, and by a current of pure oxygen conducted to the top of the experimental tube the atmosphere was displaced, and it was then easy to introduce in these tubes traces of moisture, carbonic acid, and ammonia.

After a period of four months the blades of iron so exposed gave the following results:—

Dry oxygen	No oxidation.
Damp oxygen	In three experiments only one blade slightly oxidised.
Dry carbonic acid.. ..	No oxidation.
Damp carbonic acid ..	Slight appearance of a white precipitate of the iron found to be carbonate of iron. Two only out of six experiments did not give these results.
Dry carbonic acid and oxygen	No oxidation.
Damp oxygen and carbonic acid	Oxidation most rapid, a few hours being sufficient. The blade assumed a dark green colour, which then turned brown-ochre.
Dry oxygen and ammonia..	No oxidation.
Damp oxygen and ammonia	No oxidation.

The above results prove that, under the conditions described, pure and dry oxygen does not determine the oxidation of iron, that moist oxygen has only feeble action; dry or moist pure carbonic acid has no action, but that moist oxygen containing traces of carbonic acid acts most rapidly on iron, giving rise to protoxide of iron, then to carbonate of the same oxide, and last to a mixture of saline oxide and hydrate of the sesquioxide of iron.

These facts tend to show that carbonic acid acid is the agent which determines the oxidation of iron, and justifies me in assuming that it is the presence of carbonic acid in the atmosphere, and not its oxygen or its aqueous vapour, which determined the oxidation of iron in common air. Although this statement may be objected to at first sight, on the ground of the small amount of carbonic acid gas existing in the atmosphere, still we must bear in mind that a piece of iron, when exposed to atmospheric influences, comes in contact with large quantities of carbonic acid during twenty-four hours.

These results appeared to me so interesting that I decided to institute several series of experiments.

When perfectly clean blades of the best quality of commercial iron are placed in ordinary Manchester water they rust with great facility, but if the water is previously well boiled and deprived of oxygen and carbonic acid, they will not rust for several weeks. Again, if a blade of the same metal is half immersed in a bottle containing equal volumes of pure distilled water and oxygen, that portion dipping in the water becomes rapidly covered with the hydrate of the peroxide of iron, whilst the upper part of the blade remains for weeks unoxidised; but if a blade be placed in a mixture of carbonic acid and oxygen, a very different chemical action ensues, as not only that portion of the blade dipping in the water is rapidly attacked, but the upper part of it immediately shows the result of chemical action, and also the subsequent chemical reactions are greatly modified by the presence of the carbonic acid. For in this case that portion of the blade is only covered with a film of carbon, together with a dark deposit, composed of carbonate of the protoxide and hydrate of the sesquioxide. The fluid, instead of remaining clear, becomes turbid.

These series of experiments substantiate the interesting fact observed—that carbonic acid *promotes* oxidation.

A long series of experiments were also made to try and throw some light on the curious fact first published by Berzelius, subsequently studied by other chemists, and well known to soap and alkali manufacturers, namely, that caustic alkalies prevent the oxidation of iron; my researches can be resumed as follows:—

(1st) That the carbonates and bicarbonates of the alkalies possess the same property as their hydrates; and (2nd). That if an iron blade is half immersed in a solution of the above-mentioned carbonates, they exert such a preservative influence on that portion of the bar which is exposed to an atmosphere of common air (oxygen and carbonic acid), that it does not oxidise even after a period of two years.

Similar results were obtained with sea water to which had been added carbonates of potash and soda.

ON THE

AMOUNT OF HEAT LIBERATED BY THE ACTION OF OXYGEN ON HYDROCHLORIC ACID.

By FERDINAND HURTER, Ph.D.

IN a paper read before the British Association (Liverpool meeting), Mr. Henry Deacon stated that the reaction—viz., the action of oxygen on hydrochloric acid at a high temperature, and in presence of certain metallic compounds, was itself a source of heat, and that 10,679 units of heat are given out by the decomposition of one equivalent of hydrochloric acid.

M. Jules Thomsen, of Copenhagen,* raised the objection that this amount of heat is not liberated, because, not liquid water is formed, but steam.

This objection rests on a misunderstanding on the part of M. Thomsen, caused by a faulty translation of Mr. Deacon's paper, which appeared in the *Chemisches Centralblatt*.

Mr. Deacon authorised me to reply to M. Thomsen's objections, and I shall try to show the accuracy of Mr. Deacon's statements.

In Mr. Deacon's process, the chlorine and the water are finally obtained at a temperature of 15° C.; consequently the amount of heat liberated is the whole amount above stated, less a few units, which the water and gaseous mixture require to be heated to 15°. Mr. Deacon considered Favre and Silbermann's figures sufficiently near to represent the amount of heat liberated during the whole of the process.

Mr. Deacon was well aware that the water in the decomposing apparatus is in the form of steam, and stated distinctly that the water present absorbs heat, and thus reduces the apparent amount.

He said that the quantity actually liberated is of material assistance to make up for the loss by radiation. This has been translated as follows:—The amount of heat thus liberated *suffices* to make up for the loss of heat by radiation.

That the amount of heat liberated is of material assistance to cover the loss by radiation is proved by the following figures:—

---

* *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, No. 19.

	Units of heat.
By the combination of 1 volume O with 2 vols. H are liberated	68,924
By the decomposition of 4 vols. of HCl are absorbed	47,566
Heat set free	21,358

The reaction occurs at about 300° C.

Before the decomposition we have—

	Units.
4 vols. of HCl to be maintained at 300° C. require	4,040
1 vol. of O to be maintained at 300° C. require..	1,047
Total heat required to maintain the mixture at 300° C.	5,087

After the decomposition we have to maintain—

	Units.
2 vols. Cl at 300° C. require	2,585
2 vols. H_2O at 300° C. require	13,158
Total heat required to maintain the products at 300° C...	15,743

The difference in the amount of heat necessary to maintain the reagents and the products at 300° C. is 15,743 − 5087 = 19,656, and this amount of heat does not become apparent in the decomposing apparatus.

The heat which is actually liberated in the decomposing apparatus is 21,358 − 10,656 = 10,702 units.

This heat is sufficient to raise the temperature of the gaseous mixture, including nitrogen, 347° C. It is actually more than that which has to be imparted in order to induce the reaction. This Mr. Deacon termed of material assistance to make up for the loss of heat by radiation.

Laboratory of Gaskell, Deacon, and Co.,
Widnes.

ON THE ROLE WHICH CHALK PLAYS IN BUTYRIC FERMENTATION.*

By O. LOEW.

It is a well-known fact, that a solution of sugar, when not protected against the access of air develops gradually a smell of butyric acid and the formation of micro-organisms. Béchamp† found that one to two drops of creosote or phenol are sufficient to prevent, in 100 c.c. of sugar solution, the development of infusoria and fungi, but that such a small quantity of creosote would not be sufficient to prevent a regular fermentation by yeast. For the latter purpose, much greater quantities of creosote would be necessary.

Béchamp concludes from this observation that small quantities of creosote would kill imperfectly developed micro-organisms, but not the better developed cells of the yeast plant. A series of experiments, however, have shown me that the real cause of the above-named fact depends simply upon the relative proportions of the antiseptic substance and the quantity of the micro-organisms. The quantity of the spores falling from the air into a sugar solution is small in comparison with the number of cells in a few cubic centimetres of yeast. That a given quantity of creosote or phenol can only have a definite and limited antiseptic influence is self-evident, but, as regards these limits, few investigations have been made. I instituted a number of experiments, of which I will mention here a few results:—

(*a*). 300 c.c. of a 5 per cent grape-sugar solution were left standing in a glass-stoppered bottle, which was opened from time to time; soon the presence of micro-organisms could be detected under the microscope and a smell of butyric acid could be recognised; after a lapse of two months, the quantity of decomposed sugar amounted to 9·3 per cent of the whole.

(*b*). A similar quantity of the same solution, as in (*a*), remained entirely unaltered, after the addition of 0·5 grm. phenol.

(*c*). A mixture of 300 c.c. sugar solution of the same strength and 30 c.c. yeast (2·16 grms. dry substance) underwent a regular and complete fermentation, but, after addition of 0·5 grm. phenol, another sample (*d*) went on much slower, and, after two weeks, only 59·5 per cent of the sugar was decomposed by fermentation; on application of 1 grm. phenol, the fermentation was still more retarded, but 5 grms. stopped it entirely.

An interesting contribution to the understanding of butyric fermentation seems to have been made by Béchamp, when he announced small fungi appearing like vibrating dots, which are able to produce butyric fermentation in sugar solutions.

He calls these "microzyma cretæ." Simultaneously he said that the residue of chalk insoluble in acids (0·8 per cent) consists of a body, containing carbon and nitrogen, the real substance of the "microzyma cretæ." It seemed to me interesting to investigate this residue. 100 grms. of chalk, dissolved in hydrochloric acid, left behind 0·82 grms. residue, and this consisted of silica, oxide of iron, alumina, lime, water, and a slight trace of a humus-like substance, the quantity of which was too small to be determined. In order to obtain a clearer insight into the fermenting properties of the chalk, 50 grms. of it were mixed with 300 c.c. of a 5 per cent sugar solution and left to itself (*e*). After a few days, a development of gas bubbles could be observed, and a smell of butyric acid; after two weeks 29·3 per cent of the sugar was found to be decomposed by fermentation.

In another experiment, (*f*), 0·5 grms. phenol were added, the other circumstances being the same. A fermentation set in very soon and the 0·5 grm. seemed to have but very little influence, which agrees with the fact, described by Béchamp, but for which an entirely different explanation must be given than he gave. After two weeks 22·7 per cent of the sugar was decomposed by fermentation. On the addition, however, of 5 grms. phenol, the fermentation was entirely stopped. In a further experiment, (*g*), the chalk was heated with the sugar solution, for a length of time, to 100° C. and set aside in a well-closed flask. No fermentation set in, but, after lifting the stopper from time to time, unmistakable evidences of fermentation showed themselves, and even an addition of 0·5 grm. phenol could not prevent the process.

In the next experiment, (*h*), the chalk was at first heated to 250° C., then, after cooling, left in contact with the atmosphere for a short time, and then brought into the sugar solution, which had been previously boiled and allowed to cool in a well-closed flask; here the same fermentation made its appearance, as mentioned in (*e*) and (*f*).

It seems to me to follow, undoubtedly, from these experiments:—

(1). That Béchamp, while powdering his piece of chalk, taken from the interior of a great lump, gave admittance to the spores contained in the atmosphere, which latter produced fermentation.

(2). That the effect of the small quantity of phenol which in *absence* of the chalk would have been sufficient to prevent fermentation, was destroyed, the phenol having been absorbed by the pores of the chalk. The phenol, thus absorbed, was no more able to prevent the development of the spores falling from the atmosphere into the sugar solution.

Béchamp maintains that a small quantity of creosote would kill the spores coming from the atmosphere, but would be unable to kill the "microzyma cretæ." Further, he says, that powdered marble could not replace the chalk, the latter only containing the "microzyma." It will be

* Communicated by the author, having been read before the Lyceum of Natural History of New York.

† Jahresber., 1865.

seen very clearly, that a small quantity of phenol is easier absorbed by the porous chalk than by the powdered marble. On the other hand, leaving the creosote or phenol entirely out in both cases, the fermentation proceeds equally well, after the chalk as well as the marble had been exposed for a short time to the atmosphere.

(3). That in the presence of a lime salt, the fermentation is accelerated, when once initiated by the spores of the air, the development of the cells being much favoured by the neutralisation of the butyric acid, and by the presence of a lime-salt in solution, as butyrate of lime. Compare (*a*), (*e*), and below (*i*).

(4). That the nitrogenised matter necessary for the development of the cells is not originally present in the chalk, but is derived from the sugar, which always contains traces of it.*

In order to furnish a further proof that phenol absorbed by porous bodies has no antiseptic qualities, 50 grms. of powdered charcoal, which, for some time, had been exposed to the air, were brought into 300 c.c. of a 5 per cent sugar solution, which had been previously boiled, and was allowed to cool in a well-stoppered flask, and 0·5 grm. phenol added; after a few days, the development of gas bubbles could be observed, the phenol smell disappeared and butyric acid could be distinctly recognised by the odour; after four weeks, 23·1 per cent of the sugar was decomposed. Doubtless the presence of a small quantity of carbonate of lime would have accelerated the fermentation. In another experiment, 5 grms. phenol were employed, under similar circumstances, but then, not the slightest fermentation could be observed: it was clear the absorbing power of the coal was overbalanced.

Béchamp employed 80 grms. sugar, 1500 cc. creosote-water, and 140 grms. chalk, and obtained, after two months, 2·6 c.c. alcohol, 4·5 grms. butyric acid, 6·8 grms. acetate of soda, and 9 grms. lactate of lime. This would correspond to 40 per cent of the decomposed sugar. How is it possible for another than a neutralising *rôle* for the chalk, the common butyric fermentation being over in one to one and a-half weeks, and even, if the "microzyma cretæ" would exist, for which, however, I looked in vain. In all my experiments, I found only the common butyric ferment formed by the spores of the atmosphere.

ON THE

ALLEGED ACTION OF COLD IN RENDERING IRON AND STEEL BRITTLE.†

By J. P. JOULE, D.C.L., F.R.S., &c.

As is usual in a severe frost, we have recently heard of many severe accidents consequent upon the fracture of the tires of the wheels of railway carriages. The common-sense explanation of these accidents is, that the ground being harder than usual, the metal with which it is brought into contact is more severely tried than in ordinary circumstances. In order apparently to excuse certain Railway Companies, a pretence has been set up that iron and steel become brittle at a low temperature. This pretence, although put forth in defiance, not only of all we know of the properties of materials, but also of the experience of every-day life, has yet obtained the credence of so many people that I thought it would be useful to make the following simple experiments:—

1st. A freezing mixture of salt and snow was placed on a table. Wires of steel and of iron was stretched so that a part of them was in contact with the freezing mixture, and another part out of it. In every case I tried the wire broke outside of the mixture, showing that it was weaker at 50° F. than at about 12° F.

2nd. I took twelve darning needles of good quality, 3 inches long, 1-24th of an inch thick. The ends of these were placed against steel props, 2⅛ inches asunder. In making an experiment, a wire was fastened to the middle of a needle, the other end being attached to a spring weighing machine. This was then pulled until the needle gave way. Six of the needles, taken at random, were tried at a temperature of 55° F., and the remaining six in a freezing mixture, which brought down their temperature to 12° F. The results were as follow:—

Warm Needles.	Cold Needles.
64 oz. broke.	55 oz. broke.
65 ,, ,,	64 ,, ,,
55 ,, ,,	72 ,, ,,
62 ,, ,,	60 ,, bent.
44 ,, ,,	68 ,, broke.
60 ,, bent.	40 ,, ,,
Average 58⅓	Average 59⅝

I did not notice any perceptible difference in the perfection of elasticity in the two sets of needles. The result, as far as it goes, is in favour of the cold metal.

3rd. The above are, doubtless, decisive of the question at issue. But as it might be alleged that the violence to which a railway wheel is subjected is more akin to a blow than a steady pull, and as, moreover, the pretended brittleness is attributed more to cast-iron than any other description of the metal, I have made yet another kind of experiment. I got a quantity of cast-iron garden nails, 1¼ inches long, and ⅛th of an inch thick in the middle. These I weighed, and selected such as were nearly of the same weight. I then arranged matters so that by removing a prop I could cause the blunt edge of a steel chisel, weighted to 4 lbs. 20 zs., to fall from a given height upon the middle of the nail as it was supported from each end, $\frac{1}{10}$th of an inch asunder. In order to secure the absolute fairness of the trials the nails were taken at random, and an experiment with a cold nail was always alternated with one at the ordinary temperature. The nails to be cooled were placed in a mixture of salt and snow, from which they were removed and struck with the hammer in less than 5″.

Up to Series 10, each set of sixteen nails was made up of those of the previous set which were left unbroken, added to fresh ones to make up the number.

Series 1. Temperature of eight cold nails 10°. Of eight warm 36°. Height of fall of hammer 2 inches.
Result. No nails broke.
Series 2. Temperature of eight cold nails 14°. Of eight warm ones 36°. Fall of hammer 2½ inches.
Result. No nails broke.
Series 3. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 3 inches.
Result. One cold nail broke. No warm one broke.
Series 4. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 3½ inches.
Result. Two cold nails broke. One warm one broke.
Series 5. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 4 inches.
Result. One broke of each sort.
Series 6. Temperature of eight cold nails 0°. Of eight others 38°. Fall of hammer 4½ inches.
Result. One broke of each sort.
Series 7. Temperature of eight cold nails 2°. Of eight others 36°. Fall of hammer 5½ inches.
Result. No cold nail broke. One warm nail broke.
Series 8. Temperature of eight cold nails 2°. Of eight others 40°. Fall of hammer 6½ inches.
Result. Two cold nails broke. One warm nail broke.
Series 9. Temperature of eight cold nails 2°. Of eight others 40°. Fall of hammer 7½ inches.
Result. Three cold nails broke. Three warm nails broke.

* The fungoid growth, observed by every chemist in the solution of tartaric acid, contains in the dried mass 3·5 per cent nitrogen, besides some silica, carbon, &c., even when the acid is commonly considered as "pure."

† Read before the Manchester Literary and Philosophical Society, January 10th, 1871.

Series 10. Experiment with the ten left in the last. Temperature of five cold nails 2°. Of the five others 40°. Fall of hammer 8½ inches.

Result. Two cold nails broke. One warm nail broke.

Series 11. Experiment with the six left from the last. Temperature of three cold nails 3°. Of the other three 40°. Fall of hammer 10 inches.

Result. Two cold nails broke. Three warm nails broke.

Series 12. Experiment with fresh nails. Twelve cooled for four hours to 3°. Twelve others 41°. Fall of hammer 7 inches.

Result. Seven cold nails broke. Eight warm nails broke.

The collective result is that twenty-one cold nails broke and twenty warm ones.

The experiments of Lavoisier and Laplace, of Smeaton, of Dulong and Petit, and of Troughton, conspire in giving a less expansion by heat to steel than iron, especially if the former is in an untempered state. Such specimens of steel wire and of watch-spring as I possess expand less than iron. But this, as Sir W. Fairbairn observed to me, would in certain limits have the effect of strengthening rather than of weakening an iron wheel with a tire of steel.

The general conclusion is this—Frost does *not* make either iron (cast or wrought) or steel brittle, and that accidents arise from the neglect of the companies to submit wheels, axles, and all other parts of their rolling stock to a practical and sufficient test before using them.

ON THE ESTIMATION OF THE ACETIC ACID IN ACETATE OF LEAD.

By HENRY SEWARD, F.C.S.

Two kinds of acetate of lead are usually offered by chemical manufacturers, the one compact and very heavy, the other in loose crystalline masses; the former is more basic, and sold at a considerably lower price than the other; occasionally, also, products of intermediate composition or, perhaps, mixtures of the above are met with. For the examination of samples of acetate of lead I have used, for the last year or two, a very convenient titration process, which is conducted as follows:—

In a flask of about 12 ozs. capacity, 100 grains of the sample is dissolved in 4 ozs. of distilled water by the aid of gentle heat; litmus solution is then added, and a standard carbonate of soda solution added very gradually. It might be supposed that the presence of a precipitate would obscure the reaction, but this is not the case; one drop of the carbonate of soda solution in excess changes the colour from the purplish neutral tint of litmus to a pale blue. As in all titrations, the best results are obtained when a second determination is made, but I think that this process will be found to satisfy all requirements.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Continued from p. 91).

When we were examining the process of oxidation, I spoke to you of alcohol as a substance eminently capable of undergoing oxidation, and showed you how readily it could be burnt to a much smaller extent than that to which we are in the habit of burning it. I had to mention ordinary acetic acid as being a product of a shorter combustion. Here is a vinegar-plant which is oxidising under alcohol, and there is an intermediate body which I have not yet spoken to you about specially. Here in this, I have some of it dissolved in alcohol. It is a substance which, in the strong state in which I have it here, has rather a sickly odour, and it was named by Liebig, to whom we owe some of the first and most accurate facts in relation to it, aldehyde, a name serving to recall one of the most important facts about it, viz., that it is alcohol from which hydrogen has been taken away. If you were to take away from alcohol some of its hydrogen, you would have aldehyde,—it is alcohol minus one-third of hydrogen, and it is, therefore, alcohol de-hydrogenised, and that is the origin of the term. When wines are undergoing very slow oxidation, it appears that aldehyde and other bodies analogous to it are formed. A great deal of evidence has been adduced of this, but I ought to mention that as yet one link in the chain of evidence is wanting, which chemists are always anxious to get in proof of their conclusions, that is, the substance itself, in a pure state, has not been obtained from wine. Still, the proof is so far conclusive that we are prepared to admit it provisionally. One fact which I mentioned to you just now is very remarkable, as part of the evidence, viz., that wines which are particularly good, either by keeping or by their own composition, combine with oxygen which is dissolved in them. Now, aldehyde is particularly greedy of oxygen. If you were to dissolve in the aldehyde in this bottle some air, and if you were to try to get the air out of the alcohol again, you would find that you could get the nitrogen of the air out again completely if you went properly to work, but you could not get the oxygen out. The oxygen is laid hold of and digested so rapidly by the aldehyde that it is no longer to be recovered, after even a very short interval of time.

I might show you one case of the avidity with which this aldehyde absorbs oxygen, On putting into a glass a solution of nitrate of silver, and then adding a little ammonia, we should find, on pouring into it a little of this aldehyde dissolved in alcohol, there would be a deposit of metallic silver around the inside of the glass. This is a very common and easy way of ascertaining whether in a mixture any body of this class is present. The ammonia liberates the oxide of silver from the nitrate, and the aldehyde acts by taking away the oxygen and precipitating the silver, and in this way we get evidence of the greediness with which aldehyde takes up oxygen. There are several other interesting reactions of this aldehyde, and amongst them I ought specially to mention one which was discovered some few years ago by some very distinguished Italian chemists, the action of which is most exact and clear for removing aldehydes from any substance in which they are present; that is, their combination with alkaline bisulphites. This common aldehyde, and every body of the same class, combines with bisulphite, and forms very definite crystalline compounds, by which they are very easily detected and removed.

When we oxidise alcohol very slowly and gradually, we are able to get aldehyde formed from it; and, in the ordinary process of keeping wine, when it undergoes that slow oxidation which Pasteur affirms to be the proper process, aldehydes are proved to be present in it; but, together with them, there are a considerable number of other bodies, which we are in the habit of calling ethers. I have spoken to you already about some ethers; for instance, the compound which sulphuric acid forms with alcohol; that is a kind of ether, although it is not one of the bodies we are commonly in the habit of so describing. Ethers present a class of bodies which are certainly amongst the most pleasant of chemistry. I have a good many here; one is the commonest of all; it is the ether which is, I believe, present, to judge by the flavour at any rate, in the celebrated *Lachryma Christi*. It is a body which I might describe as a salt. It is a salt formed whenever hydric acetate, the hydrogen salt of acetic acid, is present for a sufficiently long time in alcohol. Whilst the alcohol of the wine is becoming oxidised, and whilst aldehydes are being formed from it there is also formed some acetic acid, and also probably some valerianic acid, butyric acid, and others analogous, which are formed by the oxidation of the bodies present with the alcohol. Al

* The Cantor Lectures. Delivered before the Society of Arts.

these acids, while undergoing the process by which they are formed, combine with the alcohol and bodies like it and form these ethers; and it has been already shown that, at all events, in some cases the aroma of the wine is dependent upon the presence of bodies of this kind. One of the most remarkable processes of manufacture of bodies of the kind which has been successfully performed of late, is the process of preparing artificial ethers, for the purpose of imparting to alcoholic liquids the same flavour, aroma, or bouquet which they are found to possess when made from the same natural substance; for instance, oil of brandy is got from the skins and seeds of the grape which are left when the grape-juice has been pressed out. They are fermented, and a quantity of alcohol and aromatic substances are formed by the fermentation, and this forms the so-called oil of brandy, which is used for making brandy artificially; that is, common corn spirit is flavoured with it, and sold as genuine cognac. In like manner, various kinds of these acids have been made, and there is now in Germany a manufactory for making butyric acid on a large scale from sugar; it is then made into this ether, which is a very fragrant substance, and then in small quantities it is used for flavouring various alcoholic liquids, in imitation of natural products which naturally would possess the same substance or a similar one in them.

Amongst the processes which are detrimental to the quality of wine, I have already mentioned the excess of air having access to it. That is the one which is most known, and against which people least need to be cautioned; but it has been found by wine-growers and wine-makers, especially in the case of higher-class wines, like those of Burgundy and some other districts, that they are liable to particular maladies which produce evils, each one peculiar and different from the others. Amongst these maladies, the first and simplest of all, is acetification, or the transformation of the alcohol into acetic acid. That is one which is so well known now, and so well understood, that, I think, wine-growers are well able to guard against it with tolerable completeness. By the use of a micrcscope, these little acetic cells on the surface of the liquid would at once be seen, and you would know, of course, that there would then be a tendency in the wine to pass over into acetic acid, and that, unless those cells are removed, or if they are present, unless oxygen be excluded,—because the presence of the cells does not of itself make the wine into vinegar, it is necessary that they should be present with a continuous supply of air,—so that if they are removed, or if you prevent a supply of air, that malady is arrested or cured. But there is another malady which is well known, and frequently spoken of amongst wine-growers as the "turning" of wine. It is a process which is, in its general features, something analogous to acetification, but chemically it is very different. When the wine is put into casks, it begins to give off gas, and in French they call it *la pousse*; it pushes out the ends of the casks, and, if a hole were made, the wine would be ejected with considerable force. M. Pasteur has examined not only the wine itself when undergoing this process, but also the deposit, the little solid particles which are present in it, and he has found two things which are correlative to one another,—in the first place, that there is always present in wine which is suffering from this malady certain little films, which can be seen quite distinctly by the microscope, and which are different from any particles found under any other conditions, and which he therefore believes to be little organic bodies, just as much as in the alcoholic ferment or acetic ferment, and he calls them the ferment of the turning or *la pousse*. That is one fact which is established. These little particles are compared in their structure to little bamboos, as they consist of little straight joints one at the end of the other, the length being considerably greater than the diameter; they are not little spherical matters, like the wine ferment, and their diameter is exceedingly small, being about the one-thousandth part of a millimetre. That is one fact which M. Pasteur has established, and the other is this, that while this process is going on, lactic acid is present; and his explanation of the malady consists in attributing it to the presence of this particular parasite in the wine, which is transforming the materials of it into lactic acid, with no doubt some other product, at the same time. Another malady which not frequently occurs in wine, is ropiness; and it is said that wine suffering acutely from this malady might almost be mistaken for oil, when poured from one vessel to another, so thickly does it flow. That peculiarity is attributed solely to the presence in it of a number of little films peculiar to it; but they are very different to the eye, and very different also in their functions from the films which constitute the active agency of the process I have just mentioned, that of turning. These little films are like little strings of beads, little spherical particles, a great number of them joined end to end. The particular nature of the transformation which the wine undergoes has not been investigated, but, as far as M. Pasteur's observations go, and they are very numerous and accurate, these little strings of beads are really active agents in that particular transformation which constitutes ropiness, and which destroys wine; for, if it cannot be arrested, the wine ceases to be drinkable, and becomes worthless. The fourth malady, which is also one of frequent occurrence, is one which produces a bitterness, and it is said that this malady is one to which wine is subject in its youth and also in its old age; that it sometimes occurs when wine is two or three years old, and sometimes, though in a less acute form, when it is very old. Here, also, a particular parasite is present, little organised particles, which have been minutely described and depicted, and they are found in various states, some differing very much from the others. Some pictures of the parasites which constitute the bitter ferment are like little branches with a number of little knobs or warts upon them, and some of them are clear and transparent, whilst others are coated with an incrustation. M. Pasteur, however, has already shown that the little knobs or warts upon them and the incrustation which frequently occurs, are nothing else than foreign matter deposited upon them, and when the parasite dies it is liable to be encrusted, but that in a pure state it is clear and transparent. This parasite, when it occurs in young wine, renders it completely worthless, but when it occurs in old wine, it only gives such an amount of bitterness as is not fatal to the wine, and is, to a certain extent, exceedingly common, so that it is considered almost a natural accompaniment of old wine. Amongst the remedies for these processes is sulphurous acid, which, of course, would destroy parasites when they are present. We can quite understand that wine which has got germs of these little organic beings present in it is liable to undergo these injurious changes if the germs are allowed to develop themselves, but that it would be free from all such tendency if, by any poisonous material, the little germs or organisms were destroyed. We can also understand that any mechanical process of filtration, or of forming in the liquid a gelatinous mass which will subside and carry down with it any fine particles which may be present in suspension, but which are too light to settle of themselves, would effect the same object; and it is quite intelligible, that if Pasteur's view is correct, that these little solid particles are the active agents of those transformations, the processes which have commonly been in use for preventing the detriment of wine by such changes should be effectual; we can quite see why they ought and indeed must be effectual, but, at the same time, we cannot help seeing that they would be very liable to be incomplete. Of course, it is a matter of chance whether, if you form a precipitate in the liquid, the little light particles would all happen to be caught by some of this precipitate when going down. We should expect that any such process would be efficacious in diminishing the evil, but not in arresting it completely, and I believe that is exactly what is found by experience.

(To be continued).

PROCEEDINGS OF SOCIETIES.

ROYAL IRISH ACADEMY.

THERE was a general meeting of this Society held on Monday, the 13th ult.

Mr. C. E. BURTON sent a communication "*On the Results Obtained by the Agosta Expedition to Observe the Recent Solar Eclipse.*" The other scientific papers were connected with the subject of *Eozoon Canadense.*

(1). One by Professor W. KING and T. H. ROWNEY "*On the Geological and Microscopical Structure of the Serpentine Marble of Ophite of Skye.*"

(2). Principal DAWSON, of Montreal. "*On Eozoon Canadensæ.*"

(3). Dr. H. HUNT, "*On Messrs. King and Rowney's Paper on Eozoon Canadensæ.*"

Professor W. King and Mr. Rowney, take up the position that *Eozoon canadense* is not an organism, but is simply the result of chemical and physical phenomena which produce the appearance known under that name. These two gentlemen have brought a combination of knowledge to bear upon the subject which is invaluable in connection with a disputed question.

ROYAL DUBLIN SOCIETY.

ON Monday evening, the 20th ult., there was a scientific meeting of this Society. Dr. C. King in the chair.

A discourse was delivered by Dr. ALEXANDER MACALISTER "*On Recent Advances Made in Comparative Anatomy.*" Dr. Macalister's important discoveries in connection with this science (described some time since at the Royal Irish Academy) rendered his discourse doubly interesting. His own researches are some of the most important made in this direction for some years. The subject being, however, foreign to the objects of the CHEMICAL NEWS, we must pass to the next paper, which was by Mr. JOHNSTONE STONEY, F.R.S., "*On Recent Discoveries with the Spectroscope.*" In this paper the author described one of his own investigations in connection with the absorption spectrum of chromochloric anhydride, CrOCl. This substance, which boils at 118° C., is a mobile red liquid by transmitted light, but is nearly black by reflected light. It gives off yellowish red vapours having a specific gravity of 5·48.

Mr. Stoney's interesting remarks in connection with this substance were to the effect that the yellowish-red vapour gave a spectrum—all of which were to be referred to a single series of lines—all of which were to be referred to a single motion within the molecule of the gas. The periodic time of the motion was rather more than the million-millionth of a second, and it had been possible to ascertain that the vibrations which it impressed upon the ether resembled the motion of a violin string nearly one-fifth of the length of the string from the end.

NOTICES OF BOOKS.

Chemical Phenomena of Iron Smelting; an Experimental and Practical Examination of the Circumstances which Determine the Capacity of the Blast Furnace, the Temperature of the Air, and the Proper Condition of the Materials to be Operated upon. By I. LOWTHIAN BELL. Newcastle-upon-Tyne: M. and M. W. Lambert, 50, Grey Street. 1871.

THE work before us contains, in 148 octavo pages, the first portion of an interesting and highly valuable contribution to metallurgical science and practice, from the hand of one of the most competent men to deal with this subject—the well-known ironmaster and chemical manufacturer whose name is mentioned above. In order to give our readers some idea of the bearing of this work, we quote from the first section (the introduction) the following:—

"My occupation as a pig-iron manufacturer, extending over more than twenty-five years, has afforded me many opportunities of studying the practical results produced at the blast-furnace. More recently, mere observation of the process of smelting has been supplemented by experimental research, the first fruits of which were communicated (by desire of the Chemical Society) in a paper to be found in their *Transactions* for May, 1869. Since that period, more extensive investigation has somewhat modified the opinions expressed in my communication to that body, and has added to the information previously possessed. To some extent, my views, in respect to the consumption of fuel in iron-smelting, as affected by the size of the blast-furnace and the temperature of the air, have been submitted to the criticism of the members of the Society of Mechanical Engineers and of the Iron and Steel Institute, and the results may be found in the *Proceedings* of these two Associations. In the meantime the performance of a number of furnaces of different constructions, and using different materials, has been carefully studied on the spot, a task which I have been able to perform by permission, most readily granted, of esteemed colleagues in the iron trade. The experience thus obtained opened out an extensive field of enquiry for the chemical laboratory, involving the performance of many experiments requiring great delicacy of manipulation. To assist me in these, I have had the advantage of the co-operation of Mr. C. R. A. Wright, D.Sc., who has bestowed the greatest care in carrying out my wishes in connection with a somewhat prolonged and laborious investigation, recorded in these pages under the following headings of sections:—Formation of Carbonic Oxide in the Blast-Furnace; Action of Carbonic Oxide on Iron Ore in the Blast-Furnace; Experiments to Ascertain the Temperature at which Carbonic Oxide commences to Act on Oxide of Iron; Behaviour of Oxide of Iron in contact with Carbonic Oxide at a Red Heat; Action of Carbonic Oxide on Metallic Iron; Action of Mixtures of CO and CO_2 on Fe; Action of Mixtures of CO and CO_2 on Fe_2O_3; Action of Gases of Blast-Furnace on Oxide of Iron; Reducing Effects of Gases at Different Depths of the Blast-Furnace; Dissociation of Carbonic Oxide in the Blast-Furnace; Circumstances which Limit Carbon Deposition; Carbon Deposition as effected by the Blast-Furnace; Change experienced by Metallic Iron and by Iron Oxides which have served to Dissociate CO (heat alone does not suffice for dissociating CO in the blast-furnace); Effect of certain Metallic Substances, besides Iron, on Carbonic Oxide, including Silicon; Summary of Action of CO and CO_2 on Metals and their Oxides, including Silicon; Deposited Carbon, the supposed Partial Cause of certain Apparent Anomalies in the Composition of Blast-Furnace Gases; Office Performed by Deposited Carbon; Generation and Behaviour of CO_2 in the Blast-Furnace; on Hydrogen, its Source and possible Influence in the Blast-Furnace; on Ammonia in the Blast-Furnace; on Hydrocarbons in the Blast-Furnace; on the Presence of the Metallic Bases of the Fixed Alkalies in the Gases of the Blast-Furnace; on the Presence of Alkaline Cyanides in the Blast-Furnace; 'Fume' Emitted by the Blast-Furnace; Estimate of the Quantity of Heat required in the Process of Iron Smelting."

It is evident from the above that this work is not simply intended for the metallurgist, but treats thoroughly of the chemistry of the blast-furnace, as investigated experimentally in all its bearings and relations by no less than some 515 experiments. These are described in minute detail, and are valuable also as a contribution to our knowledge of the action of bodies upon each other at high temperatures. The whole research is a pattern of good and refined methods of analysis, requiring philosophical skill in conception, as well as great dexterity of manipulation. We shall lay before our readers extracts from this work at an early date.

Year-Book of Pharmacy; comprising Abstracts of Papers relating to Pharmacy, Materia Medica, Therapeutics, and Chemistry contributed to British and Foreign Journals from July 1st, 1869, to June 30th, 1870. With the Proceedings of the British Pharmaceutical Conference at the seventh annual meeting, held at Liverpool, September, 1870. London: J. Churchill and Sons. 1870.

The title of this book indicates the contents we may expect to find in it, and we therefore welcome the volume with pleasure, because it inaugurates a new era in this country for that branch of the practice of medicine usually called pharmacy. The work is highly creditable to its authors, and contains the following matter:—A preface, extremely well written and to the point; weights and measures—most useful, complete, and carefully made up; American pharmacy; American elegant pharmacy and American recipes; English and Continental pharmacy; materia medica; chemistry—notation and nomenclature; physiological and pathological chemistry; chemical analysis—manipulation and apparatus; modern chemical theory, by Dr. Debus; notes; societies and institutions; personal notices; bibliography; unclassified memoranda; proceedings of the British Pharmaceutical Conference, Liverpool; pharmaceutical exhibition; century of old books. This last chapter is by no means the least interesting portion of this volume, and is a novelty which deserves imitation; one hundred volumes are included in this collection, those only being noticed of special interest. Space forbids us to enter here into more extensive details on the contents of this interesting and well got up work, which proves that the pharmacists of this country are becoming more and more what they should be—men of scientific education and of general learning.

CORRESPONDENCE.

JOULE AND SCORESBY'S EXPERIMENTS ON THE MECHANICAL POWERS OF ELECTRO-MAGNETISM, STEAM, AND HORSES.

To the Editor of the Chemical News.

Sir,—I was rather startled some weeks ago by the perusal, in the Chemical News of January 27th (vol. xxiii., p. 42), of the Rev. H. Highton's examination of Joule and Scoresby's experiments on the mechanical powers of electro-magnetism, &c.

The idea soon presented itself to my mind that, if the writer had thoroughly comprehended their experiments and formulæ, he could not have made such rash assertions as are found in his communication. Since then I have carefully read the account of the researches on which he comments, as given in the *Philosophical Magazine* (3), xxviii., and I hope to be able to point out where the reverend gentleman is at fault.

In the first place, he says that he cannot understand why Joule and Scoresby introduced the assumption, that the heat evolved in the circuit is directly proportional to the squares of the intensities of the currents when the engine is at rest, and when it does work, alleging that no use is afterwards made of this assumption. He then accuses these eminent physicists of having framed their formula on a perfectly different and contradictory hypothesis—viz., that the heat produced in the two cases just mentioned is directly proportional to the intensities of the currents, or the quantities of zinc consumed. If he had said, and understood, that the heat evolved per "*given quantity*" of zinc consumed was directly proportional to the intensities, he would have been quite correct, and would probably have avoided the subsequent errors into which he has fallen.

This matter, which would seem to have puzzled Mr. Highton, appears to me sufficiently simple, and to admit of easy explanation.

Let us call the heat evolved in the two cases H and H', the intensities of the currents a and b, and the quantities of zinc consumed Z and Z'. We will have, as found by Joule, $H : H' :: a^2 : b^2$, also $Z : Z' :: a : b$; therefore by division—

$$\frac{H}{Z} : \frac{H'}{Z'} :: a : b.$$

That is to say, the heat evolved per grain of zinc when the engine is at rest : the heat evolved per grain of zinc when the engine is doing work $:: a : b$. $a - b$ will therefore represent the quantity of heat per grain of zinc converted into useful mechanical power. But Joule has shown that, if the whole heat developed by the consumption of 1 grain of zinc in a Daniell's battery could be converted into useful mechanical power, it would be equal to 158 foot-lbs. We therefore obtain the value of x, on the economical duty of 1 grain of zinc, by the following proportion:—

$$a : a-b :: 158 : x$$
$$x = \frac{(a-b)\ 158}{a}$$

I trust that this will be sufficient to convince the Rev. H. Highton that the assumption $H : H' :: a^2 : b^2$, is not an irrelevant statement of M. Joule's, but is, in fact, the fundamental principle on which the formula is based.

It is evident, from inspection of this formula, that when b vanishes, or becomes infinitesimally small, x, or the economical duty, is a maximum; but Mr. Highton says that it is certainly a most startling result that the maximum of work should be done when no zinc at all is consumed.

Surely he cannot have a very clear idea of the meaning of the phrase "economical duty." When b becomes infinitesimally small, the amount of work done by the engine is certainly very small also: but x, or the amount of heat converted into mechanical power per grain of zinc, is, nevertheless, a maximum.

Your correspondent, in his criticism, goes on to say that Joule and Scoresby framed their equation on the assumption that the battery produced the same amount of heat whatever was the quantity of zinc consumed. He then remarks, very logically, that when the consumption of zinc fell from 500 to 200 grains per hour, the difference of heat corresponding to these two quantities could not disappear, because it never was produced at all. Mr. Highton should have known that $a-b$ represents the difference of heat evolved per grain of zinc, and not, as he supposed, the difference between the total amounts of heat produced by the battery in each case, which would be represented by a^2-b^2.

Having thus pointed out, I hope clearly, where Mr. Highton seems to me to have gone astray—I am, &c.,

Richard Apjohn.

South Hill, Black Rock,
February 27th, 1871.

WOOD DISTILLING.

To the Editor of the Chemical News.

Sir,—I notice, by your report of my account of wood-distilling read before the Chemical Society on the 16th inst., that I have, by a strange oversight, given an incorrect account of the gases produced. I have omitted all mention of hydrogen, I suppose, because I took it for granted that it was there, and known to be there by all chemists. Not only is it present, but it constitutes a very large part of the gas, and may almost be said to be its most important constituent.—I am, &c.,

Ernest T. Chapman.

February 27th, 1871.

HYDRATE OF CHLORAL.

To the Editor of the Chemical News.

Sir,—Whenever a new chemical agent is introduced by the medical profession, the manufacturer of the preparation must be grateful to the scientific chemist for pointing out any defect or inferiority in the quality of the article. Never, perhaps, was this more clearly the case than with the introduction of chloral hydrate; because of its decided action upon the human system demanding an almost absolute purity, and because of the astonishing rapidity with which it has found favour. This compound, discovered nearly forty years ago by Professor Liebig, remained a chemical curiosity until within the last two years. Its medicinal value was first ascertained by Dr. Liebreich; and to-day it is manufactured and sold by the hundredweight as an ordinary article of commerce. Nearly the whole, if not the whole, quantity used in this country is imported from Germany.

We happen to represent one of the largest manufacturers in Germany, Messrs. E. de Haen and Co., of Hanover, and we were much pleased on first seeing a paper on chloral hydrate reprinted in the *Pharmaceutical Journal* of the 7th ult., giving analyses of samples representing seven or eight different firms, including Messrs. De Haen and Co.; but, on reading the paper, we were much disappointed in finding that the writer, a Mr. M——, of Liverpool, had penned the article in ignorance of the matter he was treating.

He stated, as the result of his analyses, that the samples of only one house were good chloral hydrate, yielding, on decomposition by ammonia, very nearly the theoretical proportion of chloroform (72·2 per cent); whereas all the other samples were said to have yielded so little chloroform (varying from 53·6 to 56·6 per cent), that the reader was led to draw the inference that they were not chloral hydrate, but chloral alcoholate. Two of these samples were marked as De Haen and Co.'s manufacture; and, as we had no doubt about the inaccuracy of the above statement, we placed our stock at Dr. Versmann's disposal, who, from samples taken by himself, found them to be good marketable chloral hydrate, perfectly free from alcoholate. His report and analysis is published in full in the *Pharmaceutical Journal* of the 4th inst.

On applying to Mr. M—— for an apology for the injury done, he confessed that one sample named as Messrs. De Haen and Co.'s was from another house; and, as he had part of the original samples left, he suggested to send them to a chemist to act as referee, proposing, at the same time, Dr. Benjamin Paul, an offer we cheerfully accepted.

Dr. Paul's analysis, as below, proved Mr. M——'s figures to be totally wrong. As no doubt a great many of the readers of the Chemical News, have seen the paper, and may possibly have believed Mr. M——'s assertions, we feel it our duty, in justice both to the house we represent and to ourselves, not only to publicly contradict the same, but to prove them utterly groundless, and for this reason we request the favour of your inserting this letter in your journal. In conclusion, we may say—

(1). That chloral hydrate is extremely hygroscopic, which satisfactorily accounts for the somewhat smaller numbers found by Dr. Paul. The samples received by him were several months old, and were no doubt repeatedly exposed to the air, thereby taking up moisture. If kept in well-stoppered bottles, they certainly would have given the proper percentage of chloroform.

(2). That the much greater yield of the sample stated to be E. De Haen and Co.'s when analysed by Dr. Paul, and the smaller yield of the manufacture stated to be Liebreich's, in comparison to Mr. M——'s analysis, shows that the analysis of manufactures of all good makers gives about the same results; but the ammonia test for chloral hydrate, simple as it is—and, in fact, any analysis laying claim to correctness, ought only to be published after several trials of one and the same sample—carefully and scientifically made, shows the same result.

(3). That, before publicly comparing the products of respectable houses, particularly when a new article is in question, application for information ought to be made to them; the same will always most readily be given by them.

Results of Analyses of Chloral Hydrate.

		Dr. Benjamin Paul.	Mr. M——, Liverpool.
No. 1. }	Stated to be Liebreich's	66·62	71·5
No. 3. }		67·36	70·3
No. 8.	,, ,, De Haen's	64·37	55·6

—We are, &c.,

Domeier & Co.

47, Basinghall Street, London, E.C.,
February 22nd, 1871.

[We have also received from Messrs. H. and Geo. Schœttensack a letter saying that the paper by Mr. A. H. Mason is calculated to do them serious injury; they enclose the results of an analysis of their chloral hydrate by Mr. K. Müller, and a certificate of Professor F. Wöhler, which prove the purity of their preparation. We shall return to this communication shortly.—*Ed.* C. N.]

MISCELLANEOUS.

Victims of the War.—According to notes by Mr. Thomas Suttton in the *British Journal of Photography*, we learn that M. Thénard, whose name has often appeared in our pages, is now a prisoner in Germany. He was taken from his peaceful pursuits in his laboratory as a hostage, after having been twice heavily requisitioned. The young artist, M. Regnault, son of the distinguished chemist, having joined the Paris army was killed in the last sortie. He had pinned to the lining of his coat a card bearing the name and address of his *fiancée*, with a request that if he fell she might be immediately informed of the circumstance.

Chemical Society.—The following is the Council's list for the election of officers on March 30th, 1871:—President—E. Frankland, D.C.L., F.R.S. Vice-Presidents who have filled the office of President—Sir B. C. Brodie, F.R.S.; Warren De la Rue, Ph.D., F.R.S.; A. W. Hofmann, D.C.L., F.R.S.; Lyon Playfair, M.P., Ph.D., C.B., F.R.S.; A. W. Williamson, Ph.D., F.R.S.; Col. P. Yorke, F.R.S. Vice-Presidents—H. Debus, Ph.D., F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; H. M. Noad, Ph.D., F.R.S.; W. Odling, M.B., F.R.S.; T. Redwood, Ph.D.; J. Stenhouse, Ph.D., F.R.S. Secretaries—A. Vernon Harcourt, M.A., F.R.S.; W. H. Perkin, F.R.S. Foreign Secretary—H. Müller, Ph.D., F.R.S. Treasurer—F. A. Abel, F.R.S. Other Members of the Council—E. Atkinson, Ph.D.; H. Bassett; C. L. Bloxam; A. Dupré, Ph.D.; F. Field, F.R.S.; Dr. M. Holzmann; E. J. Mills, D.Sc.; ; H. McLeod; W. J. Russell, Ph.D.; H. E. Roscoe, Ph.D., F.R.S.; R. Angus Smith, Ph.D., F.R.S.; A. Voelcker, Ph.D.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

The American Journal of Science and Arts, February, 1871.

This number contains the following original papers and memoirs relating to chemistry and physical sciences:—

Observations on the Variation of the Magnetic Declination in connection with the Aurora of October 14th, 1870; with Remarks on the Physical Connection between Changes in Area of Disturbed Solar Surface and Magnetic Perturbations.—Dr. A. M. Mayer.—This paper contains, beside diagrams, a series of results of observations embodied in a tabulated form; but the contents, however valuable, are not well suited for abstraction.

Notes on Granitic Rocks.—Dr. T. Sterry Hunt.—The first portion of a monograph containing the following sections:—Definitions of granite and syenite; structure of granitic and gneissic rocks; felsites and felsite-porphyries; gneisses and granites of New England; granitic dykes and granitic vein-stones; Scheerer's theory of granitic veins; Elie de Beaumont on granites and granitic emanations; granitic distinguished from concretionary veins; Von Cotta on granitic veins; the author's views on the concretionary origin of granitic veins; the banded structure of granitic veins.

Lower Carboniferous Limestone in Ohio.—Professor E. B. Andrews.—A paper the contents of which, bearing upon palæontology, are chiefly of local interest.

Existence of the Nummulitic Formation in China.—Baron von Richtenhofen.—A contribution to our knowledge of the geology of the Chinese Empire.

Aurora Belt of October 24—25, 1870.—An account of the phenomenon alluded to as observed in a portion of the territory of the United States.

Postscript.—A loose half-page containing extracts from letters giving information respecting the observations on the eclipse of the sun December 22nd last, made in Sicily.

Polytechnisches Journal von Dingler, second number for December, 1870.

This number contains the following original papers relating to chemistry and allied subjects:—

Steam Turbine for Imparting Motion to Stirring and Grinding Apparatus intended for Chemical Purposes.—F. C. Gruncr.—The description, illustrated by a woodcut, of a simple, effective, and, moreover, cheap apparatus, which may be very useful to pharmaceutical and manufacturing chemists and large chemical laboratories.

Galvanic Battery.—W. Beek.—The description, illustrated by engravings, of an apparatus always ready for use for lecture experiments.

Photometrical Researches on a Uniform Unit of the Measurement of Light.—S. Elster.—This lengthy essay, accompanied by a series of engravings, contains the following sections:—Arrangement for fixing the normal illuminating power of the experimental candles; arrangement for fixing the maximum illuminating power of gas.

Notice on M. Steinmann's Lime-Kiln Heated with Gas.—Dr. O. von Tech.—Illustrated with engravings. It appears that, for equal weights of lime obtained, the heating of the kilns with gas, according to M. Steinmann's plan, effects a saving of fully 50 per cent of the cost of fuel, while the lime is a more valuable article, on account of its greater purity and freedom from contamination with ash.

Action of the Magnesia during the Hardening of the Lime-Alumina Silicates under Water.—Dr. C. Bender.—The main results, obtained by a series of experiments, of which the author gives an account in this memoir are, that magnesian limestones, even when so burnt as to convert the smallest possible quantity of carbonate of lime into lime, do not thereby become good hydraulic limes, while the presence of a large quantity of magnesia in lime-alumina silicates, even when burnt with care, impairs the hardening of these substances; while, if the burning of these materials is carried out to the extent of sintering (semi-fusing) the same, it appears that a portion of the lime remains uncombined with the silica, and as a consequence the mortar made with this substance crumbles to powder when under water.

Imparting of a Yellowish Hue to White Marbles.—R. Weber.—This lengthy essay contains, as chief point of interest, the hitherto unknown fact that alcoholic solutions of perchloride of iron are not precipitated by carbonate of lime, and may therefore be applied in different degrees of concentration to impart a more or less deep yellow hue to white marble.

Spectrum Analysis of Fatty Oils.—Dr. J. Müller.—A paper illustrated by a woodcut, and giving an account of the spectra exhibited by various samples of fixed or fatty oils.

Alloy suited for Soldering Steel and Iron to Brass.—Dr. E. Dingler.—Three parts of tin, 39½ of copper, and 7½ of zinc. It is stated that, by this alloy in molten state, steel and iron, as well as these and brass, can be so joined together as to prevent any breakage.

Neues Jahrbuch für Pharmacie, von Dr. F. Vorwerk, July, 1870.

This number had not been forwarded at the time of its publication; it contains the following original papers relating to chemistry:—

Officinal Phosphoric Acid, and Detection of Phosphorous Acid in the same.—Dr. Reickher.—This lengthy essay treats on the preparation of phosphoric acid by the oxidation of phosphorus by means of nitric acid, and on the detection of phosphorous acid in the resulting product. As regards the proportions of nitric acid and phosphorus, the author thinks that 14 parts of the former to 1 part of the latter are required. The reagent employed for detecting the presence of phosphorous acid is mercuric chloride (corrosive sublimate) in slight excess, and application of heat to about 80°.

Chemical Analysis of the Mineral Water of Ochsenhausen (Wurtemberg).—O. A. Biberach.—One hundred thousand grammes of this water contain the following substances, in quantities of grms.:—Silica, 1·4; carbonate of lime, 13·08; carbonate of magnesia, 4·162; chloride of potassium, 5·221; sulphate of potassa, 4·187; nitrate of potassa, 5·736; carbonate of soda, 4·212; carbonate of protoxide of iron, 0·26; semi-combined carbonic acid, 9·791; free carbonic acid, 3·675;—total of solid and volatile (gaseous) constituents, 51·726.

Java Cinchona Barks, and the Quantity of Alkaloids therein Contained.—J. Jobst.—A lengthy memoir containing a detailed pharmacologico-chemical account of researches made on a series of samples of cinchona bark imported from Java.

October, 1870.

This number contains the following original papers relating to chemistry:—

Sulphovinate of Soda.—Th. Diez.—The author prepares this salt, which is now medicinally employed, by first mixing 12 ounces of pure (free from arsenic) and concentrated sulphuric acid with 8 ounces of pure alcohol (sp. gr. 0·825 = 97·6 per cent). The acid is poured into the alcohol, so as to cause a considerable elevation of the temperature of the mixture, but without causing it to become deeply coloured. After having been left standing for several hours, water is added, and next carbonate of baryta (artificial is to be preferred). The sulphate of baryta having been removed, the solution containing sulphovinate of baryta is moderately heated, and then treated with a solution of pure carbonate of soda in water, until a slightly alkaline reaction is obtained. The solution is next filtered for the removal of the carbonate of baryta, and the filtrate evaporated upon a water-bath until a dry powder is obtained. The sulphovinate of soda—

$$(2SO_3,C_4H_5O,NaO+HO),$$

is capable of crystallisation, but the salt is extremely deliquescent. Genuine sulphovinate of soda, free from any carbonate of soda, is not acted upon by baryta solutions unless heat be applied. If the dry sulphovinate of soda is mixed with dry acetate of potassa, and the mixture heated in a test-tube, acetic ether is given off, which reaction is a characteristic test for the sulphovinate. This on being taken internally, becomes decomposed by the action of the acid in the stomach, and converted slowly into alcohol and sulphate of soda, thus yielding a laxative medicine with a counter-agent to its too great debilitating action.

November and December (double number), 1870.

Contains the following original papers relating to chemistry:—

Materials Suitable for the Manufacture of Cements.—Dr. C. Bender.—The continuation and conclusion of a very lengthy memoir on this subject.

Hydrochloric Acid as the Basis of Alkalimetry.—Dr. Græger.—Reserved for full translation.

Soup Tablets.—H. Reinsch.—Take 11 parts, by weight, of good suet, melt it in an iron pan, and make it very hot (brown it); add, while keeping the fat stirred, 18 parts of rye meal, and continue heating so as to make the mass (constantly stirring it) brown; add then 4 parts of dried salt and 2 parts of coarsely-pulverised carraway. The mixture is then poured into tins somewhat like those used for making chocolate into cakes. The cakes have the appearance of chocolate, and are chiefly intended for the use of soldiers while in the field. A quantity of about 33·5 grms. of this preparation is sufficient to yield, when boiled with some water, a portion of good soup, and, in case of need, the cakes, being agreeable to the taste, may be eaten raw.

Chemical Composition of Edible Mushrooms.—Dr. O. Siegel.—*Boletus edulis, L.*, contains, in fresh state—Water, 15·42 per cent; dry matter, 84·52 per cent; 100 parts of which consist of—Proteine compounds, 22·82; mannite, 5·84; fat, 1·98; extractive matter, 57·29; woody fibre, 6·55; ash, 6·22, containing, in 100 parts, 20·12 of phosphoric acid and 50·95 of potassa. *Cantharellus cibarius* contains in fresh state, 92·08 per cent of water, and when air dry, 16·48 per cent. 100 parts of the dry substance contain—Proteine compounds, 23·43; fat, 1·38; extractive matter, 46·85; woody fibre, 9·47; ash, 8·19, containing, in 100 parts, 31·32 of phosphoric acid and 48·75 of potassa, *Clavaria flava* contains, in fresh state, 21·43 per cent of water. 100 parts of the dry substance contain—Proteine compounds, 24·43; mannite, 7·81; fat, 2·13; extractive matter, 48·94; woody fibre, 6·94; ash, 9·75, containing, in 100 parts, 35·07 of phosphoric acid and 51·47 of potassa. *Morchella esculenta* contains, in fresh state, 15·82 of water. 100 parts of the dry substance contain—Proteine compounds, 33·90; mannite, 7·48; fat, 1·71; extractive matter, 40·59; woody fibre, 6·58; ash, 9·74, containing, in 100 parts, 37·75 of phosphoric acid and 50·04 of potassa. *Tuber cibarium* contains, in fresh state, 70·83 per cent of water. 100 parts of the dry substance contain—Proteine compounds, 36·32; fat, 2·48; extractive matter, 23·16; woody fibre, 28·31; ash, 9·73, containing, in 100 parts, 30·85 of phosphoric acid and 55·97 of potassa.

Annalen der Physik und Chemie, von Poggendorff, No. 12, 1870.

This number contains the following original papers and essays:—

Elasticity of Iron, Copper, and Brass, especially in reference to Temperature.—F. Kohlrausch and F. E. Loomis.—This lengthy essay, illustrated by woodcuts and engravings, contains the following sections:—Description of apparatus and mode of observation; reduction of observations; calculation of the co-efficients of temperature; temperature and modulus of elasticity of iron, copper, and brass; absolute moduli of elasticity of iron, copper, and brass wires, as determined by the aid of torsion oscillations and velocity of sound.

Relation Existing between Meteorites and Stones (Rocks) met with on our Globe.—Dr. C. Rammelsberg.—A review of a paper published in the *Comptes Rendus*, vol. lxii. (1866), by M. Daubrée, under the title "Expériences Synthétiques Relatives aux Météorites; Rapprochements aux quels ces Experiences Conduisent, taut pour la Formation de ces Corps que pour celle du Globe Terrestre."

Olivine Rock occurring at Dreiser Weiher (a Mountain of the Eifel Range, Prussia).—Dr. C. Rammelsberg.—This extensive monograph contains a comparative description of a series of minerals —viz., olivine, broncite, diopside, chrome-iron ore. The olivine rock alluded to consists of 68·50 per cent of matter soluble in hydrochloric acid, consisting of—Silica, 27·41; magnesia, 34·24; protoxide of iron, 6·85; and of 29·69 of matter insoluble in hydrochloric acid, consisting of—Silica, 15·57; alumina, 1·74; magnesia, 8·35; lime, 2·29; protoxide of iron, 1·74; and a trace of chromium.

New Sulpho-Salts.—R. Schneider.—The fifth portion of a lengthy essay on this subject. Sodium-sulpho palladate and disulphide of palladium, Na_2S,PdS_2 (equivalent=248·6), contains, in 100 parts—Na_2, 18·50; Pd, 42·88; S_3, 38·62. Silver-sulpho palladate, Ag_2PdS_3 (equivalent=418·6), contains, in 100 parts—Ag_2, 51·60; Pd, 25·46; S_3, 22·94. Potassium-palladium-sulpho palladate and semi-sulphide of palladium, $K_2Pd_3S_4$ (equivalent = 526·06), in 100 parts—K_2, 14·87; Pd_3, 60·80; S_4, 24·33.

Determination of the Ratio of the Specific Heat of the Air.—Dr. C. W. Röntgen.—An excellent monograph on this subject, illustrated with engravings, but not suited for useful abstraction.

Measurement of the Intensity of Sound.—A. Heller.—An algebraico-physical treatise illustrated by engravings.

Spectrum of the Aurora Borealis.—F. Zöllner.—Illustrated by woodcuts.

Deduction of the Fundamental Equations of Capillarity from the Principle of Virtual Velocities.—L. Boltzmann.

Observations on the Paper published by MM. Bettendorff and vom Rath "On the Combinations of Sulphur with Selenium."—B. Rathke.

Fusion of Leaden Balls and Leaden Shot when Coming into Contact with an Iron Target.—J. Bodynski.—This paper contains a critical review on a paper published on the above-named subject by Dr. E. Hagenbach.

Review on a Paper published by Dr. Feddersen in vol. cxxxix., p. 639, of the above-named periodical.—K. W. Knockenhauer.—Since the number of this periodical above quoted belongs to vol. cxli., and since the title of Dr. Feddersen's paper is not given, it is scarcely possible to make any abstract of this memoir.

Appendix to a Treatise on the Minimum Deviation of a Ray of Light through Symmetrically-Arranged Prisms.—R. Most.

Apparatus for Analytico-Synthetic Composition of Mixed Colours.—E. Ketteler.—Illustrated with woodcuts.

Observations on Molecular-Statistical Aphorism, as set forth by M. Lüdtge.—Dr. G. van der Mensbrugge.

An Experiment with Small Shot.—E. Reusch.—This paper, illustrated with a woodcut, contains a discussion on the forces at play when a small pair of forceps holds 2 grains of small shot.

Observations on the Researches of Dr. Andrews on the Compressibility of Carbonic Acid.—D. Mendelejeff.

NOTES AND QUERIES.

Tattooed Marks.—Is it possible to erase them? If so, what are the safest means for erasing them from the hand, without injury to it afterwards?—Scotia.

Testing Gold.—(Reply to "Inquirer.")—Obtain a touch-stone and touch-ncedles, as they are called, being small pieces of gold variously alloyed, and used to compare, on the stone, the streak they make with that given by articles of jewellery, while at the same time a nitric acid of proper strength is used to detect baser metals.

Tar Distilling.—(Reply to "Tar Distiller.")—It appears from your account that you work from the first with fire, instead of by heating with coils of pipes inside the still, through which high-pressure steam is made to pass. The boiling over is due, in all likelihood, to the mode of setting the stills in the furnace, whereby too much heat is imparted to the bottom of the still, and a sudden formation of vapours there causes the thickish fluid to boil over.

Manufacture of Alcohol.—Reply to R. Fisher.)—You may not manufacture alcohol from malt grain, nor distil, unless you take ou tthe nccessary licenses and place your premises under the constant supervision of the Excise and Inland Revcnue officers; for any infringement of these regulations you will become liable to heavy fines, confiscation of apparatus, and even imprisonment.

Lute for Corks, &c.—It may be of interest to some of your readers to know that anthracene acts capitally as a substitute for paraffin (either by itself, or mixed with the latter) in covering corks or joints of apparatus requiring to stand a comparatively high temperature. A luting of anthracene is capable of standing a high pressure and temperature combined for a lengthened period.—Robert F. Smith.

Chemical Agriculture and Manufacture of Sugar.—(Reply to J. T.)—"The Nature and Properties of the Sugar Cane, with Practical Directions for the Improvement of its Culture and the Manufacture of ts Products," by George R. Porter, F.R.S.; second edition; London: Smith, Elder, and Co.; 1843. "The Practical Sugar Planter, a Complete Account of the Cultivation and Manufacture of the Sugar Cane, &c.," by L. Wray; London: Smith, Elder, and Co.; 1848 (2 vols.). "Lectures on Agricultural Chemistry and Geology," by James F. W. Johnston, M.A., &c.; second edition; W. Blackwood and Sons, Edinburgh and London. "Principles of Agriculture," by Albert D. Thaër; translated by W. Shaw; 2 vols.; London: Ridgway; 1844. "Traité des Arts Céramiques," par A. Brongniart; 2 vols. and atlas, with plates; Paris; 1844 (Béchet Jeune, editeur). "Chefs-Dœuvres of the Industrial Arts," by Phillippe Burty; edited by W. Chaffers; London: Chapman and Hall; 1869.

MEETINGS FOR THE WEEK.

Monday, March 6th.—Medical, 8.
—— Royal Institution, 2. General Monthly Meeting.
—— London Institution, 4. Mr. R. A. Proctor, M.A., F.R.A.S., "On Astronomy."
Tuesday, 7th.—Royal Institution, 3. Dr. Foster, "On Nutrition."
—— Civil Engineers, 8.
—— Ethnological, 8.
—— Zoological, 8.
Wednesday, 8th.—Society of Arts, 8.
—— Geological, 8.
Thursday, 9th.—Royal, 8.30.
—— Royal Institution, 9. Dr. Odling, "On Davy's Discoveries."
—— London Institution, 7.30. Prof. J. C. Thorold Rogers, "On the Colonial Question."
—— Royal Society Club, 6.
Friday, 10th.—Royal Institution, 9. Dr. W. B. Carpenter, F.R.S., &c., "On the latest Scientific Researches in the Mediterranean and Straits of Gibraltar."
—— Quekett Microscopical Club, 8.
Saturday, 11th.—Royal Institution, 3. Mr O'Neil, "On the Spirit of the Age."

TO CORRESPONDENTS.

C. R. C. Tichborne.—We shall feel obliged if you will continue to send the journal to the same address.

J. Wallace Young.—The paper arrived too late for insertion this week.

R. Apjohn.—Received.

A. H. Allen.—Received with thanks.

X. Y. S.—A few copies of the "Cattle Plague Report," by the Editor, still remain on hand; one will be forwarded on your sending thirteen stamps to our publisher.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 589.

ON THE EFFECT OF COLD ON THE STRENGTH OF IRON.*

By PETER SPENCE, F.C.S., &c.

In the conversation at the last meeting of the Society on one of the causes of railway accidents, namely, the breaking of the tires of the carriage wheels, there was a general expression of opinion that the reduction of temperature during frost had the effect of reducing the strength of iron, and that this was the proximate cause of these occurrences. Dr. Joule, on the other side, stated that, however general the impression might be, he knew of no experiments that tended to prove that impression to be a correct one.

It seemed to me that a few experiments on cast-iron could without much difficulty be made, and which might set the matter at rest one way or the other.

I therefore decided on having some lengths of cast-iron made of a uniform thickness of ½ inch square, from the same metal and the same mould; these I obtained after a good deal of trouble, on account of the moulders being off work at new year's time, and this must be my excuse for not being able to give due notice of my communication.

Two of the four castings I got seemed to be good ones, and I got the surface taken off, and made them as regular a thickness as was practicable.

I then fixed two knife-edged wedges upon the surface of a plank, at exactly nine inches distance from each other, with an opening in the plank in the intervening space; the bar being laid across the wedges a knife-edged hook was hung in the middle of the suspended piece of the bar; to the hook was hung a large scale on which to place weights.

The bar was first tried at a temperature of 60° F.; to find the breaking weight I placed 56-lb. weights one after another on the scale, and when the ninth was put on the bar snapped. This was the only unsatisfactory experiment, as 14 or 28 lbs. might have done it, but I include it among the others. I now adopted another precaution, by placing the one end of the plank on a fixed point and the other end on to a screw-jack, by raising which I could, without any vibration, bring the weight to bear upon the bar. By this means, small weights, up to 7 lbs. could be put on while hanging, but when these had to be taken off and a large weight put on, the scale was lowered to the rest, and again raised after the change was made. I may here state that a curious circumstance occurred twice, which seems to indicate that mere raising of the weight, without the slightest apparent vibration, was equal in effect to an additional weight. 3¾ cwts. were on the scale, a 14-lb. weight was added, then 7 lb., then 4 lbs., 2 lbs., 1 lb., making 4 cwts. and 1 lb. This was allowed to act for from one to two minutes, and then lowered to take off the small weights, and replaced by a 56 lb., intending to add small weights when suspended, raised so imperceptibly by the screw, that the only way of ascertaining that it was suspended was by looking under the scale to see that it was clear of the rest. As soon as it was half an inch clear it snapped, thus breaking at once with one pound less than it resisted for nearly two minutes.

Six experiments were carefully conducted at 60° F. the parts of the bars being selected so as to give to each set of experiments similar portions of both bars: the results are marked on the pieces. My assistant now prepared a refrigerating mixture which stood at zero, and the bars were immersed for some time in this, and we prepared for the breaking trials to be made as quickly as could be, consistently with accuracy; and to secure the low temperature each bar on being placed in the machine had its surface at top covered with the freezing mixture. The bars at zero broke with more regularity than at 60°, but instead of the results confirming the general impression as to cold rendering iron more brittle, they are calculated to substantiate an exactly opposite idea, namely, that reduction of temperature *cæteris paribus* increases the strength of cast-iron. The only doubtful experiment of the whole twelve is the first, and as it stands much the highest, the probability is that it should be lower; yet, even taking it as it stands, the average of the six experiments at 60° F., gives 4 cwts. 4 lbs. as the breaking weight of the bar at that temperature, while the average of the six experiments at zero gives 4 cwts. 20 lbs. as the breaking weight of the bar at zero, being an increase of strength from the reduction of temperature equal to 3·5 per cent.

In the discussion which followed the reading of this paper Mr. W. Radford, C.E., asked if Mr. Brockbank had considered what the effect must be upon iron used in Russia, Sweden, Norway, and Denmark, for if the theory which was sought to be established were true, the tires of railway wheels in those countries must fly to pieces in winter; as far as his experience went in Denmark such had not been the case on the Copenhagen Railway.

* Read before the Manchester Literary and Philosophical Society.

ON THE PHOSPHATE PROCESS FOR THE UTILISATION OF SEWAGE,

AS APPLIED TO SEWAGE IRRIGATION AND TO THE PURIFICATION OF SEWAGE BY PRECIPITATION.

By DAVID FORBES, F.R.S., &c.

The subject of the disposal of the sewage of towns is now recognised as one of the great sanitary problems of the age, and admitted to be of vital importance, not only to the welfare of the inhabitants, but also to the country at large. The very difficulties, however, which are connected with its consideration have apparently been the principal reasons why, until comparatively lately, this subject has been so little studied, and if not altogether neglected, at least treated in a sort of makeshift way, until the evil effects of such indifference have made themselves felt to such an extent that its mature consideration can no longer be evaded.

If the subject of sewage utilisation be considered merely from a purely agricultural point of view, it is self-evident that the direct application to the soil of what may be called the original unadulterated excreta, both solid and liquid, must be the most advantageous of all systems for their disposal and utilisation.

In towns, however, and more particularly in large ones, the imperative necessity for the instant removal of matter which every moment is becoming more and more offensive and injurious in a sanitary point of view, combined with the difficulty and inconvenience arising from its direct collection and conveyance to the land, are so great as to have resulted in the all but universal adoption of the closet system, in which water is employed as a medium for its transport into and along the sewers, to such a distance as may be considered beyond the limits within which its further decomposition could give rise to discomfort and disease.

When, however, this had been accomplished, the question still remained to be answered, as to what must ultimately be done with the excreta thus enormously diluted, and consequently immensely reduced in agricultural

value; the more so from their having received, in their transit so far, the addition of the rainfall and surface-dirt, and the admixture of the waste products of manufactories, which in themselves are not only valueless, but too often even highly injurious to either animal or vegetable life.

The ancient time-honoured, but barbarous system, unfortunately too often followed, even in the present day—that of turning the filthy sewage into the nearest watercourse or river—can no longer be tolerated in these days of advanced civilisation, when we have at length become convinced that by these means our streams have in many instances already become so polluted, that they can no longer furnish water fit for either man or beast to drink, or fishes to live in, besides having our river beds silted up by deposits of fœtid mud, and where, in the more thickly populated districts, we witness rivers which we remember to have once been clear and pure, now actually converted into so many open sewers, exposing a large surface of putrid liquid to taint the surrounding atmosphere and impair the health of those dwelling in the vicinity; whilst, at the same time, they carry down with them into the sea whatever agricultural value is represented by the excreta, to the loss of the country, which must pay for foreign guano and other fertilising substances to replace it.

It is no wonder, therefore, that attention has of late years been directed to the devising of means for effecting the purification of sewage, at least to such a degree that the affluent water may, without injury or offence, be allowed to mix itself with the natural drainage water of the land. Up to the present time, however, no one of the various systems as yet submitted to a practical test, can be said to have given satisfactory results; the experience acquired already has, however, clearly proved that no merely mechanical system of filtration can ever be expected to succeed, and shown that it is absolutely essential that chemical forces also be brought into play in order to effect the desired purification.

The various modes hitherto adopted for sewage purification, which depend directly or indirectly upon the co-operation of chemical action, may be classed under the two heads of (1) purification by means of irrigation, and (2) purification by the direct chemical treatment of the sewage. Although these two systems have hitherto been too often regarded as altogether antagonistic to one another, the results of the latest inquiries into the subject clearly show that this need not in reality be the case, and indicate that a combination of both these systems promises, in practice, advantages which neither of them, taken singly, can be expected to attain.

The phosphate process for the purification and utilisation of sewage, first brought before the public a few months back, at the Liverpool meeting of the British Association, and which is, it is believed, the first attempt hitherto made to devise a process equally applicable to either sewage irrigation or precipitation, consists in treating the sewage with a solution of the native phosphate of alumina dissolved in sulphuric or hydrochloric acid. This solution is in itself a powerful antiseptic and disinfectant, completely arresting further putrefaction, and depriving the most fœtid sewage of its offensive smell, causing, at the same time, the supernatant water to be clear and colourless, even if tinctorial substances of great intensity be present in the liquid, effects which are mainly due to chemical reactions between the alumina salts and the organic matter in the sewage, by which compounds are formed, especially with the nitrogenous or albumenoid constituents, more or less insoluble in water—a property which has long been known and utilised in the arts of dyeing, the manufacture of pigments and sugar, and the clarification of liquids.

The experiments to illustrate this method for purifying sewage, shown upon a small scale at the Liverpool meeting of the British Association, have since been repeated on the large scale at Tottenham and elsewhere, with the most satisfactory results, and thus enable me to speak with more confidence as to the applicability of this process when employed in connection with either of the two systems for utilising the sewage of towns in general.

I. Sewage Irrigation.

Sewage irrigation is a good example of a true mechanico-chemical system of treatment, since by its employment the soil itself acts mechanically as a filter, whilst the oxidising action of the air and the growth of vegetation bring powerful chemical actions into operation, to decompose and assimilate the organic and other compounds in the sewage which may be valuable as fertilising ingredients.

There can be no question whatever but, that when the local circumstances of climate and soil are favourable to irrigation and the conditions essential to its success properly observed, that sewage irrigation is the most natural and effective system for the utilisation of sewage, since it is only by this means that we can render available the whole of the ammoniacal salts upon which so very much of the fertilising value of sewage depends.

Supposing, however, that sewage irrigation is employed, even under the most favourable local and other circumstances, there is still one feature in the present mode of carrying it out in practice which will no doubt be admitted by any impartial person conversant with the subject to be highly objectionable, and which, no doubt, also, is one of the principal reasons why sewage farming has not, in practice, met with that favour or success which might have been expected from its theoretical consideration. This objection is, that the suspended and most offensive solid matters in the sewage, when pumped up and distributed over the surface of the land along with the liquid, cannot, like the latter, sink into the soil to contribute its share in the development of the plants; but, on the contrary, in great part covers and stops up the pores of their leaves with a slimy film of fœtid matter, which cannot but tend to impede, if not also injure, the growth of vegetation, and, by its further decomposition, to contaminate more or less the surrounding atmosphere. This solid sewage matter never comes at all within reach of the roots of the plants which could assimilate it, and consequently its agricultural value may be put down as lost; for even if it does not do an absolute injury to vegetation, we imagine that no farmer would consider it profitable to put such manure on the top of his growing crops.

On this subject, we have the testimony of Professor Voelcker, who, in the *Journal of the Royal Agricultural Society of England*, vol. vi., states—

"All who are practically acquainted with sewage irrigation are fully aware of the obstacles which the suspended matter in raw sewage interposes to successful irrigation. Clarification, by any good precipitating plan, whilst it only removes a small portion of fertilising matter from the sewage, greatly improves its suitability for irrigating purposes. Experimental sewage farmers, I believe, will bear me out in maintaining that sewage, perfectly clarified, has a greater practical value than it has in the raw state with all the suspended filth in it. . . . Whenever practicable, therefore, clarified and disinfected sewage should not be wasted, but employed for irrigating our fields."

The fact that sewage, notwithstanding that it is comparatively rich in ammoniacal salts, is extremely poor in phosphates, so essential to the growth and cultivation of many of the most important crops, renders sewage irrigation less suited for general farming and restricts its operations to market gardening and the cultivation of green crops such as Italian rye-grass, &c., for immediate consumption.

There is, therefore, every reason for believing that a great improvement would be effected in the present system of sewage irrigation if measures were taken (1) to separate at least the greater part of the suspended substances; (2) to disinfect, or to some degree arrest, the offensive decomposition until the sewage can be absorbed by the soil and thus conveyed to the roots of the plants; and (3) to increase the manurial value of the sewage by the introduction of phosphates into the sewage in such a form as to be at once taken up by the crops.

By the application of the phosphate process these three conditions can be at once secured in the most simple and efficacious manner, as has already been proved on the large scale at the Tottenham Sewage Works, where it was only found necessary to let the required quantity of the phosphate solution flow into the tanks in which the sewage is received at the pumping station, to arrest the decomposition and effect a speedy clarification caused by the immediate precipitation of the matters held in suspension in the sewage as a deposit which, although of no high agricultural value, may still be used with advantage as a solid manure after the supernatant clarified water has been drawn or pumped off and applied for irrigating purposes for which it is now much better suited, owing to the amount of soluble phosphates which it now contains in addition to its other fertilising ingredients.

By this process, therefore, which might, if required, be even carried out in the sewers themselves (for, in point of fact, the phosphate solution may even be employed instead of the more expensive chlorides of soda or lime, permanganate solution, carbolic acid, chloralum, or other disinfectant), an immediate partial purification of the sewage is at once effected, previous to sending the sewage on to the land, thus doing away with its offensive smell, whilst, at the same time, by adding more or less of the phosphate solution, the agricultural value of the irrigating water may be increased to any desired extent, and thus grain or other crops quite unsuited to sewage irrigation, as at present practised, may be grown without any other manure than the sewage water so treated; the supply of phosphates necessary for the growth of the plants being under perfect and immediate control according to the requirements of the crop and the nature of the soil. In other words, this process is the first attempt made to strengthen the manurial properties of the otherwise miserably weak sewage water, and, at the same time, to so far effect its purification that it can be employed without offence—a feature which distinguishes this system of treating sewage from all other attempts to deodorise or disinfect it which depend upon the addition of substances which, if they do not lower the value of the sewage for agricultural purposes, do certainly not augment it.

II. Sewage Precipitation.

Although, as before stated, sewage irrigation, in all cases where the local circumstances are favourable for its employment, must be regarded as the most likely method of purifying the sewage, and, at the same time, obtaining the greatest agricultural value out of a given quantity of sewage water, there are, nevertheless, many instances in which the application of this system is altogether impracticable or not attended with corresponding advantages. In the first place, the outlay for machinery and appliances for collecting, pumping, and distributing the sewage in towns, may often be so enormous as to deter the ratepayers whose shoulders have to bear the burden, from adopting so expensive a system, especially when they remember that the area of land required to be secured for utilising by irrigation the entire sewage of even a moderately sized town is extremly large as well as most costly to purchase, owing to the price of land in the vicinity of towns in these days of cheap railway communication being, as is well known, out of all proportion to its mere agricultural value.

In many places, also, the soil is a stiff, non-absorbent clay, which, instead of allowing the sewage to sink into it allows it merely to flow off its surface into the watercourses without undergoing any purification; in other places, well-drained, porous, and absorbent land, such as is alone suitable for sewage irrigation, is not to be had at all, or only at such great distances as still further to augment the outlay required in the first instance.

When considering this subject it must never be forgot that the purification effected by sewage irrigation is mainly dependent on the chemical forces brought into activity by the growth of vegetation, and which, consequently, is more and more powerful in proportion as the vegetation itself is more luxuriant and the reverse; so that in winter, when in our climate vegetation continues in a torpid and almost dormant state, it naturally follows that the power of assimilating, which, in other words, means purifying and extracting the agricultural value out of the sewage water, is reduced to its minimum at a time when, owing to the extra rainfall at this season of the year, we are, nevertheless, obliged to continue drenching the land already, perhaps, supersaturated with moisture, with even far more sewage water than is supplied to it at other times of the year when the plant most requires it, and thus, by keeping its roots continually wet and cold, we are supposed to injure its health,—an opinion which most farmers who know the beneficial effects of good draining will probably endorse. At the same time it would be but deceiving ourselves to suppose that land thus irrigated in winter could retain or store up until spring other than a very small portion of the more valuable fertilising ingredients in the sewage water, such as the ammoniacal salts, nitrates, &c., for these substances being of an extremely soluble nature, must be in greater part washed out and carried off with the natural drainage-water of the land in the event of the plants themselves not being in a condition to arrest and assimilate them.

No one, therefore, who takes all these circumstances into due consideration can fail to be convinced of the great desirability and importance of having at command some system by which at certain seasons of the year or in localities unsuited for irrigation, the sewage may be at once purified by precipitation to such an extent that the effluent water may be run directly into the sea or nearest river: with this object, therefore, a modification of the phosphate process, already described, has been devised, which, after numerous trials on a working scale at the Tottenham sewage works and elsewhere, has given the most satisfactory results.

The process itself may be described in a few words, since it is of the simplest nature and requires no machinery or special appliances beyond the tanks or reservoirs. A suitable quantity of the solution of the native phosphate of alumina in sulphuric or hydrochloric acid is allowed to run along with, and so mix itself with, the sewage as it enters the tanks, precisely as when it is used in clarifying the sewage employed for irrigation as before described. Instead, however, of allowing the clarification to proceed and the suspended matter in the sewage to settle down, a quantity of milk of lime, just sufficient to neutralise the acid in the phosphate solution, is immediately afterwards also run into the tank for the purpose of precipitating the soluble phosphates thus added to the sewage, and by so doing effect a still further purification of the water.

The precipitated matters subside rapidly, and the supernatant water is at once seen to become colourless, clear, and soon loses any offensive odour or taste. The time required for the complete purification varies considerably with the quality and nature of the sewage, rarely requiring more than from three to eight hours, after which the supernatant water may be drawn off from the deposit (preferably by an arrangement which commences drawing or pumping it off first, from the surface downwards) and can be discharged directly into the neighbouring stream without destroying the fish or otherwise polluting them to any sensible extent.

Effluent Water.—With regard to the effluent water from this process, it is not pretended that it attains any such high degree of purity as to admit of comparison otherwise than in appearance with the water supplied for the water works, and it should be recollected that the object of the process is to purify sewage, not to manufacture drinking water out of it. It has, however, been already demonstrated by experiment, that sewage from London, Liverpool, Swindon, Birmingham, Coventry, Tottenham, and several other towns, when so treated, yields effluent water quite as clear and colourless as ordinary river water; whilst it is so free from any disagreeable smell or taste that it may be taken into the mouth and drunk without

repugnance; nor does it upon standing deposit any offensive sediment. Fish, as may be seen at Tottenham, even when in confinement, live and thrive in the purified sewage water. The effluent water from London sewage taken at the Barking Creek outlet, when thus purified, remained during the entire hot summer of last year without evincing either in open or closed vessels any tendency to putrefy or emit any offensive odour.

In face of the above facts, it is maintained that such water may be allowed to flow directly into any river without occasioning any offensive deposits in its bed, polluting the water to any sensible degree, or causing injury either to the fish in them or the health of the inhabitants on the banks.

Sewage Precipitate or Manure.—The sediment or deposit thrown down from the sewage in this process of precipitation, contains the entire amount of mechanically suspended matter present in the sewage, along with much of the organic matter (particularly the nitrogenous or albumenoid) held in solution in the sewage previous to its purification, and the whole of the phosphates employed in the process, which now, however, are in a much more valuable condition, since they are precipitated in a hydrated or soluble state fit for being at once taken up by the plants.

It is not pretended, however, that this process extracts anything like the entire fertilising ingredients contained in the sewage, since it is admitted that by far the largest portion of the ammonia present in the sewage flows off with the effluent water, and is consequently lost unless this water is employed for the purpose of irrigation.

The precipitate is in itself, however, a valuable manure, which is mainly owing to the most important and characteristic feature of the phosphate process, in which it differs from all processes now in use, which is that the substances employed in effecting the purification of the sewage are only such as augment greatly the agricultural value of the precipitated deposit, which can be increased at pleasure by merely adding more of the phosphate, so that it can by this means be rendered sufficiently valuable to bear the cost of transport to a distance from the seat of manufacture.

The proportion of the native phosphate of alumina required in this process naturally varies according to the strength and character of the sewage. Although in some cases less than half this quantity was found to be sufficient, the actual amount now employed on the large scale at the Tottenham sewage works is in the proportion of 1 ton of the crude phosphate to 500,000 gallons of sewage.

The crude phosphate is prepared merely by mixing it in the state of powder, with sulphuric acid in the proportion of 1 ton of the phosphate to from 12 to 14 cwts. acid (170° Twaddle), and allowing it to stand a short time; the prepared phosphate has then the appearance of a brown paste of about the consistence of stiff clay, and only required being dissolved in water, and allowed to flow into the sewage in the proper proportions. The lime employed was ordinary burnt limestone or chalk slaked in water, and before being added to the sewage was stirred up in a tank with as much water as would bring it into the condition of so-called milk of lime. The amount employed varied with the quality of the sewage, and the quantity of phosphate added; it was, however, always added until the sewage acquired a very faint alkaline reaction, which, in the case of the Tottenham sewage, required in general the addition of about one-half the weight of the prepared phosphate previously employed.

The native phosphate of alumina contains from 30 to 40 per cent of phosphoric acid (equivalent to from 65 to 87 per cent tribasic phosphate of lime), and consequently at its present price of £3 10s. per ton is probably the cheapest of all sources of phosphoric acid known in the market. When, however, it has been dissolved in acids and re-precipitated by lime, it is then converted into hydrated phosphates, which, weight for weight, are then worth fully double the value of the original crude phosphate, since they are then much more soluble and readily available for plants. Against this increased value must, however, be placed the expense of the acid, labour, &c., so that except in manufacturing districts, this is not expected to leave any great margin of profit; still there can be little doubt but that it will as a rule be found more profitable to use considerably more phosphate (say 2 tons to the million gallons of sewage) than is absolutely necessary to effect its purification, in order to augment the market value of manure, and so to a great extent make the process a self-paying one.

In practice, the quality of the manure must naturally be regulated to suit the market of each locality, and it will probably be found most advantageous to add as much phosphate to the sewage as will enable the manure to find a ready sale at a price somewhere between £3 and £5 per ton; in order, however, to have an unbiassed and competent opinion on the value of a high class manure of this character, Dr. Voelcker was requested to treat a quantity of London sewage by this process, and report his analysis of the deposited manure.

The whole experiment was left entirely in the hands of Dr. Voelcker, who procured the sewage from London Bridge, and reported as follows:—

My Dear Sir,—The following is the composition of the sewage deposit which you desired me to analyse:—

100 parts of the dry sewage deposit contain:—	
Moisture (loss on drying at 212° F.)	3·98
* Organic matter and water of combination	20·11
† Phosphoric acid	28·52
Lime	13·09
Alumina and oxide of iron, and a little magnesia and alkaline salts	29·95
Insoluble siliceous matter	4·35
	100·00

* Containing nitrogen 0·57; equal to ammonia, 0·69.
† Equal to tribasic phosphate of lime, 62·26.

The whole of the phosphoric acid, I may state, occurs in this manure in the shape of precipitated phosphates, a form I need hardly say in which the phosphates are readily available by plants. The presence of a large amount of water of combination in the deposit, that is, water which is not expelled on drying at the boiling-point of water, shows that the phosphate with which the water enters into chemical combination, and which does not escape at 212° F., are not likely to return readily into the ordinary and, comparatively speaking, ineffective condition in which they exist in phosphatic minerals. Being obtained by precipitation from their solution, the phosphates are present in the deposit in a very efficacious form.

The sample analysed by me contains an amount of such precipitated phosphate, which is equivalent to 62 per cent of tribasic phosphate of lime, and an appreciable amount of nitrogenous organic matter, capable of yielding 0·69 per cent of ammonia.

It possesses valuable fertilising properties, and in my opinion, a sewage manure, equal to the sample analysed by me will command a ready sale at £7 7s. per ton.—Believe me, yours faithfully,

(Signed) AUGUSTUS VOELCKER.

A. P. Price, Esq.

When perusing the above report, however, it must be borne in mind that this high estimated price is not due to the matter actually precipitated out of the sewage itself, the agricultural value of such sewage precipitate (independent of the phosphates added in the process) being probably seldom more than £2 10s. per ton, if so high; on the contrary, it is all but entirely due to the value of the phosphates added in the process, which must be paid for, together with the cost of acid * and labour required in

* In such manufacturing towns as produce muriatic acid as a waste product which can be bought at a nominal price, this acid could be used instead of sulphuric acid, and would, no doubt, bear a handsome profit.

the manufacture to bring them into the soluble condition; consequently, as before mentioned, the profit, if any, will be but small, and not sufficient to encourage the belief that the ratepayers can altogether escape putting their hands into their pockets in order to cover the expense of disposing of their sewage.

There is every reason, however, for believing that the manure produced in this process, if it does not actually cover the expense, will at least go a long way towards doing so; and when the small outlay required to start this process is compared with the enormous capital necessary in most cases to carry out the elaborate plans of sewage irrigation, it is believed that in many instances this system of sewage precipitation will be found considerably more advantageous and economical in practice.

ON THE

ANALYSIS OF CHLORAL-HYDRATE.

By K. MULLER.

THE method of dissolving chloral-hydrate in ammonia, as also in caustic alkalies, with the application of heat, has shown unsatisfactory results, differing very materially from each other; the cause of these deviations was in the partial transformation (taking place at 50° C.) of the chloroform, under the influence of caustic potash, into chloride of potassium and formiate of potash, according to the formula—

$$\underbrace{CHCl_3}_{\text{Chloroform.}} + 4KOH = \underbrace{CHOKO}_{\text{Formiate of potash.}} + 3KCl + 2H_2O.$$

All heating, therefore, has to be avoided; and for this purpose the decomposition is carried out in a burette, divided from the bottom into tenths of cubic centimetres, in such a manner that 25 grms. of chloral-hydrate are carefully covered by a layer of a dilute solution of caustic potash in water. This is allowed to remain for about fifteen minutes, and the decomposition is completed by careful and thorough shaking. After the lapse of several hours, the reaction is completed, and both liquids —viz., chloroform and a solution of formiate of potash, with some caustic potash, have separated clearly and distinctly. It is now only necessary to read off the cubic centimetres of the lower layer of chloroform, and then multiply it with the specific gravity of the chloroform (1·525 at 32° F.), in order to arrive at the weight produced (in grammes), and thereby calculate the percentage.

I first directed my attention to the chloral-hydrate manufactured by Mr. E. Saame, of Louisenhall, near Gottingen, as being the most convenient for me, and whose process of manufacture (agreeing fully with that recommended by Dr. O. Liebreich) I had often the opportunity of observing.

The results I obtained are as follows:—

Sample No.	Manufacturer, and by whom supplied.		Boiling-pt. Cent.	Weight of Chloroform obtained from 25 grms	Percentage of Chloroform.	General Remarks.
				Grms.	Per ct.	
1.	Manufactured by Mr. E. Saame, and supplied by Messrs. H. and G. Schoettensack London.	Cake.	96·5	17·98	71·9	A crystalline cake of dazzling whiteness; agreeable, slightly pungent chloral odour; easily soluble in water; melting-point, 52° Cent.
2.		Powder.	96·5	17·98	71·9	A white powder; results same as above.
3.		Crystals.	97·3	17·85	71·4	Colourless, transparent, well-formed, large rhomboid crystals, with an agreeable odour; soluble in water.

I therefore recommend my simple method, as shown above, to every chemist, in order that he may be able to form, with little trouble, a judgment of the value of chloral-hydrate supplied to him.

Laboratory, University of Göttingen,
January, 1871.

ON THE

ANALYSIS OF AN ALKALINE SPRING FROM CATHKIN, NEAR RUTHERGLEN.

By J. WALLACE YOUNG.

THIS spring issues from a bore, put down by the proprietor of the ground, in order to ascertain if any workable minerals were to be found. It is in the carboniferous formation bordering on the Trappean Hills of the district. Unfortunately, I cannot give any information as to the depth or the nature of the strata passed through.

Water clear and colourless, does not deposit anything on standing. Turmeric paper immersed in the water for a short time gives a well-marked alkaline reaction.

	Grs. per gallon.	Grs. per 100,000.
Total residue	35·700	51·000
Consisting of—		
Volatile and combustible matter	0·650	0·925
Fixed residue	35·050	50·075
The fixed residue consisted of—		
Lime	0·890	1·271
Magnesia	0·320	0·457
Soda	17·410	24·871
Sodium	1·270	1·814
Silicic anhydride	0·400	0·571
Carbonic anhydride	13·420	19·171
Sulphuric anhydride	trace	trace
Chlorine	1·950	2·785
Peroxides of iron and alumina	trace	trace
	35·660	50·940

The soda was determined both directly and indirectly, the two results closely agreeing. A sufficient quantity of soda to saturate the chlorine has been calculated as sodium.

The total quantity of carbonic anhydride present in the water was determined in two different samples, taken at different times, the result being similar.

	Grs. per gallon.	Grs. per 100,000.
Total CO_2 by experiment	26·95	38·500
Total CO_2 required to convert the bases into bicarbonates	26·84	38·342
Excess of CO_2	0·11	0·158

I have arranged the constituents as follows, but, as I have given the original figures, any one can make an arrangement to suit himself.

The silicic anhydride is probably combined with part of the soda, but I have followed the usual custom of putting it down alone.

	Grs. per gallon.	Grs. per 100,000.
Bicarbonate of lime	2·30	3·285
Bicarbonate of magnesia	1·03	1·471
Bicarbonate of soda	42·11	60·157
Chloride of sodium	3·22	4·600
Silica	0·40	0·571
	49·06	70·084

No nitrates present. The residue of the water, when examined with the spectroscope, showed nothing but the sodium line.

The absence of sulphuric acid is rather remarkable. Most of the deep well waters of London contain more or less carbonate of soda, but the composition otherwise is

very different (see Dr. Odling's paper "An Account of Guy's Hospital Well," in the CHEMICAL NEWS, vol. iii., pp. 35 and 49).

In the *Philosophical Magazine* for May, 1860, there is also an analysis, given by Mr. Abel, of a water from the coal strata of Bradford Moor, Yorkshire, which contains 43·53 grs. per gallon of bicarbonate of soda.

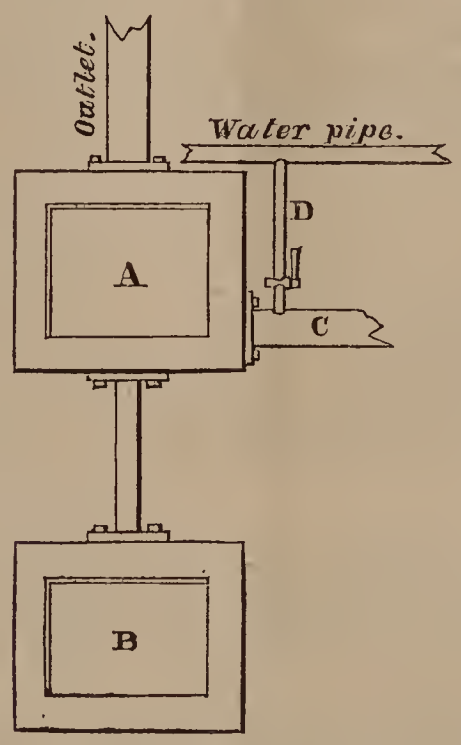
NOTE ON THE PUMPING OF HOT LIQUIDS.

By J. SUTHERLAND.

HAVING occasion last summer to erect pumping apparatus to pump off hot lyes from two large soap pans, I experienced considerable difficulty in getting a sufficient flow of the liquid when its temperature was at all near the boiling point,

When the pump was set on, and the connections cold, it threw well for two or three minutes, but directly the metal became heated to the temperature of the liquid, we could pump almost nothing but vapour.

This result, of course, arose from the well-known fact that liquids boil at lower temperatures exactly as the pressure on its surface is decreased. The pump itself was



simply a steam cylinder, having a set of ports opened and closed by slide valves, and producing an exhaust by means both of the up and the down stroke. This arrangement gave excellent results so long as the liquid was comparatively cold, throwing a continuous stream 2½-inch bore or about 10 to 12 tons spent-lye per hour. Our spent lyes are usually very hot, however, and, as we could not possibly wait till they cooled, the pump must do duty at any temperature. Of course, the cause of failure was due to the exhaust produced by the pump being as rapidly filled up as formed by the vapours generated from the liquid, brought to a state of ebullition by the decreased pressure produced in the pump connection.

Had it been admissible to place a foot-valve, opening inward, at the lower end of the inlet pipe, it would, probably, have done some good, but, in our case, this was not allowable.

It then occurred to me that if I could introduce a fine spray of cold water at the inlet of the pump it would condense the troublesome vapour and allow the pump to do its work. A water-pipe passed along the wall within a foot or two of the inlet, and, between the two, I opened a connection with a ¼-inch lead pipe having a stop-cock to regulate or shut off the water supply. The end fitting into the cast-metal pipe was filled with a small rose in order to spread the water on entering. The accompanying rough sketch shows the arrangement.

A is the pump cylinder, B the steam cylinder, C a 3-inch inlet, D ¼-inch pipe introduced on the upper surface of the inlet pipe. It is imperative that it be introduced on the upper surface of a horizontal pipe as it is there the first vapour will gather and be exposed to the condensing action of the spray. Were the cold water introduced at the lower surface of the pipe it would mix with the hot liquid and, in all probability, its action would be almost *nil*. The application proved a thorough cure, and not more than about ⅛th of an inch bore of water was necessary to set the 3-inch inlet in full action.

ON FERMENTATION.*

By Professor A. W. WILLIAMSON, F.R.S.

(Concluded from p. 103).

M. PASTEUR has introduced a process which, I gather from his statements, has already been adopted by a considerable number of wine-growers and merchants, which goes to the root of the matter in such a way as to leave nothing to chance, for he has proceeded upon the knowledge previously acquired by his accurate and masterly experiments, regarding the nature of all these little organisms, and the conditions which are favourable to their development, and which are destructive of them. He finds that when wine is heated to near the boiling-point in any vessel in which it may be enclosed, and left in that vessel to cool, it may then be kept (provided the vessel be not opened) for any length of time without undergoing any of these deleterious changes. He finds also that so high a temperature as that I have named is not absolutely necessary, but that even if wine, which on keeping would be subject to the malady of acetification or ropiness, be heated to a temperature of 60° C. (that is, about 140° in the clumsy and inconvenient scale which is still, I am sorry to say, in common use in this country), it will kill these organisms completely; and the experiment is so easily performed that any of you may do it with a very moderate amount of care. You ought to perform the experiment with several bottles at the same time, and to keep some similar bottles in the original state, in order to observe the difference. In the month of September, when I last saw M. Pasteur, he gave me several bottles of wine, some of which were in their original state, whilst the others had, after bottling, been heated to a temperature of 60°, or a little more. I have not yet opened any of them, but I have some of each sort here, and we will presently see what has been the result of the treatment. I ought to say that they have not been kept with proper precautions, for, on opening the case, I found that it had been left in such a position that the bottles had been standing with their necks upwards, which is not very favourable to the preservation of the wine. The experiment is so simple, that it is worth while for everybody to perform it. You should take some light wine, which you have reason to believe will not keep well; the bottles should not be too tightly corked, and there should be a little space left below the cork. You put several of these bottles into a vessel of water, cautiously if the water be warm, to avoid breaking them, and with them one bottle full of water, uncorked; you then warm the whole very gradually, until you find, by inserting a thermometer into the open bottle of water, that the temperature is up to 140° F.; you then allow the whole to cool slowly. The corks are generally lifted a little by the expansion of the liquid and air within the bottles, and will require to be struck in again to their proper place. The same operation is performed on a large scale by wine-growers and merchants in France, in casks, and several contrivances have been described for the purpose. The simplest of all is to put a cask, with

* The Cantor Lectures. Delivered before the Society of Arts.

its bung upwards, into any convenient vessel of water, so placing it that the top of the cask is just above the water. The water surrounding the cask is then warmed gradually until it is found, by lifting the bung and inserting a thermometer, that the wine is of a temperature 60° or 70° C. The bung is then closed, and the whole allowed to cool. Another form of apparatus has been figured in a late book of M. Pasteur's on acetification, which consists of a cask with one of its ends removed, and replaced by a sort of double bottom of metal. This cask is then put on the fire, so that the water in the false bottom may be heated, and raise the temperature of the wine in the cask above, without danger of burning it. M. Pasteur recommends that when the wine has reached the right temperature it should be allowed to run, while still hot, into the cask into which it is to be kept, so that any little germs which may be present there may be as much heated as the wine which comes in contact with them. He heats the wine in this operation to about 65° or 70°, but he says there is reason to believe that even 50° is sufficient. Upon all occasions on which it has been tested it has been found that the little parasites which are present, and which are the seeds of the maladies of which we have been speaking—and, no doubt, other organisms, are changed in such a manner as to be practically dead. Whether they are susceptible of being revived is another thing. It is not known and it is not affirmed that there are no germs which might not, by contact with oxygen, be afterwards brought into life; but practically there are no organisms in the wine, after that temperature has been applied to it, capable of growing in the closed vessel in which it is kept. M. Pasteur says that if the wine were, after that treatment, bottled, he would expect that some bottles would contain wine which would spoil, whereas the greater number probably would not, the reason being that the wine, on its passage from the cask to the bottle, would be liable to get some little germs from the air which possibly might retain some vitality, which would be stimulated by the oxygen. Therefore, all he affirms is, that when the wine is kept in the same vessel in which it was heated, it undergoes no further change whatever.

With regard to the ordinary process of aërating wine by keeping it in bottles, I should like to show you an experiment which illustrates, in a very simple way, what is a very familiar well-known fact. Every one who has had occasion to keep wine knows what an immense difference there is if you keep a bottle standing upwards or lying down. The difference is of this kind. If it stands upwards, the cork is dry, and air has access at a very rapid rate to the contents of the bottle, and the wine gets oxidised and spoilt; whereas, when the bottle is left lying down, the cork is wet, and the air has access much less rapidly,—in fact, only at such a low rate as is suitable for mellowing and improving the wine. I have here a couple of glass tubes, both open at one end, and closed at the upper end by a porous substance, which I may call a cork—it is, in fact, a cork made of plaster of Paris, a particularly porous substance,—one cork being wetted, so that the pores are full of water, whilst the other has been carefully kept dry, and this one is covered for the present with a little cap to prevent the access of the air. Here, in another vessel, is a mixture which is giving off hydrogen gas, which is passing upwards into these two tubes, one with a wet cork and one with a dry one. After a minute or two, both tubes will be full of pretty nearly pure hydrogen, and then we will remove them, and put the lower ends into this jar containing a coloured liquid. Most of you know that this porous substance allows hydrogen to pass through it more rapidly even than the air which is now outside passes in, and therefore as the hydrogen passes out of these tubes more rapidly than the air comes in, the liquid will be sucked up in the tubes, and we shall have a measure of the rate at which our gas passes through the wet cork and the dry cork, by noticing the difference in the rise of the liquid in the two tubes. If it passes quicker through the dry cork than the wet one, we shall find that the liquid will rise more rapidly in that tube, and I think you will find that the difference will be very great indeed. They are now both standing in the coloured liquid, and already there is a perceptible rise in the tube with the dry plug; but in the other one I cannot yet see the liquid at all. So it is in the simple case of a wine bottle. If the cork of a bottle is wetted, so as to allow an exceedingly slow diffusion of air through the contents, the wine gets very slowly oxidised, and undergoes only that gradual transformation which is wanted; whereas, in the other case, it is turned sour and spoilt by too rapid oxidation.

I could gladly have entered upon many other facts and considerations which belongs to this subject, but one must stop somewhere. I cannot, however, part from you without expressing my very strong sense of the debt which we owe to that great investigator whom I have already quoted so many times, I think there are few precedents of research so fruitful as those of M. Pasteur, if we consider, not only the theoretical importance, I mean with regard to our knowledge of the processes of life, and the origin of life, of his investigations regarding germs in the air, and these processes of fermentation; but if we take into account also the fact that he has succeeded in working out one of the most complete practical applications of it in a process like wine-making and keeping,—we cannot refrain from admiring the truly perfect adaptation of the highest science to a useful purpose. I will now proceed to open these two sets of bottles, some of which have been heated and some not, and I hope the rusult will be satisfactory.

[The samples of wine were then tasted by the audience, the difference being most remarkable, not only in taste, but also in colour and general appearance.]

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

March 2nd, 1871.

Professor Williamson, F.R.S., President, in the Chair.

The following gentlemen were elected Fellows:—G. D. Harding, W. H. Hudleston, A. H. Mason, J. J. Nicholson.

The following papers were read:—

"*On the Distillation and Boiling-Point of Glycerin*," by Thomas Bolas. It is well known that, when glycerin, subjected to the ordinary atmospheric pressure, is heated so much as to cause ebullition, it is more or less rapidly decomposed by repeated distillations. This decomposition may be, however, entirely prevented by a reduction of the pressure in the apparatus employed to 12·50 m.m. The boiling-point of glycerin was determined by effecting the distillation in a long-necked flask, having a supplementary neck attached at right angles to the principal one. In the principal neck, the thermometer was fixed by the aid of a caoutchouc cork, while the smaller neck was connected in a similar manner with a two-necked receiver. The glycerine, together with a few fragments of tobacco-pipe (this latter being required to prevent the bumping which would otherwise occur), being placed in the retort-flask, the receiver was connected with a Sprengel's mercurial pump and a manometer, the caoutchouc joints being made air-tight with glycerin in the usual way. Unless the glycerine distilled had been dehydrated by previous distillation in a vacuum, the first portion of the distillate consisted principally of water; afterwards, when the glycerine in a pure state came over, the temperature indicated

by the thermometer was 179·5° C. At this time, the pressure on the liquid was 12·5 m.m., a pressure nearly corresponding to the tension of aqueous vapour at the temperature of the receiver. A determination of the carbon and hydrogen in the glycerin distilled as above was made; the oxidant employed being copper oxide, followed by oxygen gas, (I.), 0·4281 grm. CO_2 and 0·3439 grm. H_2O.

	Theory.		Found.
			I.
C_3	36	39·1	38·9
H_8	8	8·7	8·9
O_3	48	52·2	
	92	100·0	

Under a pressure of 50 m.m., glycerin distils without change at about 210° C. Glycerin, dehydrated by distillation, absorbs water from the atmosphere to the extent of about 50 per cent of its weight. The amount absorbed is, as might be expected, very variable.

"*On the Action of Heat on Silver Nitrite*," by Edward Divers, M.D. The products of this action consist principally of silver nitrate, reguline silver, and oxides of nitrogen. But the relative proportions of the quantities of these substances to each other, and, as a consequence of this, the composition also of the gaseous matter, vary considerably in different experiments. When silver nitrite is heated in a crucible or watch-glass over a lamp, its decomposition becomes first evident by its turning grey when in contact with the hot crucible, and by its giving off red fumes. The mass very soon enters into a state of semi-fusion, and then becomes solid again, and of a whitish grey in some parts and greenish yellow in others, red fumes still being produced (but much less copiously than during the intumescence). Lastly, on continuing the heat, the yellow portions of the mass slowly change to grey, like the rest, with the formation of a small quantity more of red fumes. Prolongation of the heat beyond this point causes the glass or porcelain to be attacked; but, as the entire greying of the mass has been found to be a sign that nothing, or almost nothing, remained but silver nitrate and free silver, it is here unnecessary to pursue, beyond this point, the changes which take place. As there is found to be a material difference in the loss of weight sustained in different experiments, it might naturally be supposed at first that this is due to a decomposition of the silver nitrate formed. But that it is not so is shown, firstly, by the fact that longer heating at about the same temperature causes very little more loss; secondly, that the temperature does not appear to be so high as that required for the free decomposition of silver nitrate; and thirdly, that the loss sustained bears no direct relation to the degree of heat employed. When the nitrate in an open dish is heated in an oven at any temperature between about 85° on the one side, and 140° C. on the other, it loses its sensible characters almost imperceptibly, becoming whitish grey and non-crystalline, without fusion or any apparent change in bulk. The change which takes place in the nitrite when it is heated in an open vessel in either of the above ways, does not generally differ very much in its results from that represented by the equation—

$$3NO_2Ag = N_2O_3 + Ag_2 + NO_3Ag.$$

When, instead of employing an open crucible, or dish, for heating the nitrite over the lamp, a closely-covered one is used, so that the gaseous and fixed products of decomposition may be kept for a time in contact, the loss of weight experienced by the mass is less, and the amount of nitrate formed greater. The ultimate change effected in this way approaches, though not closely, to that expressed by the equation—

$$2NO_2Ag = NO + Ag + NO_3Ag.$$

The effect of keeping gases given off in contact with the undecomposed nitrite remaining is to cause nitric oxide alone to be lost. This has been shown by heating the nitrite in a vessel nearly closed, so as to decompose the nitrite very gradually. 0·4433 grm. of the nitrite were placed in a small tube, and the empty portion of the tube filled up by a glass rod barely small enough to be pushed into it. After twelve hours at about 90°, it weighed 0·4431 grm.; after twelve more at 95°—99°, it weighed 0·4437; after twelve hours more at 115°—135°, 0·446 grm.; after fourteen hours more at about 134°, it weighed 0·450 grm.; after again fourteen hours, mostly at 136°, 0·453 grm.; and after fourteen hours more, when the heating was discontinued, 0·4544 grm. There was, therefore, instead of a loss, a material gain in weight equal to 0·0111 grm. Faintly reddish gas was first detected in the tube at 115°. At 134°, the crystals began to shrink and fuse together, and this occurred first near the stopper of the tube, and only very slowly proceeded backwards to the end of the tube. It would not, therefore, be right to conclude that silver nitrite fuses at this temperature; the fusion apparently taking place only in the nitrite after it had become mixed with nitrate. During the heating the mass scarcely altered in colour. The explanation of the gain in weight is, of course, evident. The nitrite decomposing and evolving nitric peroxide, or at any rate a nitrogen oxide, this becomes reduced to nitric oxide, if not already such, by oxidising unchanged nitrite; the nitric oxide then serves as a carrier of atmospheric oxygen to the nitrite, as it is known to do to other substances. The extent to which atmospheric oxygen was thus fixed in the preceding experiment is more fully shown by the examination which was made of the contents of the tube. These were treated with water, and found to contain 0·0216 grm. of metallic silver, which, therefore, as nitrite had been in combination with 0·0092 grm. of gaseous matter. This loss being balanced against the oxygen absorbed, its weight must evidently be added to the observed gain in weight to obtain the actual amount of oxygen absorbed by the rest of the nitrite. The total oxygen absorbed is, therefore, equal to 0·0203 grm.; of this, about 0·017 grm. must have been derived from the air, the rest coming from the decomposed nitrite, thus: $NO_2Ag = Ag + NO + O$.

It is calculated that not very much less than half the nitrite was converted into nitrate in this experiment. This experiment is sufficient to establish that hot nitrate reduces the higher oxides of nitrogen to nitric oxide; so that we have proof that the tendency under one set of circumstances is to ultimately convert nitrite into the products metallic silver, nitric oxide, and silver nitrate, according to the equation already given. But it is quite clear that nitric oxide and the silver nitrate are secondary products of the action of heat. This can, however, be shown in another way to be undoubtedly the case, as in the following experiments:—2·098 grms. were heated in a dish in an oven for seventy-two hours, at a temperature of about 98°, in a moist atmosphere obtained by keeping a dish of water in the oven. The loss in this experiment is equal to 0·441 grm., and the residual mass, after washing out with water and re-ignition, yielded 1·157 grms. of silver. The soluble matter contained a little nitrite. 0·794 grm. was heated for about ninety-five hours in an atmosphere of steam at a temperature rising from 98° to 140°. The residue weighed 0·580 grm. This experiment was not pursued further. These two experiments are sufficient to establish the fact that, under a given set of conditions—viz., free exposure in a moist atmosphere—silver nitrite tends to yield only metallic silver and nitrogen peroxide, thus: $NO_2Ag = Ag + NO_2$. The conclusion, then, from all these experiments is that, like other silver salts, the nitrite splits up, under the influence of heat, into metallic silver, and the acid radical, or its components, and that silver nitrate, nitric oxide, and perhaps nitrous anhydride, are formed only by secondary reactions. With regard to nitrous anhydride, the experiments which have been here given to prove that the gases evolved are either nitrous anhydride or its equivalent furnish no proof that they actually include this body. The fusion which occurs in

the mass of heated nitrite, so soon as it has undergone some oxidation, suggests that the nitrate formed combines with the nitrite to form a nitrite-nitrate or hyponitrate; the point at which this fusion occurs is far below that at which silver nitrate fuses. In one experiment, the nitrate formed is calculated to have been in nearly the proportion of one atom to one atom of the undecomposed nitrite; it is, therefore, not improbable that an unstable nitrite-nitrate exists.

The silver nitrite Dr. Divers had employed in his experiments had been carefully prepared, and some of it ascertained to contain 70·06 per cent silver. The theoretical quantity is 70·13.

The President inquired in what way Dr. Divers had ascertained that the solid residue contained no argentic oxide.

Dr. Divers replied that the residue had the appearance of metallic silver, and that, after weighing it, he had subjected it to strong ignition, and then weighed it again, when usually no difference presented itself in the two weighings. In one case only, there was, after ignition, a loss of less than ½ a milligramme.

Dr. Odling asked what were the evidences for the existence of a silver nitrite-nitrate. The fact of such a compound existing would greatly modify the views at present entertained on the constitution of salts.

Dr. Divers said that he deduces it from the circumstances that the nitrite began to fuse at about 140°, after partial decomposition. Since the fusing-point of silver nitrate is much above that degree, he is inclined to attribute it to a compound of the nitrite and the nitrate, which, like alloys, may have an intermediate fusing-point; but, on the whole, he considers the combination only a loose one.

Dr. Odling observed that the circumstance Dr. Divers had mentioned proved merely that there was a mixture of the two salts, since it is well known that such mixtures fuse at some lower degree of temperature than either of the salts separately.

Dr. Divers agreed with Dr. Odling that the evidence for a nitrite-nitrate is but a slender one. He wished, however, to point to one of the experiments mentioned in his paper where the proportion of the nitrate formed to that of the nitrite left undecomposed was that of one atom to one atom. He, moreover, had reasons to doubt the existence of a silver hyponitrate, silver being monovalent. It stood differently with lead, which is a divalent element.

Dr. Armstrong, with regard to the latter opinion, remarked that Professor Wislicenus, of Zürich, had, in a recent publication,* stated some reasons for viewing silver as a divalent metal.

Dr. Mills asked what proof there was that the gas really was nitrous acid, and not a mixture of nitric oxide and oxygen.

Dr. Divers did not insist rigidly upon that view: it may be, perhaps, such a mixture.

After the reading of the above papers, Dr. Gladstone communicated some remarks "*On the Relations of Chemical Reaction and Time.*" He had instituted most varied experiments bearing on this subject, and in briefly alluding to some of them he wished to call the attention of chemists to this wide field of inquiry. Hitherto, observation seems to be limited only to the circumstances at, and the products with, which a chemical reaction begins and ends; all that happens between was left wholly unnoticed. Yet, how fruitful attention paid to the intermediate stages and products of a reaction could be was seen in the beautiful results which Professor Williamson had gained in his researches on etherification.

The President, Dr. Odling, Mr. Vernon Harcourt, and others concurred in Dr. Gladstone's view as to the importance of a closer study of this subject.

* *Berichte der Deutschen Chemischen Gesellschaft zu Berlin*, iv. Jahrg., p. 63.

CORRESPONDENCE.

JOULE AND SCORESBY'S EXPERIMENTS.

To the Editor of the Chemical News.

Sir,—Let me thank Mr. Apjohn for his letter to you published in the Chemical News of March 3rd.

As my only object is truth, I shall gladly yield any point in which I am convinced of error.

(1). In the first place, therefore, let me say that if I appeared to express a doubt that a grain of zinc could be doing a maximum *duty* when doing least *work* in a given time, I retract my expression.

(2). Next let me say that after reading Mr. Apjohn's explanations of Joule and Scoresby's arguments, I am as little satisfied with their reasonings as before, though I confess, I somewhat misunderstood them. If, indeed, it be true (and on reading Mr. Apjohn's explanation I think it is true) "that the assumption $H : H' :: a^2 : b^2$ (H, H' representing the heat, and a and b the intensities of the currents) is not an irrelevant statement, but is, in fact, the fundamental principle on which the formula is based," then I reply that the formula is based on a false principle. For the heat is as the square of the intensity only when the resistances are equal. And in this case the resistances are not equal, the working of the engine introducing a fresh element of resistance.

(3). Mr. Apjohn says that "if I had said and understood that the heat evolved per "*given quantity*" of zinc consumed was directly proportional to the intensities, I should have been quite correct, and should probably have avoided the subsequent errors into which I have fallen." On the contrary, I maintain that the heat evolved in a given battery by a "*given quantity*" of zinc is *always* the same and does not vary as the intensity or in any other manner.

(4). Mr. Apjohn says "Mr. Highton should have known that $(a-b)$ represents the difference of heat evolved per grain of zinc, and not, as he supposed, the difference between the total amounts of heat produced by the battery in each case, which would be represented by a^2-b^2." On the contrary, I assert that, *with the same battery*, if the resistance be varied so as to reduce the intensity from a to b, the *total amount* of heat produced by the battery in a given time is reduced in the proportion of a to b, and not in the proportion of a^2 to b^2.

(5). If Joule and Scoresby assert, as they do, that $a-b$ represents the diminution of intensity, and the diminution of consumption of zinc, and also the amount of heat converted into useful mechanical effect, I think I was justified in drawing the conclusion that this involved the assumption that a less quantity of zinc produced an equal amount of heat, but that part of this heat was absorbed in doing work. I confess I do not see how this inference can be avoided. I conclude, then, that though Mr. Apjohn has certainly shown that I misapprehended part of Joule and Scoresby's reasoning, this does not affect the general result or make their position more tenable.—I am, &c.,

H. Highton.

Putney, March 4, 1871.

NITROGEN TRIOXIDE A SUPPORTER OF COMBUSTION.

To the Editor of the Chemical News.

Sir,—Some weeks ago one of your correspondents rediscovered that tin was soluble in dilute nitric acid: I trust the following observation is somewhat newer than the above.

The red gas nitrogen trioxide, N_2O_3, has the power of re-kindling a glowing splint of wood or the glowing wick of a taper, just as oxygen or nitrous oxide would do.

The flame continues to burn with a bright but peculiar light, the red fumes disappearing.

The gas prepared by heating either starch or arsenious trioxide with nitric acid exhibits this property.

I am well aware that nitrogen *tetroxide* supports combustion, but I believe the above fact is new.—I am, &c.,

Alfred H. Allen.

Sheffield, February 28, 1871.

AUTOMATIC SPECTROSCOPE.

To the Editor of the Chemical News.

Sir,—My friend Dr. Huggins has recently described to the Royal Society (see Chemical News, vol. xxiii., p. 98) the very valuable little registering spectroscope devised by Mr. Howard Grubb, C.E., of this city, and lent by the latter gentleman to the Oran Eclipse Expedition. This instrument has been re-adjusted and supplied for use in my laboratory as an auxiliary to a very powerful spectroscope with automatic adjustment of prisms lately constructed for the Royal Dublin Society by Mr. Grubb. As some addition has been made to the registering spectrocope, which, in my opinion, renders it specially convenient for laboratory use, I will, with your permission, add a few words to the very interesting description of it given by the distinguished physicist who brought the apparatus under the notice of the Royal Society.

The slit, fitted to the spectroscope in the first instance, has been changed for one carrying a reflecting prism for the comparison of two spectra. A metallic cover encloses the whole of this part of the apparatus when not in use, so that the slit is completely protected from acid fumes.

Instead of employing a card on which to make the pricks in registering the positions of lines in a spectrum, I use a mica plate (of the size sold for covering *cartes de visite*). The mica plate is held securely in its position on the movable table of the apparatus, and two standard points marked at the beginning and end of a set of registrations. The relative positions of the lines are then read off by placing the mica plate over a graduated arc, decimally divided, on a silver plate, taking care to make the standard points on the mica always coincide with particular divisions of the scale. The scale I use is an arc graduated into 250 parts, beginning at the violet end of the spectrum. The measures of standard spectra obtained by this arrangement are entered finally on maps by means of a 250 m.m. scale. I should add that each division of the graduated silver plate corresponds to ten minutes of arc, and, of course, each millimetre on the map has the same value in angular measurement. While I much prefer the mica plates for use in recording observations, card slips can likewise be used, the graduated arc being then laid over the card register and the positions of lines read off. The spectroscope is now placed upon a strong metallic stand, on which it can be moved in a horizontal direction by loosening a small clamping screw. The instrument is now one of the most convenient of its kind I am acquainted with for general laboratory use, as very accurate observations can be made and recorded with its aid in a surprisingly short time.—I am, &c.,

J. Emerson Reynolds, M.D.

Laboratory, Royal Dublin Society,
March 8, 1871.

DEVELOPMENT OF FUNGI IN POTABLE WATER.

To the Editor of the Chemical News.

Sir,—Will you permit me to correct one or two slight inaccuracies in your report of my remarks on Dr. Frankland's paper, and also to add one or two statements which I omitted to make at the time. With respect to charcoal filtration, I did not state that I had never found growths in the filtered water, but that on the large scale the first indication we had that the charcoal required re-burning or renewing, was that the filtered water produced these growths when mixed with sugar. This seems to me incompatible with Mr. Warington's suggestion, that new charcoal would impart phosphates to the water and so cause the growths to appear, whilst old charcoal would not do so, as, in fact, we begin to find them only after all soluble phosphate must have been long removed. I may here remark that it is a curious fact, that West Middlesex water, which may be mixed with sugar and kept for weeks without producing these growths, nevertheless seems to contain the germs in sufficient number to impart them to the charcoal, and, if permitted to accumulate, and perhaps grow in it, they are at last washed out by fresh water in number sufficient to produce growths rapidly, if treated with sugar. Dr. Frankland related how he aired a filter before passing water through it, but he did not tell us how long the charcoal had been under water before he did so, which is a point of great importance. When the large filter of which I have spoken was first put up, the water was kept constantly on it; for about three weeks the water was very good, but, at the end of five or six weeks, it was far worse than the unfiltered water, and no amount of airing put it right. The charcoal was then renewed, and one-half of the filter (which is in two compartments) was used for twenty-four hours, the other half was then put to work, while the first was drained and air passed through it for the next twenty-four hours, and so on. In this way the charcoal does its work well for about twelve months.

With respect to the rapidity of the growth and decay of the fungus, it seems to me to depend much on the proportion between the number of germs and the quantity of sugar which may be, to a certain extent, regarded as their food. If there be a large number of germs, a little sugar, the growth is rapid, and, when complete, breaks up so as to lose its distinctive character, but does not strictly disappear as stated in your report. If there be comparatively few germs in proportion to the sugar, they take longer to make an apparent growth, but go on increasing for a long time. When bacteria are produced in any large numbers, they very speedily destroy the fungus. In most of my experiments, but few of these animals appeared till long after the fungus was developed. I do not at all doubt that phosphorus plays an important part in the *development* of these germs, as Pasteur has shown that it does in that of all ferments, but, at present, I doubt their presence being so universal that the mere addition of a phosphate to any water will set them growing. I am now again at work on the subject, and hope shortly to have some results to communicate to the Chemical Society.—I am, &c.,

Charles Heisch.

Middlesex Hospital.
Feb. 28, 1871.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Journal für Praktische Chemie, No. 1, 1871.

This number contains the following original papers;—

Hypobromite of Soda as a Reagent.—Dr. G. Hübner.—This very lengthy essay, illustrated with several woodcuts, treats on the use of hypobromite of soda for the purpose of decomposing ammonia and other nitrogenous compounds in such a manner as to make it possible to measure the gaseous nitrogen evolved, and hence lead to the quantity of ammonia or nitrogenous matter present in any compound. The author describes at length the following subjects:—Estimation of urea; action of the reagent on other amide-containing substances.

Improvements in the Arrangements of Chemical Laboratories.—Dr. H. Kolbe.—This paper treats of the ventilation, heating, and on the best plan for getting rid of the fumes of acids, and working with badly-smelling or irritating gases, especially sulphuretted hydrogen.

Constitution of Ultramarine.—W. Stein.—The first portion of a paper; we shall leave it until quite concluded.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 20, 1870.

This number only contains the account of the annual general meeting of this Society and the condensed report of its treasurer.

Neues Jahrbuch für Pharmacie, von Dr. F. Vorwerk, January, 1871.

This number contains the following original papers more especially relating to chemistry:—

Poisoning with Hydrate of Chloral, and the Medical Treatment to be Employed in such Cases.—Dr. T. Husemann.—This paper contains an account of experiments made by the author to study the effect of chloral and its antidotes in animals, and also the record of some cases of poisoning which have happened.

Pulverised Iron Containing Copper.—Dr. Mirus.—This memoir treats on an adulteration, usually quite inadvertently made, of that preparation of pharmacy known as ferrum pulveratum, with metallic copper and other metals—lead, tin, antimony—precipitable from acid solutions by sulphuretted hydrogen, which reagent, according to the author, should not tinge in the least an acid solution of 4 grms. of the sample of pulverised iron to be tested.

Testing of Antimonial Preparations for Arsenic.—Dr. Von Ankum.—The author has instituted a comparative investigation of the various methods suggested for the detection of arsenic in antimonial preparations, especially in tartar emetic. The author's method is to ignite the antimonial preparation to be tested with four times its weight of pure nitrate of soda, to exhaust the residue with water, to acidulate the solution slightly with nitric acid, to concentrate by means of evaporation, and to add first nitrate of silver, and next, very carefully, some ammonia; the result being the formation of a more or less deep brown-red coloured precipitate, indicating the presence of arsenic, if present. The author states that, having taken only 10 grs. of tartar emetic, containing in that quantity less than half a milligramme of arsenic, he was enabled to detect the latter very readily by the method described.

Chloroform as a Means of Improving the Taste of Cod-Liver Oil.—Dr. Hager.—The author states that the addition of ten drops of chloroform to 100 grms. of cod-liver oil render that fluid perfectly agreeable and palatable to take, without in the least impairing its good quality, or interfering with its effect on the human frame.

Polytechnisches Journal von Dingler, first number for January, 1871.

This number contains the following original papers relating to chemistry and collateral sciences:—

Utilisation of Peat.—Dr. Dingler.—An account of the manufacture of good fuel from peat, as carried on in some parts of Bavaria, with full statements of cost and profit.

Testing the Genuineness of Silver Plating on Metals.—Dr. Böttger.—A cold saturated solution of bichromate of potassa in nitric acid (sp. gr. 1·2) is taken, and applied to the metallic surface to be tested, after having been previously cleaned with strong alcohol, in order to remove dirt, fatty matter, and especially any varnish. A drop of the test fluid is then applied to the metallic surface by means of a glass rod, and immediately afterwards washed off with some cold water. If pure silver is present (as regards silver coins, these are left in contact with the test fluid for a greater length of time) there will appear clearly a blood-red coloured mark (chromate of silver). Upon German silver the test liquid appears brown, but after washing with water the blood-red coloured mark does not appear; the so-called Britannia-metal is coloured black; on platinum no action is visible; metallic surfaces coated with an amalgam of mercury yield a reddish speck, which, however, is entirely washed off by water; on lead and bismuth the test liquid forms a yellow-coloured precipitate; zinc and tin are both strongly acted upon by this test liquid, which, as regards the former metal, is entirely removed by water, while, as regards the latter, the test liquid is coloured brownish, and addition of water produces a yellow precipitate which somewhat adheres to the tin.

Second Number for January, 1871.

This number contains the following original papers and memoirs relating to chemistry and allied substances:—

Researches on a Graphite from Styria.—J. Stingl.—The raw material here alluded to exhibited a coarsely foliated structure, strong metallic lustre, sp. gr. 2·1443. 100 parts of the substance were, on analysis, found to contain—Carbon, 82·4; silica (belonging to the ash), 12·38: alumina, 3·9; peroxide of iron, 0·53; proto-sesquioxide of manganese, 0·62; lime, 0·02; and traces of alkalies. A sample of the quartz imbedded in and interspersed through the graphite was also analysed and found to contain, in 100 parts—Silica, 99·20; peroxide of iron, 0·46; alumina, 0·12.

Contribution to our Knowledge of the Chemistry of the Blast Furnace, being an Appendix to the Papers on the Blast Furnace, by T. Schinz.—From a foot-note added to this exhaustive essay, we learn that this paper will shortly be published in full in English.

Interposition Scale for the Spectroscope.—Dr. J. Müller.—A lengthy essay, illustrated by woodcuts and engravings.

Experiments on the Making of Malt without Germination.—Dr. H. Fleck.—This essay contains the detailed account of a series of experiments made by the author, with the view to ascertain how far it is possible to substitute for the ordinary process of malting the method of steeping the grain (barley or any other) desired to be converted into malt, in weak and dilute acids, to obtain thereby the same effect as produced by germination, and, moreover, in a far shorter period of time. It appears that, provisionally, the author has succeeded in his attempt, but is engaged in further experiments. Dilute nitric acid, containing 1 per cent of acid, yields excellent results.

Annalen der Chemie und Pharmacie, January, 1871.

This number contains the following original papers and memoirs:—

Dissociation of Caffeidine by Hydrate of Baryta.—F. Rosengarten and A. Strecker.—After first stating that, by treating caffeine some years ago with baryta, they obtained caffeidine, according to the following formula:—

$$\underbrace{C_8H_{10}N_4O_2}_{\text{Caffeine.}}+H_2O=\underbrace{C_7H_{12}N_4O}_{\text{Caffeidine.}}+CO_2;$$

while, at the same time, methylamine, ammonia, and some other not yet defined substances, were formed. The authors treat, in this paper, on the action of hydrate of baryta upon caffeidine, alluding thereby, in the first place, to Dr. Schultzen's researches on this subject, published in the *Zeitschrift für Chemie*, 1867, p. 614. It appears, from the researches made by the authors above named, that caffeidine is, by boiling with water and hydrate of baryta for forty hours continuously, slowly converted into sarkosine; and the formula of dissociation for caffeine is—

$$C_8H_{10}N_4O_2+6H_2O=2C_2O_2+2NCH_5+CH_2O_2+C_3H_7NO_2.$$

Compounds of Strychnine-Oxethyl.—Dr. R. Messel.—This paper treats on the following substances:—Sulphate of strychnine-oxethyl, a crystalline body readily soluble in water, also soluble in boiling alcohol, but separating therefrom on cooling. Hydrate of strychnine-oxethyl is obtained from the chloride of the base by treating it with excess of oxide of silver; the hydrate is precipitated by chloride of platinum, yielding a compound—

$$\begin{matrix}CH_2\\\|\\CH.N(C_{21}H_{22}NO_2)Cl\end{matrix}+PtCl_2,$$

containing 17·4 per cent of platinum.

Sulpho-Maleic Acid.—Dr. R. Messel.—This very lenghy essay contains the following sections:—Neutral sulpho-maleate of potassa, $C_4H_3K_3SO_7+H_2O$. Acid sulpho-maleate of potassa. Neutral sulpho-maleate of lead, $C_4H_3Pb_3SO_7$, containing 61·43 per cent of lead; lime, baryta, and silver salts of the acid. Products of the action of fusing caustic potassa upon the acid—viz., maleic and fumaric acids by the elimination of SO_2+H_2O, and succinic acid by elimination of sulphurous acid. Sulpho-succinic acid.

Some of Products Derived from Asparaginic Acid.—Dr. E. Schaal.—This memoir is divided into the following chapters:—Preparation of asparagine from asparaginic acids; by a lengthy and circuitous process, first the ether of asparaginic acid, and from that ether, by the action of ammonia thereupon, asparagine is obtained. Action of hydrochloric acid (the dry gas), at a high temperature, upon asparagine. Copper salts from asparaginic acid. Optical behaviour of asparaginic acid.

Pyro-Sulpho-Uvic Acid.—Dr. T. Wieland.—The formula of the hydrate of the acid alluded to, as deduced from the formula of the calcium salt, is—

$$C_3H_5\begin{cases}COOH\\SO_2OH\\COOH\end{cases}$$

the formula of the pyro-uvic acid being—

$$C_3H_6\begin{cases}COOH\\COOH\end{cases}$$

The paper, which contains a series of formulæ, winds up with a chapter on the products of decomposition of the sulpho-acids when treated by fusing caustic potassa.

Compounds of some of the Aldehydes and Amides.—Dr. L. Medicus.—This exhaustive memoir contains the following sections:—Action of oenanthal upon benzamide; action of oenanthal upon nitro-benzamide; action of essential oil of bitter almonds upon oxamide and oxamethan.

Compounds of Aceto-Piperidine.—Dr. K. Kraut.—This essay treats on—Oxyhydrate of aceto-piperidine, $C_7NH_{15}.O_8$, a substance readily soluble in water, crystallising in colourless rhombic-shaped crystals, difficultly soluble in alcohol. Compound of oxide of copper and oxyhydrate of aceto-piperidine, $Cu.2(O.C_7NH_{12}O)$, a beautifully blue-coloured crystalline body, readily soluble in water. Chloride of aceto-piperidine, $Cl.C_5H_{10}.HN.CH_2.CO.O.H$, a compound which combines with chloride of gold, forming the combination—

$$3(HCl,AuCl_3),4C_7NH_{13}O_2,$$

also a crystalline substance soluble in water. Chloride of barium aceto-piperidine, $C_{17}NH_{13}O_4,BaCl_2$.

Preparation and Composition of Hyoscyamine.—H. Hoehn and E. Reichardt.—This very lengthy and exhaustive essay is divided into the following chapters:—Preparation of hyoscyamine; hyoscyamine (this portion contains the detailed description of the properties, reactions, salts, and some of the products of decomposition of this alkaloid; the formula of hyoscyamine is stated to be $C_{30}H_{23}NO_6$); dissociation of hyoscyamine; hyoscinic acid, $C_{18}H_{10}O_6$; hyoscine, $C_{12}H_{12}N$.

Description of an Apparatus for Exhibiting the Formation of Water.—Dr. F. Wöhler.—Accompanied by a woodcut necessary to the proper understanding of the description.

NOTES AND QUERIES.

Watering the Streets with Salts.—Could you kindly inform me if Mr. Cooper has a patent for the above, and, if so, what is the date of it?—C. T.

Laughing Gas.—(Reply to "Reader.")—You must have used an aqueous solution of the gas; for, although the nitrous oxide gas may be liquefied by pressure or cold, it is impossible that it should be for sale in that state, unless in small quantity in strongly-made sealed glass tubes, since the liquefied gas boils at $-88°$, and at $+7°$ exerts a pressure of 50 atmospheres (750 lbs. to the square inch). The gas, if prepared with due care from pure materials, will have a different effect and properties from those you mention.

Tar Distilling.—(Answer to "Tar Distiller.")—It is the presence of ammonia-water amongst the tar which causes the latter to froth up and boil over in the stills, although a moderate degree of heat be applied, and even that gradually; the danger never ceases till almost all the ammonia-water has come over, and it is greater the larger the amount of this water present in the tar. The reason, then, that it is found at times that the stills will inevitably boil over, and that there is scarcely any possibility of preventing it, is that the men, in charging, have run more water in with the tar than usual. The best means of separating the two bodies (the tar and the water) is, I believe, as follows:—The tar and ammonia-water are emptied together from the barrels into one well, in which the two separate, the ammonia-water being in the upper layer of fluid, and the tar, holding a considerable amount of water in suspension, in the lower. There ought to be a tank elevated on brick-work, so high that the bottom thereof shall be level with the man-holes of the tar stills; and this tank should be sufficiently large to hold, at the least, a charge fully sufficient for the largest still, so as to preclude the necessity of re-filling. If possible, it would be most convenient to have the tank of such capacity that it would contain two or three charges of tar, but they must be full charges (*e.g.*, no fraction of a charge left). The tank is to be furnished with tap and 3-inch bore pipe, leading to the man-holes of the stills. The tar is now to be pumped from the well, by a pump whose pipe dips nearly to the bottom of the layer of tar, into the tank above, and there it should stand, at least twelve hours, if possible more, before running into the stills. By this means the water held in suspension by the tar has time to rise, and will be found, after the above-mentioned lapse of time, to be floating in a thin layer on its surface. It is impossible thus to separate all the water, but *it is* possible to separate such a portion thereof that the distillation of the tar may be rendered practicable. Of course, even after taking these precautionary measures, the greatest care is still requisite in commencing operations, and till, in fact, all the crude benzol has ceased to come off.—WATSON SMITH, F.C.S., Prestolee Alkali Works, near Manchester.

MEETINGS FOR THE WEEK.

MONDAY, 13th.—Medical, 8.
—— Geographical, 8.30.
—— London Institution, 4. Mr. R. A. Proctor, B.A., F.R.A.S., "On Astronomy."
TUESDAY, 14th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
—— Photographic, 8.
WEDNESDAY, 15th.—Society of Arts, 8.
—— Meteorological, 7.
—— London Institution, 6.30. Conversazione. Mr. Henry Holiday, on "Stained Glass Æsthetically Considered with Reference to Modern Art."
THURSDAY, 16th.—Royal, 8.30.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— Royal Society Club, 6.
—— Chemical, 8.
FRIDAY, 17th.—Royal Institution, 9. J. Norman Lockyer, F.R.S., "On the Eclipse."
SATURDAY, 18th.—Royal Institution, 3. Mr. O'Neil, "On the Spirit of the Age."

TO CORRESPONDENTS.

A. Barfoot.—We are unable to give our correspondent a formula for making soaps in imitation of Gossage's soaps.

Subscriber.—We cannot insert queries about price, &c. of ordinary chemicals; almost any wholesale house would supply the article you want.

Dr. Cameron.—The journal was at press when your letter came.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 590.

THE TAILS OF COMETS, THE SOLAR CORONA, AND THE AURORA, CONSIDERED AS ELECTRIC PHENOMENA.*

By Professor OSBORNE REYNOLDS, M.A.

Part II.

In the paper which I read before this society, on the 29th of November last, I endeavoured to show that it is probable that these phenomena are a species of that action known as the electric brush taking place in the medium which fills space, be it ether or simply gas, or both, The reasoning I made use of was, essentially *à fortiori*. I pointed to the fact that the electric brush as seen in the Geissler tubes exhibits similar appearances, and that at the times of greatest display on the part of comets and the aurora similar conditions are present, such as a change in the action of the sun, conditions which, to say nothing more, are favourable to electric disturbance. I purposely avoided all attempts to explain how the brush may be produced, feeling that it was sufficient to point to the aurora, which is universally admitted to be electrical, as a proof that such phenomena do exist even if we cannot explain how. This proof, however, is perhaps not quite satisfactory. In order that it may be complete, the other phenomena must be produced in the same way as the aurora, and this, although possible, is not necessary. An assumption which is commonly made respecting the phenomena of the aurora cannot be made with respect to the others. This assumption assigns the two magnetic poles of the earth as the the two electrodes between which the electric discharge takes place, which forms the aurora borealis and the australis. If this assumption be maintained, some other explanation must be found for the manner in which electricity may form the tails of comets and the corona. It is quite clear that the tail of a comet cannot be due to a discharge between two electrodes situated on the comet itself. In the same way, from the position occupied by the corona, it can hardly be due to electricity passing between two electrodes on the sun. In fact, if a comet's tail is electrical, it is due to a discharge of electricity of one kind or another from the comet, which, for the time, answers to one of the electrodes only. The same may be said of the corona and the sun. If we could observe the aurora from a point distant from the earth, it is very probable that we should find the same to be the case, but whether this would be so or not, an assumption has been made as to the cause and nature of the aurora, which will answer just as well for the corona and comet's tails; it is that the sun, acting by evaporation or otherwise, causes continual electric disturbance between the earth and its atmosphere, the solid earth being negatively charged and the atmosphere positively, and that the aurora is the reunion of these electricities taking place in the atmosphere.

Now, as has been already said, this assumption will serve for the comets and the sun as well as for the aurora. If there is a continual electric disturbance between the sun and the medium in which it is placed, so that the sun becomes negatively and the medium positively charged, the reunion of these electricities would form the corona. It must not be supposed that I assume the sun to be a reservoir of electricity which it is continually pouring into space. I consider that the supply of electricity in the sun is kept up by some physical action going on between the sun and the medium of space, whereby the sun becomes negatively charged, and the medium positively.

This may be well illustrated by reference to the common electrical machine; here the motion of the glass against the rubber causes the glass to become positively and the rubber negatively charged; and these electricities do not unite instantly there and then, but remain and accumulate in the respective bodies, until collected and brought together again by the conductor.

Assume, then, that the sun is in the position of the rubber, while the ether is in that of the glass: then the corona corresponds to the spark or brush which leaves the conductor. On the same assumption, the negative electricity of the comet would be more and more set free by the inductive action of the sun as the comet approached it, and would also be driven off by induction in a direction opposite to that of the sun; and combining with the positive electricity in the ether would form the tail of the comet, in a manner analogous to that in which a negative spark is given off by the lid of the electrophorus.

I think that a rational account may in this way be given of the manner of the electrical action to which I have attributed these phenomena, but I do not consider that the probability of the truth of this electrical hypothesis depends on the value of such an explanation. It is an assumption based on the manner in which it fits into its place, and explains the appearances presented by these beautiful phenomena.

Since this paper was written, my attention has been called to the fact that Mr. Richard Proctor has published views of these phenomena which somewhat resemble mine. He attributes them in part to electricity and in part to meteors. There is, however, this fundamental difference between our views, that he considers the tails of comets as consisting of cometary matter, the difficulty of conceiving which was the origin of these speculations. Moreover, I can conceive no electric discharge between two meteors without a medium, between them, and if there is a medium, why is there any necessity for meteors? If, as I see good reason to suppose, gas, when glowing with electricity, reflects or scatters rather than and absorbs light of the wave-length which it radiates, that portion of the coronal light, which is polarised and assumed to be reflected, will be accounted for. I think that the recent observations have confirmed the probability of these speculations, inasmuch as they have confirmed the facts on which these speculations were based. There is one point which has not been already noticed, but which seems to me to be of some importance.

If the corona be an electric discharge, the electricity will be continually carrying off some of the elements of the sun into space where they will be deposited and condensed. May not this stream of matter be the cause of the existence of small meteors, and supply the place of those which continually fall into the larger bodies?

* Read before the Manchester Literary and Philosophical Society, February 7th, 1871.

ON KREOSOL AND PHENOL AND THEIR HOMOLOGUES.

By PAUL SCHWEITZER, Ph.D.

In the year 1832 a body was discovered by Reichenbach, in the distilled oils of beech-wood tar, which, on account of the peculiar property it possessed of preserving meat and other highly-organised substances, was called by him kreosot. This substance attracted the attention of many chemists, who studied its properties, and sought to separate it from tar oils in general, and it was with a degree of satisfaction that two years later F. F. Runge announced his discovery of a creosote in coal-tar oil, which he called carbolic acid. Reichenbach, fearing his right of the first discovery infringed by this carbolic acid, which, according

* Read before the Lyceum of Natural History of New York.

to Runge, differed slightly from his compound, tried to demonstrate, for his own glorification, the identity of both substances; and though Laurent, in the year 1841, proved carbolic acid to be phenylic hydrate, and pronounced it a different body from creosote, Reichenbach did not give up the contest, but conducted it with a pertinacity which caused a general confusion among chemists—a confusion which became quite lamentable when, in 1855, cresylic hydrate was discovered in coal-tar by Fairlic, and which, on account of the near resemblance to true creosote, was generally accepted to be identical with the original compound obtained from beech-wood tar.

Many chemists have given their attention since then to the study of those two compounds, but carbolic acid very shortly after its discovery began to be introduced in commerce and sold as creosote. It was difficult, and at times impossible, to procure the oils made from wood-tar; and it was owing to this that the publication of some chemists are found to be at variance in their results, as they were, in fact, working with carbolic acid, while speaking of creosote. Hlasiwetz and Gorup-Besanez, among many others, have now cleared up the questions, and brought light and intelligence into the many contradictory statements which we find in the literature of creosote and carbolic acid. We know to-day that two homologues are contained in that part of the oil of wood-tar which dissolves in caustic potassa, and which bear a certain relationship to the compounds obtained from the same part of the oil obtained from coal-tar. We are justified in saying that those oils are two distinct and different fluids; for while coal-tar oil contains principally—

Phenylic hydrate, C_6H_6O, and
Cresylic hydrate, C_8H_8O;

wood-tar oil contains—

Guaiacol, $C_7H_8O_2$, and
Kreosol or Homo Guaiacol, $C_8H_{10}O_2$.

I intended saying here a few words about the place we have to assign those compounds in organic chemistry, and about their constitution, before giving the differences by which one class may be distinguished from the other. Phenylic and cresylic hydrate belong to a class of compounds the radicals of which differ by the complex CH_2. They are probably very numerous, and of those that are known I will only mention—

Phenyl, C_6H_5
Benzyl, C_7H_7
Xylyl, C_8H_9

The name of xylyl is at present somewhat obsolete, and phloryl is substituted for it. In combination with hydrogen those radicals form—

Phenylic hydride, C_6H_6 or benzol,
Cresylic hydride, C_7H_8 or toluol,
Phlorylic hydride, C_8H_{10} or phloruol (xylol);

the alcohols of them are—

Phenylic hydrate, C_6H_6 or phenol,
Cresylic hydrate, C_7H_8O or kresol,
Phlorylic hydrate, $C_8H_{10}O$ or phlorol (Würtz, xenol).

In treating these alcohols with oxidising substances, we obtain, at least in the case of phlorol, substances which contain more oxygen and less hydrogen than the alcohols, and which we call—

$C_8H_8O_2$.	$C_7H_6O_2$.	$C_6H_4O_2$.
Phloron.	Kreson.	Chinon.

I said in the case of phlorol; for, though we are able to produce the other two compounds, the first by heating chinic acid, which, in combination with lime, is a constituent of all Peruvian barks, and the second by decomposing kresol, we will, no doubt, in time arrive at a proper method for obtaining them all directly, by treating the alcohols with nascent oxygen.

In subjecting gum guaiac to destructive distillation, we obtain a fluid which, in many respects, bears a great resemblance to creosote. This distillate has been investigated especially by Völckel and Lobrero, and Hlasiwetz, who succeeded in separating it into two fluids that gave crystallisable compounds with bases, and which they called guaiacol and homo-guaiacol. A later and closer study of creosote revealed the fact that it consisted of those two identical compounds in different proportion. Let us compare them with the products we obtained from the alcohols of the phenylic series—

$C_6H_4O_2$ (chinon).	$C_7H_6O_2$ (kreson).
$C_6H_6O_2$ (hydrochinon and pyrocat.)	$C_7H_8O_2$ (guaiacol).
	$C_8H_8O_2$ (phloron).
	$C_8H_{10}O_2$ (homo-guaiacol or creosote.)

But we have also the member corresponding to the chinon in a compound called pyrocatechin, thereby completing two series of compounds, which differ only by the addition of two equivalents of hydrogen. If we treat chinon with hydrogen, we obtain directly hydrochinon; the attempt made in this direction with the others succeeded perfectly, so that we possess another series of homologues—

Hydrochinon
Hydrocreson
Hydrophloron

of exactly the same composition as—

Pyrocatechin
Guaiacol
Kreosol

but different in properties.

Now let us look at the constitution of those compounds. It is very probable, and almost admitted by Kekulé, that kresol is monomethylated phenol, and phlorol bimethylated phenol. In the same way, may we feel entitled to consider guaiacol monomethylated pyrocatechin and kreosol bimethylated pyrocatechin; in fact, in treating guaiacol with hydriodic acid, we obtain iodide of methyl and pyrocatechin, which amounts to a proof of this view, and which gives us, therefore, two links, by which we may connect the series of coal-tar oils with those of wood tar. To render it apparent to the eye, I will give the formulæ as follows:—

$C_6H_6(OH)$ (phenol.)	$C_6H_5(OH)$ (pyrocatechin).
$C_6H_4\{^{OH}_{CH_3}$ (kresol).	$C_6H_4O\{^{OH}_{CH_3}$ (guaiacol).
$C_6H_3\{^{OH}_{CH_3}_{CH_3}$ (phlorol).	$C_6H_3O\{^{OH}_{CH_3}_{CH_3}$ (kreosol).

It may be stated, as a further proof for the correctness of considering the above compounds as methylated pyrocatechin, that the molecule of methylene, CH_3, may be introduced directly into the constitution of pyrocatechin, by heating it in closed tubes with caustic potassa and methyl-sulphate of potassa, producing thereby guaiacol.

The homologues of the phenylic series are crystallisable compounds, of the respective boiling-points of 184° C., 203° C., 220° C., while the two derivatives of pyrocatechin are oily liquids boiling at a temperature of 200° C. and 219° C.

Phenol,	184° C.	Pyrocatechin	—
Kresol,	203° C.	Guaiacol,	200° C.
Phlorol,	220° C.	Kreosol,	219° C.

It may be seen, therefore, that in cases of working with a mixture of the individuals of both series, we should find it impossible to separate them by fractional distillation. Their products of decomposition, however, may be resorted to as a means of distinguishing between them. For, while phenol and its series yields, with nitric acid, nitrophenol and similar compounds, we obtain with guaiacol oxalic acid; chlorine converts phenol into chlorophenic acid and choranil, while the products obtained in this way from creosote, though similar to them, present differences which stamp them as peculiar and distinct bodies.

THE TOWNSHEND JEWELS.

By A. H. CHURCH, M.A.

Much scientific as well as artistic interest centres about many of the minerals which have been used for purposes of ornament. The study of the intruding substances and the cavities often occurring in quartz, ruby, and topaz, has led to the discovery of many singular facts, and has partially lifted the veil which shrouds from curious eyes the origin of some of the most beautiful products of the earth. So also the crystalline forms and optical properties of precious stones, together with the action of heat and other forces upon them, have afforded ample and profitable materials of research for the mathematician and the physicist. Thus it would be easy to vindicate the claims on scientific consideration which jewels possess. Were we to travel for a moment out of the path which we intend to follow in the present notice, and to speak of the artistic qualities of precious stones, we should have a rather difficult task to accomplish, for, strange to say, it has become usual, amongst a certain clique of artists and connoisseurs, to depreciate their beauty. Professor Ruskin*, for example, talks of the colours of gems as "entirely common and vulgar;" he calls the green of the emerald as "vulgar as house-painting," and states (what is absolutely incorrect) that "no diamond shows colour so pure as a dewdrop." "The ruby," he adds, "is like the pink of an ill-dyed and half-washed-out print compared to the dianthus." Other writers on art, ignorant of those wondrous properties of jewels which are developed only by judicious cutting, would not allow a specimen to be facetted, but simply rounded and polished in the way known as *en cabochon*. Our present purpose, however, is not to offer a logical justification of the fondness for precious stones, which most people exhibit to some degree, but to direct the attention of our readers to the superb suite of specimens which has been recently bequeathed to the nation by the late Rev. C. H. Townshend. The collection is to be seen in the South Kensington Museum, and is, or was recently, in one of the picture galleries. A catalogue† of the specimens has been published under the auspices of the Science and Art Department, but, unfortunately, it is in many points an inadequate production; for ease of reference it will, however, be expedient to follow the order in which the specimens are therein described.

A fine large crystal of diamond (No. 1172) affords an example of the peculiarly brilliant lustre which distinguishes even the natural surfaces of this stone, and permits anyone who appreciates its character to single out a rough diamond from all other stones. The specific gravity of the diamond (3·53) does not enable us to distinguish it from glass or white topaz with certainty, though it separates it from white quartz, which is only 2·65, and white beryl, which is only 2·71. There is a black diamond (No. 1173) in the collection, which displays on its facets the characteristic lustre of the stone. A colourless diamond of pure water, and about 5-12th of an inch across, is a splended brilliant from the Hope collection. A lustrous yellow diamond of the same size should also be noticed. Of other coloured diamonds there are two green specimens, one of a pale blue hue, one of a curious and very rare puce tint.

It will not be necessary to dwell upon the peculiarities of any of the many forms of silica represented in the collection. Almost all the ordinary and most of the rare varieties may be here seen mounted in rings, and, in some cases, set off by borders of diamonds. One amethyst (1187) is remarkable for containing several cavities partially filled with fluid. Some of the translucent specimens known as *cat's-eye*, of a yellow or brown colour, are good examples of the peculiar "chatoyant" effect produced by regularly disposed asbestos filaments penetrating the quartz. Several of the specimens catalogued under the heading Silica belong, however, to far more *récherché* species. One, for instance (No. 1188), is probably a pink topaz, and not an amethyst; another, called burnt quartz (No. 1194), appears to be a splended chrysoberyl, the dispersive power of the stone being far too high for silica. Burnt quartz is the name given (after strong heating) to a dark variety of rock crystal from Portugal and Brazil, which assumes, after having been submitted to a high temperature, a sherry-brown or red colour.

Of opals there are more than a dozen in the Townshend collection; many of them exhibiting the most superb play of prismatic colours. One is pale grey, and shows a fine blue iridescence.

The sapphires and rubies include a series of fine specimens of the highly-prized deep velvet blue, and the pigeons'-blood red colours. There are also some violet and salmon-coloured corundums of great rarity and beauty. A golden-yellow one, of great brilliancy (No. 1312) is included with the topazes. The star-rubies and star-stones are well represented. These stones somewhat resemble the cat's-eye, but they show a six-rayed star, which is best seen when the gem, cut across its axis, and left with its top *en cabochon*, or rounded, is principal viewed in sunlight or in the focus of a condensing lens. In the case of these star-stones, the internal reflection is due, not to intruding substances, but the mode in which the crystal has been built up of symmetrically disposed but not perfectly regular layers.

The turquoise comes next in order. The true turquoise is a phosphate and hydrate of aluminium coloured by a little phosphate of copper. The false, or bone turquoise, is merely fossil ivory tinted with phosphate of iron, not phosphate of copper, as stated in the catalogue,

Several of the most beautiful varieties of garnet are seen to perfection in the Townshend series. Two of the specimens (1306, 1307) are wrongly labelled "jacynth," included under the heading zircon in the catalogue. One of the so-called garnets, of an amethystine colour, seems likely to belong to another species.

A fine inscribed emerald, from the celebrated collection of the late H. P. Hope, is the first beryl in the list. It is of rich colour, and nearly ⅝ inch by ¾ inch in dimensions. Another stone (No. 1284), though smaller, is a still finer specimen, faultless in cutting, shape, and colour, and almost half an inch across. The aquamarines in the collection are of immense size and rich hues. One of them presents almost the exact tint of the more valuable blue topaz.

Nine specimens are catalogued as chrysolites, but at least four of these are incorrectly assigned to this species, Nos. 1297 and 1304 are chrysoberyls, while 1304 is a zircon of the variety known as jargoon.

The white topaz is not well represented in the collection, and there is great confusion amongst the coloured varieties of this stone. No. 1309 is much more like a spinel than a topaz, while No. 1312 is a splended yellow sapphire. No. 1318 seems only quartz.

Of the five stones labelled as tourmalines, one (No. 1322) is certainly a jargoon. The green tourmaline (Nos. 1321, 1323) is a beautiful but not brilliant stone. A peculiar interest attaches to this species, owing to its optical characters. It is a striking instance of dichroism, the rich grass-green specimens being perfectly opaque when viewed along instead of across the principle axis of the crystal.

Among the spinels is one of an indigo-blue colour (No. 1325), and two fine pink-coloured specimens.

There is a splended suite of chrysoberyls, although some of them are incorrectly included under quartz and chrysolite. The precious cat's-eye, or cymophane, is a variety of chrysoberyl. Its opalescence is due to its intimate structure, and only appears when the light is incident at a particular angle. The line of light should be straight and clear as silver wire, and shows to the best advantage when the stone is of a clear green tint.

We have not space to record the peculiarities of the remaining species represented in the Townshend gift.

* Ruskin's Lectures on Art, pp. 176 and 177. (1870).

† Catalogue of Gems and Precious Stones (Townshend Bequest), by J. Tennant. 1870.

There are moonstones and sunstones, with examples of labradorite, kyanite, and pyrite, all gorgeously mounted in gold coronet rings, and in many instances set off by rows of diamonds. We have merely noticed the existence of this very interesting and valuable collection in the hope that it may receive that attention at the hands both of artists and mineralogists which it deserves. The names assigned to about twenty of the specimens should be revised, and then we should possess in London three fine suites of authentic precious stones, from which their characteristics might be readily learnt. The other two public collections are in the British Museum and in the Geological Museum of Jermyn Street. It is greatly to be desired that the specific gravity, hardness, crystalline system, and refractive and dispersive power should be noted on the label of each specimen.

PRELIMINARY NOTICE ON THE

PRODUCTION OF THE OLEFINES FROM PARAFFIN BY DISTILLATION UNDER PRESSURE.*

By T. E. THORPE, Ph.D.,
Professor of Chemistry in Anderson's University, Glasgow,
and JOHN YOUNG.

WHEN paraffin is exposed to a high temperature in a closed vessel, it is almost completely resolved, with the evolution of but little gas, into hydrocarbons which remain liquid at the ordinary temperature.

This reaction will undoubtedly afford the most important insight into the constitution of this body.

Accordingly we have repeated this conversion on a large scale, and from about 3½ kilogrms. of paraffin melting at 44·5° C. (prepared from shale), we have obtained nearly 4 litres of liquid hydrocarbons. This mixture of hydrocarbons commences to boil at about 18° C., but the quantity coming over below 100° C. is comparatively small; by far the greater portion boils between 200° and 300°. A preliminary separation shows that the 4 litres are made up of hydrocarbons boiling—

	Litres.
Between 200° and 300°	2·7
,, 100° and 200°	1·0
Below 100°	0·3
	4·0

Up to the present time, we have principally occupied ourselves with the investigation of the fraction boiling below 100°, and have obtained conclusive evidence that it is mainly composed of olefines, the proportion of members of the $C_nH_{2n}+2$ series being but small. By repeated fractionations over sodium, we obtained perfectly colourless liquids, boiling at about 35° and 65°, which were attacked by bromine in the cold with the greatest energy. On adding the bromine slowly, and in minute drops, and carefully cooling the hydrocarbon by a mixture of snow and salt, scarcely a trace of hydrobromic acid was produced. The portion boiling at 36° may be either amyl hydride or amylene, or a mixture of both; the avidity with which the bromine combines with it shows that the latter body must be present in considerable quantity. As soon as the drops of bromine permanently coloured the liquid it was submitted to distillation. Only a small portion came over below 40°; the thermometer rose rapidly to 180°, and nearly the whole of the bromine-compound distilled at 184° to 188°. This substance is amylene bromide, $C_5H_{10}Br_2$; Wurtz gives the boiling-point of this body at about 180°. The portion, therefore, boiling at 35° is mainly amylene.

Exactly similar results were obtained from the portion boiling at 65° to 70°. This, from its boiling-point, may be either C_6H_{12} or C_6H_{14}, or a mixture of both. Bromine disappears instantly on adding it to the carefully-cooled liquid, and on distillation by far the greater portion is found to have combined with the halogen. The bromide thus obtained distils, with slight decomposition, at about 195°. Pelouze and Cahours found that hexylene bromide, $C_6H_{12}Br_2$, boiled at 192° to 198°.

We are at present engaged in the further investigation of this subject, and hope shortly to lay our results before the Royal Society.

* Read before the Royal Society, March 9th, 1871.

FURTHER EXPERIMENTS ON THE EFFECTS OF COLD UPON CAST-IRON.*

By PETER SPENCE, F.C.S., &c.

IN resuming these experiments upon the effects of cold on cast-iron, it is not necessary for me to say that I was led to resume them from the apparent undecisiveness of all the experiments brought before the Society some time ago, my own being included in that category, none of them being so free from possible sources of error as to be fitted for finally settling the matter.

In the experiments which I have now to bring before the Society I have limited my aim to a single point, namely, as to whether the reduction of temperature has any, and if so, what, effect on cast-iron in regard to its powers of resisting transverse strain either of weight or pressure, and it appears to me that if this point can be satisfactorily settled it will go a long way in settling the other points now in dispute.

As my object, in showing that I have in these experiments eliminated, as far as seems possible, all sources of error, will be best effected by minute detail, you will excuse anything that may seem trifling. As I was not trying the absolute strength of any sort of cast-iron I did not see the force of Mr. Brockbank's objection to my using ½-inch bars instead of the orthodox 1-inch bars. I could obtain ½-inch bars equally good castings, and the machinery for breaking them was more manageable and in my opinion more exact.

Messrs. Rye, Son, and Ogden, of Newton Heath, kindly undertook to make for me fifty bars, each 3 feet long by ½-inch square, all out of one ladle, and of No. 3 Glengarnock pig and Kirkless Hall common pig—I name these although it does not seem of importance; all I wanted was good, sound, clean, and equal castings; and knowing the purpose for which they were intended, with great care they turned them out so good that not one of those sent to me was rejected. I now cut each of these bars into three lengths of 1 foot each, and as they were cut they were thrown into a heap, making nearly 150 pieces. They were now taken and all their ends covered with paint, in order that the new fracture might be examined as they were broken. The heap was then brought into the laboratory, having thus had three chances of perfect mixing. A boy of eleven years of age now handed me the pieces singly from the heap, and as I received them I placed them alternately one by one in two lots, until I had got seventy pieces in each lot. One of these was now taken and put into a cask capable of holding 2 cwts. to 3 cwts. of freezing mixture composed of pounded ice and chloride of sodium (which instantly reduces the temperature to zero), and being surrounded with sawdust, they were kept there for nearly forty-eight hours.

The other seventy were now put into water at 70° F. and this was done chiefly in order that they might be broken wet, as those would necessarily be when taken out of the freezing mixture.

The mode of breaking was this:—I put a bar on the suspending wedges, then hooked on the weight scale, and with a number of weights much under the breaking load, raised the loose end of the plank by the screw-jack so as to bring the weights to bear. I now added single pounds

* Read before the Manchester Literary and Philosophical Society, February 7th, 1871.

or 2-lb. weights till 15 lbs. were put on; these were then taken off and a 14-lb. weight was placed and single pounds again put on, thus regularly adding till the bar snapped; I then recorded the breaking weight, my assistant meantime putting on another bar. I spent nearly eight hours in breaking these seventy bars, and every one got an equal amount of care.

On opening up the freezing mixture forty-four hours after enclosing it, I found it in perfect condition, little solution and no increase of temperature having taken place. The bars were taken into the laboratory in small lots and immersed in another freezing mixture, from which they were withdrawn singly with pliers. Having seized one piece with too firm a grasp I found that my fingers grew white and produced an intense pain as if burned. Some of the freezing mixture was spread on each bar by a spatula while on the machine, so that every one was broken at a temperature within one or two degrees of zero.

The mode of breaking was exactly similar to that employed with the other lot, and equal care was given to every bar. This I can affirm, as every one of them was broken by myself, and all entries made by myself.

The results are before you, and to me it was a matter of surprise, when both sets were completed and added up, to find that they almost exactly corroborated my previous experiments, which I do not think were fallacious in their character, but merely defective in their not covering a sufficient amount of ground to give certainty to the result. I have, however, so much confidence in those now detailed that I have no hesitation in giving it as an ascertained law, that a specimen of cast-iron having at 70° F. a given power of resistance to transverse strain, will on its temperature being reduced to zero, have that power increased by 3 per cent.

Breaking-Weight of ½-inch Square Cast-Iron Bars, 9 inches between Points of Suspension at 70° F.

	Cwts.	Qrs.	Lbs.		Cwts.	Qrs.	Lbs.
				Brt. forward	129	2	21
No. 1.	4	0	14	No. 36.	3	3	14
2.	4	3	26	37.	4	2	24
3.	3	2	2	38.	4	1	1
4.	3	0	14	39.	4	1	14
5.	3	3	16	40.	3	3	12
6.	3	2	14	41.	4	0	14
7.	3	2	10	42.	3	0	14
8.	3	0	0	43.	4	0	6
9.	3	3	0	44.	3	2	1
10.	3	1	1	45.	4	0	0
11.	3	1	14	46.	4	0	27
12.	3	1	14	47.	4	0	22
13.	3	1	24	48.	3	0	2
14.	3	0	14	49.	3	3	14
15.	3	3	0	50.	4	1	8
16.	2	3	14	51.	4	1	15
17.	4	2	8	52.	4	0	24
18.	4	1	1	53.	3	0	5
19.	3	1	0	54.	3	2	27
20.	3	3	20	55.	4	3	0
21.	4	1	0	56.	3	2	4
22.	3	0	12	57.	4	0	12
23.	4	1	0	58.	4	0	14
24.	3	3	14	59.	4	0	0
25.	4	0	0	60.	4	1	1
26.	3	2	14	61.	4	0	18
27.	3	1	18	62.	4	0	12
28.	3	2	22	63.	4	0	11
29.	4	0	14	64.	4	0	4
30.	3	3	1	65.	3	3	10
31.	4	0	26	66.	3	1	18
32.	3	3	8	67.	3	1	7
33.	4	2	7	68.	3	2	6
34.	2	2	14	69.	4	3	0
35.	3	2	1	70.	4	1	0
	129	2	21		268	3	18

Breaking-Weight of ½-inch Square Cast-Iron Bars 9 inches between Points of Suspension at Zero.

	Cwts.	Qrs.	Lbs.		Cwts.	Qrs.	Lbs.
				Brt. forward	139	1	22
No. 1.	4	1	15	No. 36.	3	2	35
2.	4	0	14	37.	5	0	14
3.	3	0	10	38.	4	0	4
4.	3	0	6	39.	3	3	4
5.	2	3	20	40.	4	1	15
6.	3	3	18	41.	3	0	12
7.	3	1	12	42.	3	1	0
8.	4	2	14	43.	4	2	8
9.	4	0	22	44.	3	3	22
10.	4	1	15	45.	3	2	0
11.	4	0	14	46.	4	2	1
12.	4	2	1	47.	4	1	1
13.	3	1	26	48.	4	0	4
14.	4	0	4	49.	4	0	3
15.	3	2	8	50.	4	2	14
16.	4	3	0	51.	4	0	12
17.	4	1	15	52.	3	0	18
18.	3	2	1	53.	3	2	8
19.	4	1	15	54.	4	0	15
20.	4	2	1	55.	4	1	12
21.	4	2	24	56.	3	1	13
22.	3	2	26	57.	4	1	26
23.	4	1	1	58.	2	3	10
24.	4	1	26	59.	4	1	26
25.	3	3	12	60.	3	2	20
26.	3	0	14	61.	4	0	0
27.	4	1	15	62.	3	2	24
28.	3	0	12	63.	3	3	15
29.	4	2	14	64.	3	0	20
30.	3	2	15	65.	4	0	14
31.	3	0	22	66.	4	0	4
32.	4	3	13	67.	3	0	20
33.	4	1	14	68.	3	2	12
34.	4	0	13	69.	4	2	1
35.	3	2	18	70.	4	0	1
	139	1	22		276	3	0

	Cwts.	Qrs.	Lbs.
At 70° F.	268	3	18
At zero.	276	3	0

Mr. Thomas Carrick called attention to the fact that the tabulated statement of Mr. Spence's experiments showed a maximum breaking weight of about 5 cwts. and a minimum of about 3 cwts. The minimum breaking-weight was therefore 40 per cent less than the maximum. With experiments showing such an excessive range in the breaking-weight of bars, which from the care taken in their production ought presumably to have been homogeneous in quality, it was very unsafe to rely upon a resulting difference of only 3 per cent derived from separately adding the breaking-weights of each set together and comparing the gross results. The iron used was obviously of an inferior quality and quite unsuitable for the purpose of reliable experiments.

ON THE PRIORITY OF THE DISCOVERY OF ANILINE COLOURS.

Having been favoured by Dr. A. Bauer with a copy of a lengthy paper on the above subject, which was published in the *Neue Freie Press* of Vienna of the 3rd of January last, we abstract from that paper the following. It has been written in reply to an article which appeared in the same newspaper on the 6th of December last, wherein it was said that the late Dr. Jasznüger, formerly Professor of Chemistry at the Imperial-Royal (Austro-Hungarian) Theresia University, was the

first and real discoverer of all those colours which modern chemistry has produced from aniline, naphthaline, anthracen, and phenol, while these discoveries were stated to have been made as early as the year 1816; and, as the reason why Dr. Jasznüger had never divulged his knowledge, nor published the secrets of his preparations, it was said that he had been compelled to do so by the machinations of his rivals and enemies, Drs. Prechtl and Jaquin of the same University.

Dr. Bauer reviews and scrutinises at length the documents purporting to prove the correctness of the above statement, as published in the newspaper above named in December last. As a *resumé* of the entire controversy, we may here state that Dr. Bauer conclusively and clearly proves that the colours obtained by Dr. Jasznügel from coal and peat (the latter being, as is well known, a material altogether unfit to obtain the so-called aniline colours), are nothing like the coal-tar colours, but are—(1) A peculiar black, which, according to independent testimony, appears to have been akin to lampblack. (2) Dr. Jasznügel stated himself that he had been successful in obtaining, from coals (not coal-tar) and peat, such dye materials as would render the use of certain astringent substances, and other exotic materials then (1814—1817) used in dyeing, unnecessary, in consequence of the possibility of obtaining these substances artificially. It is further clearly and impartially proved by Dr. Bauer that it was almost impossible to expect that, considering the comparatively very crude state of chemistry nearly sixty years ago, either Dr. Jasznügel, or anybody else, should at that time have discovered coal-tar colours; for even if, by mere accident, he had come across a reaction while operating on coal-tar, which reaction exhibited a peculiar colouration, the *rationale* thereof and further investigation thereon would have been out of the question. Dr. Bauer's object, in writing a lengthy paper on this topic, was to prove the groundlessness of Jasznügel's claim (never, however, made by himself), but lately brought forward for the purpose of showing that, through the animosity of Drs. Prechtl and Jaquin (who, by the bye, are fully exonerated and exculpated from any such malignity), Austria had lost the honour of being the country wherein the discovery of aniline colours was really made.

IMPROVED PROCESS OF EXTRACTING SILVER AND GOLD FROM ARSENIO-SULPHURETS OF LEAD, COPPER, &c.

By C. WIDEMANN.

This very interesting and entirely new process was discovered and has been patented in all mining countries by Cyprien Marie Tessié du Motay, of Paris, France, and is already practised on a large scale at the metallurgic establishment of Coumnies, France. The process consists in the following series of docimastic treatments of the simple sulphurets, the complex sulphurets, and the arsenious sulphurets of lead, antimony, copper, and iron, as well as coppery matts and coppers containing precious metals, in order to extract silver and gold therefrom at a great saving in time and labour.

First. In roasting the simple or complex sulphurets, the antimonial sulphurets, and the arsenio-sulphurets containing silver or gold, in the presence of pure silicates, of auriferous quartz or earthy and metallic silicates, adding, in order to complete this roasting and to expel all the sulphur contained in the minerals, either lead, which is intended to form oxide of lead, or litharge, or any other metallic oxide capable of producing, in contact with air or oxidising flames, peroxides or silicates of peroxides.

Second. In thus transforming into the state of very fusible basic silicates, the oxides of the desulphuretted metals.

Third. In melting or running in the melted state the silicates of this kind produced upon a matt of lead also melted, and in stirring or agitating them, either by paddles held in the hand or by mechanical means, or by means of gases mechanically employed, up to the moment when the gold and silver are entirely dissolved in the melted lead.

Fourth. In separating the poor scoria deprived of the precious metals of the lead, which has taken them up, and in stiring or agitating upon the same mass of lead a fresh quantity of rich scoria.

Fifth. In repeating this liquation an indefinite number of times, until the moment when the capacity of saturation of the lead for the precious metals, which is lessened by each operation, no longer permits the continuance of this mode of treatment.

Sixth. In testing the lead saturated with silver or gold, by the methods of cupellation now in use, in order to extract therefrom the precious metals.

Seventh. In removing the poor scoria, the oxides of lead, antimony, and copper, which, for the most part, are contained therein, by bringing back these oxides to the metallic state by the separate or united action of charcoal and iron.

Eighth. In separating, by the special methods presently described, the copper and antimony from the lead with which they are united.

Ninth. In re-employing, either wholly or partly, the purified lead in the treatment, by oxidation and silication, of fresh quantities of minerals.

Tenth. In reducing, either in reverberatory or cupola furnaces, or in all other melting furnaces, the basic silicates enumerated above by the action of charcoal or iron, when the latter are the produce of minerals containing copper pyrites in considerable quantities.

The metallic matter thus obtained consists of an alloy of copper, lead, silver, and gold, free from sulphur, which may be all treated at once, either by docimastic methods in countries rich in combustibles, or by the humid process in countries where mineral acids are low in price. Having thus stated, in a general manner, M. du Motay's docimastic method, let us describe the new operations and reactions whereby they are essentially distinguished from those at present employed for the same object. These operations and reactions consist, in the first place, of the roasting, the oxidation, and the scorification of the simple or complex sulphurets of antimonio-sulphurets and arsenio-sulphurets in the presence of silica and oxides of lead, or of all other metallic oxides capable of passing into the state of peroxides or of silicate of peroxides in the oxidising flames, in order—

1. To prevent the formation of the oxysulphurets of lead, antimony, and copper, which afterward, in dissolving with the silver or gold in the lead, cause the loss, during the cupellation of an appreciable quantity of the precious metals, which are dissolved in this lead, and render inapplicable for industrial purposes either the cupellated litharges or the lead proceeding by reduction from the said litharges.

2. To prevent the complete dissolution in the lead of the silver and gold contained in the basic silicates free from sulphur, without the antimony or copper, which have passed from the state of sulphurets into the state of silicates, being able to dissolve in the lead, and consequently to injure it.

3. To obtain as the final result of a series of liquations, a testable (cupellable) lead, rich in precious metals.

4. To avoid a great number of cupellations to obtain pure litharges transformable into lead of an equal purity, and to thus diminish the net cost of the industrial extraction of silver and gold, by means of the cupola, to a considerable extent.

The operations and reactions, in the second place, consist in the reduction to pure lead, or into an alloy of lead and antimony, and copper of the scoria deprived of gold or silver, in order—

1. If the extracted lead is pure, to make it again serve by way of roasting, for the oxidation of a fresh supply of sulphuretted, arsenio-sulphuretted, or antimonio-sulphuretted minerals.

2. If the lead is alloyed with antimony or copper, or with both these metals, to separate it from one of these metals or both together, in order also to use it again, after purification, for the oxidation of a fresh quantity of sulphuretted, arsenio-sulphuretted, or antimonio-sulphuretted materials.

The method for separating the lead from the antimony, or from the antimony and copper united, constitutes one of the greatest novelties and one of the most essential parts of the invention; taking, for example, an alloy of the lead and antimony, the inventor submits it, after having melted it in a reverberatory or a cupola furnace, to the action of the nascent steam produced by aërohydric or oxyhydric blowpipes, fed by a mixture in definite proportions of air, or of oxygen and pure hydrogen, or carbonated hydrogen. The flame procured by these blowpipes should be as free as possible from any trace of uncombined oxygen. It is even preferred, in order to prevent any oxidation of the lead, that the flame should contain a little hydrogen or free carbon. The nascent steam may, perhaps, be mixed with the ordinary steam generated at 212° Fahrenheit. The antimony, which decomposes water at a high temperature, oxidises in passing into the state of antimoniate of oxide of antimony, one portion of which volatilizes while the other remains attached to the inner sides of the reverberatory or other furnace, in this operation, during which almost the whole of the antimony is separated, and the lead, which does not decompose water, is but little, or not at all oxidised, but still retains traces of antimony with a strong affinitive force, and in order to entirely purge it from this metal, it is necessary to have recourse to sulphate of lead. This new reactive has the property of becoming decomposed in oxide of lead, into free oxygen, and into sulphurous acid by antimony, which, by this decomposition, taking up the oxygen set at liberty, oxidises and combines with the oxide of lead of the decomposed sulphate. The lead is then completely purified and rendered applicable for all industrial purposes. It follows, as a matter of course, that the sulphate of lead can, by itself, without the previous employment of steam in the nascent state, separate the whole of the antimony contained in an alloy of this metal with the lead, but, in many cases, to employ it would cost too much. Earthy hydrates, such as hydrates of lime and baryta, can also be effectually employed instead of steam in a nascent state, or ordinary steam from water, in order to oxidise the antimony and to separate it from the lead; but the inventor prefers the steam generated by aërohydric or oxyhydric blowpipes, which furnish at the same time the heat necessary for the fusion of the alloys and the reactive, which partly or wholly removes from the lead the antimony which injures it. When the lead is alloyed with copper, and this alloy does not contain antimony, it is melted with a quantity of sulphuret of lead rather more than equivalent to the quantity of copper, to transform into sulphuret; this fusion is conducted away from any oxidising action in a reducing medium, and produces an immediate reaction by way of double exchange. The sulphur combined with the lead acts upon the copper of the alloy, aud thus substitutes metallic lead for the copper, which scorifies and passes into the state of sulphuret. The lead thus deprived of copper, is pure and proper to be employed, as indicated above, either for the oxidation of fresh mineral, or it may be sold for general purposes. Alkaline and alkaline earths, bisulphurets, and polysulphurets, as well as metallic sesquisulphurets and bisulphurets, may be substituted for the sulphuret of lead, in order to remove the copper from the lead by causing the latter metal to pass into the state of sulphuret. The sulphuret of copper may, after roasting, be transformed into metallic copper by the reducing process at present employed. When antimonio-sulphuretted argentiferous leads have been previously brought to the metallic state by the method of reduction at present employed, the inventor submits them to the action of the same blowpipes, and when the antimony, which has passed into the state of oxide, has been separated from the lead, in which the silver remains entirely dissolved, this lead is tested or cupellated, and the precious metal extracted therefrom. The chief points of novelty in this invention may be stated as follows:—

1. The method of roasting and the silicatisation of the metallic, simple, or complex sulphurets, arsenio-sulphurets, and antimonio-sulphurets containing silver or gold, by means of the processes above described.

2. The method of liquation and fusion by successive series of argentiferous and auriferous silicates upon the same bath of lead, which deargentises and deaurifies them, by which method the same object is obtained by one cupellation which according to the best methods at present in use requires several.

3. The method by which, after the oxides of lead, antimony, or copper contained in the silicates deprived of silver and gold have been brought back to the metallic state, the lead is separated from the copper with which it is united by the addition of an equivalent quantity of sulphuret of lead, or of one of the sulphurets enumerated above, and the antimony, by the action of the aërohydric or oxyhydric blowpipes, combined with the reaction produced by a small amount of sulphuret of lead.

4. The application of the above-described methods to the entire or partial treatment of copper matts and argentiferous and auriferous coppers of the minerals of antimoniferous lead, and of the mineral of antimony containing silver and lead or pure silver.

We have no doubt this process will be accepted all over the world, as the series of manipulations are thus greatly simplified, as already oxygen is manufactured on a large scale at a reduced price, and so will be pure hydrogen in a very short time; these two gases used in a blowpipe will satisfy in a practical way all the exigencies of the gold and silver refiners and smelters.—*Journal of Applied Chemistry*.

FURTHER EXPERIMENTS ON THE EFFECT OF DIET AND EXERCISE ON THE ELIMINATION OF NITROGEN.*

By E. A. PARKES, M.D., F.R.S.

This lengthy and very interesting paper contains the account of a series of physiological experiments very carefully instituted by the author, assisted, first by Count Wollowicz, and afterwards by Serjeant Turner, of the Army Hospital Corps. The subject of the experiments was a perfectly healthy soldier 25 years of age, who weighed 145 lbs., and was a very powerful man, having been an ironworker (puddler) before enlistment; the series of experiments made are: first, ordinary regulated diet, including the careful determination of the amount of excretions, and the determination of the nitrogen therein contained, also the mean pulse and mean temperature of axilla and rectum. The second series of experiments with prepared concentrated food was discontinued in a short time, owing to the food causing indigestion. The third series of experiments with non-nitrogenous food was conducted in all particulars as the first series. As general results the author points out that, with regard to the temperature, it may be concluded that a non-nitrogenous diet continued for five days neither raised nor lowered the temperature of the axilla and rectum; it also appears that when the nitrogenous food of a healthy man was reduced to one-half for five days, and he was then kept for five days without nitrogen,

* Abstract of a paper read before the Royal Society.

he was able, on the fourth day after such deprivation, to do a very hard day's work. The non-nitrogenous food, consisting of butter, oil, starch, and sugar, kept him perfectly well, all the functions seemed natural, the man felt in all respects healthy, but on account, however, of the feebleness of the heart's action, it was not thought proper to continue the experiments, which, according to Dr. Parkes's opinion, had sufficiently proved that force necessary for greater muscular work can be obtained by the muscles from fat and starch, though changes in the nitrogenous constituents of the muscles also go on which have as one effect an increased, though not excessive, elimination of nitrogen after the cessation of the work.

ON THE CONGELATION OF BISULPHIDE OF CARBON.

By N. V. WARTHA.

The congelation of bisulphide of carbon, which, according to the treatises on chemistry, requires a temperature of −90° for its solidification, may be easily effected by directing a very rapid current of dry air upon the surface of the pure liquid (purified by an amalgam of silver) contained in a glass vessel.

If a thermometer be plunged into the bisulphide of carbon during this operation, a snowy crust will be noticed covering the sides of the vessel and the thermometer, even before the temperature has become 0°. The temperature then rapidly decends to −18° and a white mammillated mass rises to the surface, and sometimes even stops up the tube for conducting the air. Soon all the liquid disappears and the thermometer commences to rise again up to −12°, where it remains stationary as long as the bisulphide of carbon is solid. In this state it presents the same phenomena as solid carbonic acid.

The bisulphide of carbon will remain solid for some time, and in this state it possesses a peculiar aromatic odour. Its formation may be utilised for the production of ice, thus: add to some water contained in a capsule, a few cubic centimetres of bisulphide of carbon, and bring a rapid current of air to play upon it. The water will soon solidify, just as the bisulphide of carbon itself, provided the latter is present in sufficient quantity; the temperature of the mixture may then reach −15°.

Bisulphide of carbon cannot be solidified *in vacuo*, except it be mixed with ether.

The temperature above cited are in degrees centigrade. —*Deutsche Chemische Gesellschaft*, 1870, No. 2.

NOTICES OF BOOKS.

Lessons in Elementary Physics. By Balfour Stewart, LL.D., F.R.S. Macmillan and Co.

Manuals in which theory is so far illustrated by practical application as to convey clear ideas to the student are very rare, and the advent of such a work during one's studentship is to be fully appreciated by those only who have little assistance other than that afforded by their text-books. Clearness of definition, too, is not often the characteristic of an elementary work, and when, as in physical science, definitions are so various and terms so arbitrarily used, any guide that puts before the student a comprehensive and yet sufficiently full exposition of his subject, is to be commended in the highest degree. It may be expected that from the pen of Dr. Balfour Stewart there should emanate such a work, yet it is not always the case that men who are proficients in any subject are also calculated to impart the best elementary instruction in that subject. When a fact is known, the possessor almost naturally comes to the conclusion that it must be equally well known to others. This predisposition to accredit others with the knowledge they do not possess perhaps accounts for the occurrence that so many works written by able men are useless to the mind endeavouring to grasp first principles. Indeed, many so-called elementary books serve better as *aide memoires* to the man who has "passed" than as guides to the student preparing for matriculation. All who have with great labour sifted the seed from the many manuals heretofore afloat. will envy those now commencing their studies to whom is known the admirable little text-book entitled "Lessons in Elementary Physics."

Explaining, as it does, in a few hundred octavo pages, the most important of those laws which regulate the phenomena of nature, its descriptions are quite full enough to obviate in many cases reference to more extensive authorities, while the explanations of the various delicate philosophical instruments and their principles might, with advantage, be emulated by more pretentious works.

The difference between actual or kinetic and potential energy is here laid down with a distinctness that must carry understanding with it. After classifying the various forms of energy, Dr. Stewart goes on to consider the laws according to which they are transmitted into one another, giving in a concise chapter the principles of conservation of energy. The illustrative experiments in sound, heat, light, electricity, and magnetism, through all of which the various forms of energy are traced, are really of practical worth, especially the explanation of the polarisation of light. Considering a ray of sunlight as consisting of an impartial mixture of horizontal and vertical vibrations or waves, all taking place at right angles to the direction of propagation, the author goes on to explain "that there are certain substances which admit of the progress through them of a ray of light of which the vibrations all take place in one plane, while, however, they stop nearly all rays consisting of vibrations in a plane at right angles to the first," giving in illustration a string vibrating freely with a vertical undulation between two parallel vertical plates, but being prevented vibrating vertically beween two horizontal plates. Finally, in his concluding remarks, Dr. Stewart shows that with the exception of tidal energy, all the work done in the world is due to the sun, and stating that, though it is larger and hotter than any fire, yet there is no apparent reason why it should form an exception to the fate of all fires—to die out—its only difference being one of size and time. Without doubt this latest production of Dr. Stewart will become the text-book of the day.

Dr. Dobell's Reports on the Progress of Practical and Scientific Medicine in Different Parts of the World. Contributed by numerous and distinguished coadjutors. Vol. II., for the year 1870 (from June, 1869 to June, 1870). London: Longmans, Green, and Co., 1871.

It is with great pleasure that we have perused this second volume of a valuable work, for which the thanks of the medical faculty are due to its eminent compiler and editor, Dr. Dobell.

The volume opens with a report from France, from the hand of Professor Villemin, and we agree with Dr. Dobell that special thanks are due to Professor Villemin for having punctually performed his task, notwithstanding that he was at the head of a hospital full of wounded and surrounded by every kind of distraction. As an instance of the great variety of subject matter, we quote a few particulars from Dr. Saxby's report on "The Shetland Isles" (northern group, the Dr's. residence being at Baltasound). The climate, though damp, is far less severe than is generally supposed, and would be inferred from the situation in so high northern a latitude; the mean annual temperature being 2·63° above Orkney, and only 1·08° below Glasgow. The prevailing wind throughout the year is S.W. The greatest cause of disease appears (as far as in-

fluenced climatologically) to be the sudden changes of temperature, sometimes falling in summer from 81° F. (in the shade) to 48° F. within two days time. As a curiosity in reference to the mode of living of the inhabitants, we notice from this excellent and interesting report that they indulge in tea drinking to such an excess as to make it as ruinous to them as dram drinking elsewhere.

It would be simply impossible to quote even a hundredth part of the interesting scientific facts– leaving even purely medical, physiological, ethnological, and other matters thereunto pertaining out of the question—with which this book abounds. It deserves a place not simply in the library of medical men and practitioners, but in that of all who take an interest in scientific matters generally.

A Manual of Chemical Analysis, Qualitative and Quantitative. For the use of Students. Part I. Qualitative, with special directions for the detection of poisons. By Henry N. Noad, Ph.D., F.R.S., &c. Fourth edition. New Notation. London: L. Reeve, and Co.

This is the fourth time it has been our pleasant duty to speak upon this excellent work. All who know anything of qualitative analysis must be aware that it is by no means an easy task to write a book on that subject which will be comprehensible to students. That Dr. Noad has well succeeded in producing such a book is evidenced by the fact that it has now reached a fourth edition.

We can now, as we did before, recommend this manual with confidence to all who may be in want of a book on this subject. This edition has been, in every respect, brought up to the present advanced state of the science.

Gout and Rheumatic Gout: A New Method of Cure. By John W. Foakes, M.D. London: George Philip and Sons.

The author has for many years adopted a somewhat exceptional, although a chemically sound, treatment for cases of gout and rheumatic gout, which he describes in the work before us. His object is to prevent the frequency and severity of the attacks without using such depressing and injurious medicines as mercury and colchicum, and without weakening or undermining the patient's constitution. The views of the author on the action of mercury seem to accord with those of Dr. Bennett as expressed in the *British Medical Journal*, and a careful examination of the true action of such drugs on the system shows clearly that, while they may give temporary relief, their after effects are most baneful. The views of such men as Dr. Foakes and Dr. Bennett are, we are glad to say, beginning to gain ground amongst the medical profession, and as our contemporary, the *Medical Times and Gazette*, remarks, in speaking of the report of the Edinburgh committee of the British Medical Association, on the cholagogue action of mercury — There is one advantage resulting from the scientific work of the present day; in upsetting old theories it paves the way for new ones: it does this, too, by more accurately recording facts, and so making the new views more nearly approximate to truth. Our knowledge on the subject justifies us in recommending Dr. Foakes's little work to the attention of those interested in the subject.

CORRESPONDENCE.

"THE OVERTHROW OF THE SCIENCE OF ELECTRO-DYNAMICS."

To the Editor of the Chemical News.

Sir,—Under the above title, the Manchester Literary and Philosophical Society has just published a paper by Dr. John Hopkinson, the Senior Wrangler, I presume, of the present year.

In this paper, the distinguished author criticises severely some papers of mine published in the Chemical News and the *Quarterly Journal of Science*.

Dr. Hopkinson quotes one of my articles as follows:—"They (that is Joule and Scoresby) calculate the maximum theoretical power of a grain of zinc to be 158 ft.-lbs., and yet, using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electro-magnets never approaching the permanent magnets nearer than a quarter of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at hap-hazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 ft.-lbs. out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." On which he observes—"It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that, if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments, the quantity of zinc $(a-b)$ used to produce work, W, is observed. If the engine was made more powerful, if the permanent magnets were four times as strong, and the electro-magnets passed $\frac{1}{8}$th of an inch from them, doubtless W would be greater; but so, also, would $(a-b)$, and it does not follow that $\frac{W}{a-b}$, with which we are concerned, would be at all changed. What becomes, then, of the dogmatic assertion that the duty of a grain of zinc would be increased tenfold?" Why he should say "It is hardly credible, but the above looks very like a confusion between Force and Work," I know not. I cannot plead guilty to having made the slightest confusion between the two. But Dr. Hopkinson tries to persuade us that a well-constructed engine would do no more duty than an ill-constructed one; and consequently I presume that the magnets might possibly be weakened *ad infinitum*, and removed to ever so great a distance, without necessarily affecting the efficiency of the engine! And then he criticises my papers as full of fallacies! I reply that the above looks very like a confusion between $(a-b)$ and b! In these experiments of Joule and Scoresby, the quantity of zinc used to produce work, W, is represented by the authors, not as $(a-b)$, but as b, and therefore the duty per grain of zinc is not $\frac{W}{(a-b)}$, but $\frac{W}{b}$; and when the permanent magnets are stronger, and the electro-magnets are passed nearer to them, not only does W increase, but b also diminishes. So that, was I not justified in saying that the duty of a grain of zinc could, in a better constructed engine, be probably increased tenfold? And if it be increased only twofold, I have proved my point, and disproved Joule's mechanical equivalent of heat.

Next let us take Dr. Hopkinson's next criticism. My argument is this—that if the doctrine of the mechanical equivalence of heat be, *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, and if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold not heat, the doctrine of the mechanical equivalence of heat cannot be universally true. To this argument Dr. Hopkinson replies that what I quote as paradoxes are simple deductions from the two laws of Thermodynamics. Quite true, but this only shows that one of the two laws of Thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat.

If I said anything which *seemed* to imply that a minimum of work in an engine was inconsistent with a maxi-

mum of duty, I freely retract the expression; and I also acknowledge that the argument drawn from the fire syringe had better have been omitted. But my point was proved abundantly without it. I have now answered all the points of Dr. Hopkinson's criticism, and I confidently leave the decision on them to anyone who can endure to see a false idol of science broken.—I am, &c.,

H. Highton.

Putney, March 9, 1871.

PS. I hope to discuss the whole question *vivâ voce* at the meeting of the Manchester Literary and Philosophical Society next week.—H. H.

MISCELLANEOUS.

Balloon Letters.—Messrs. Letts, Son, and Co., have just published an interesting balloon letter from Paris as a memento of the late war and the advance of balloon navigation.

London Institution.—The third *conversazione* of the season was held on Wednesday evening at the Society's house in Finsbury Circus. Mr. Henry Holiday delivered an interesting lecture on "Stained Glass," which, with instrumental music and the exhibition of microscopic objects, works of arts, &c., rendered the evening agreeable to the numerous visitors.

Magnetic Motive Power.—We desire to re-affirm the fact to which we, in our last issue, with some superfluous enthusiasm, perhaps, referred, and of which, since then, we have received new corroboration. We have first the data of an examination by an experienced engineer, who gives the result of his examination as follows:—

Number of cells of battery	4
Number of revolutions per minute	340
Diameter of pulley	12 in.
Pressure of brake	65 lbs.
Developed in horse-power.	1·99 to 2.

We do not now accept the theory of infinite development, for we know nothing about it. We do not accept the postulate that there is absence of a positive relation between the consumption of battery and the magnetic development, for that stultifies our judgment. But we do contend that the production of a large magnetic force is desirable from inexpensive elements, whose power we have never suspected, and which, to us, is prophetic of great results.—*New York Telegraph Journal.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Moniteur Scientifique, No. 331, October 1, 1870.

We have received the back numbers of this periodical, almost the only scientific paper which has been regularly printed during the late war and siege of Paris. We sincerely hope such a cause of stoppage of communication with Paris will never occur again. This number contains the following original matter relating to chemistry and allied sciences:—

Vinage.—The continuation of the paper quoted, with names of authors, in Chemical News, vol. xxii., p. 60.

No. 332, October 15, 1870.

This number contains the following original matter relating to chemistry and collateral sciences:—

Supply of Food to Inhabitants of a Besieged City.—G. Grimaud de Caux.—A lengthy paper, chiefly the summary of a memoir read by the author at the meeting of the Institute on the 26th of September last, and the discussion which followed.

White Gunpowder.—A. Roseleur.—It appears, from this short paper, that the well-known mixture, consisting of 2 parts of powdered chlorate of potassa, and 1 part each of ferrocyanide of potassium and sugar, has been made on the large scale at Paris; but, from some unknown and entirely unaccountable cause, an explosion has taken place, whereby the works were destroyed and thirteen workmen instantly killed.

The Food Question.—Dr. Sée.—An extract from a free and public lecture delivered at the Medical School for the purpose of imparting some sound knowledge on the quantity of food required to keep man in a vigorous and healthy state. The daily ration for an adult might be enumerated as follows:—100 grms. of meat, 20 grms. of salt fish, 750 grms. of bread, 50 grms. of lard, and 50 grms. of dried and compressed vegetables—a total of 970 grms. of solid food, containing 88 grms. of albuminous matter. Incidentally, the author observes that crust of bread contains just twice more nutrimental value than crumb, which contains 44 per cent of water. The highly nutritive value of wine is specially alluded to, and illustrated by the fact that, in some districts of France and Spain, men live on bread and wine only for many weeks together in a healthy and vigorous state.

No. 333, November 1, 1870.

This number opens with a lengthy abstract of the subject-matter discussed at the meeting of the Institute on the 10th of October last, the main point under notice being—

The Preservation of Meat, and the Best Means of Utilising, Economically, the Various Parts, inclusive of Bones and Blood, obtainable from Cattle, Sheep, and Pigs.—J. Dumas.

Action of Disinfectants, and especially on Phenic Acid.—A. Devergie.—This lengthy essay does not contain anything new on this subject.

Principal Preparations of Phenic Acid sold in Paris.—Dr. Quesneville.—The author enumerates no less than ten different preparations, including the pure solid acid, and solutions thereof, at varying strength, in water, alcohol, acetic acid, glycerine, vinegar; also lozenges and phenic-aromatic vinegar.

No. 334, November 15, 1870.

This number contains the following original matter relating to chemistry and allied sciences:—

Pneumatic Telegraphy.—M. Guattari.—This paper contains an account of a suggestion, rather than an invention, relating to the application of strongly-compressed air (to eight or nine atmospheres, from 120 to 130 lbs. per square inch), to be conveyed in narrow tubes at certain distances, and to set in motion, at stations, pistons placed in cylinders.

Discovery of Hyposulphite of Soda, and Observations on the Nature of this Salt.—MM. Chaussier and Vauquelin.—The reproduction of a paper (now well nigh forgotten) originally published in the *Journal de la Société des Pharmaciens de Paris*, No. 6, 15 Brumaire, an. viii: (November 15, 1800). The contents of this paper are curious, as instancing the state of chemical knowledge at that period, but Vauquelin's experiments and analysis of the salt alluded to, and then made, have never been controverted.

Colouring Matter Suitable for Imparting a Good Colour to Butter.—Dr. Quesneville.—The author took carrots, cut them into slices, and dried them; and after that, ground them to powder and exhausted the powder with sulphide of carbon. That solvent having been removed, an extract was obtained, which rapidly crystallised, yielding pure carotine, an insipid, inodorous substance, akin, as regards its physical appearance, to alizarine. This material was employed for some time for colouring butter, giving great satisfaction, as being in every respect superior to annatto, but rather more expensive.

Nos. 335 and 336 (double number), December 1 and 15, 1871.

This number contains the following original matter relating to chemistry and collateral sciences:—

Best Compositions for Chemical Manures, and on the Analysis of Soils, as Propounded by M. George Ville.—Dr. Grüneberg.—This lengthy essay contains the main leading points of a lecture given on the above-named subject before a farmer's club at Cologne. As an abstract of all that has been done and published by M. Ville *in extenso*, this paper is of value.

Air Balloons which can be Steered in any Desired Direction.—Rev. F. Moigno.—A lengthy account of all that has been really and effectively done as regards this subject.

Source of Obtaining Chloride of Sodium which has not been thought of at Metz, and on the Action of Salt on the Human Body.—Dr. Quesneville.—The author calls attention to the fact that urine contains from 3 to 8 grms. of salt in 1000 grms., but does not specify how this salt could be extracted so as to be readily available again for use as a necessary condiment, since it would not be an easy matter to separate the chloride of sodium from the sulphates of soda and potassa, and the phosphate of soda simultaneously present in the

saline mass obtained after evaporation and ignition of the salts contained in urine.

Resignation of M. J. Dumas as President of the Mint Committee.—It appears, from an extract of the official paper of the French National Government, here reproduced (dated November 18th last), that the *savant* just named tendered his resignation, although invitcd to retain his post, after the dismissal of his son, M. E. Dumas, from the position of assayer to the *Bureau de Garantie des Objets d'Or et d'Argent*—that is to say, the head of that branch of administration connected with the Mint which performs the duties which in this country, by several ancient Acts of Parliament, devolve upon the Goldsmiths' Company, viz., the assaying and stamping of gold and silver ornaments and plate for domestic use.

Nos. 337 and 338 (double number), January 1 and 15, 1871.

This number contains the following original papers and memoirs bearing upon chemistry and collateral sciences:—

Historical Resumé of all that has been Experimentally done and published in reference to Gelatine.—Professor Chevreul.—This excellent memoir, too lengthy and too concisely written for any useful abstraction, contains all that is really valuable and practically available on this subject.

Peculiar System of Telegraphic Correspondence—A. Guiot—And on—

Pigeon Post—Rev. F. Moigno—Are both papers specially bearing upon well-known events.

Force Exerted by Gunpowder and other Explosive Substances.—Dr. Berthelot.—This very valuable memoir contains the following sections:—General relations and definitions; heat of composition of the oxygen compounds of nitrogen; gunpowder containing saltpetre; gunpowders containing nitrate of soda; chlorate of potassa gunpowder; chloride of nitrogen; nitroglycerine; gun-cotton; picrate of potassa, either pure or mixed with other substances.

Nitroglycerine and Various Dynamites.—C. Girard, A. Millot, and G. Vogt.

Effect of Intense Cold on the Human System.—Dr. Quesneville.—A physiological essay.

Nos. 339 and 340 (double number), February 1 and 15, 1871.

This number contains the following original papers and memoirs bearing upon chemistry and collateral sciences:—

Composition of Milk, and on the Preparation of Obsidional Milk (Lait Obsidional).—M. Dubrunfaut.—This paper, and the two following, bear upon the late exceptional condition and poverty of milk in Paris.

Preparation of Artificial Milk.—A. Gaudin.

Report to the Academy of Medicine on Substitutes for Milk.—Dr. Gubler.

Force Exerted by Explosive Gas Mixtures.—Dr. Berthelot.—Reserved for reproduction and translation.

Dynamite, and its Use as a Substitute for Gunpowder.—P. Champion.

Preservation of Meat.—Dr. Baudet.—This memoir contains the detailed account of a series of experiments made by the author on this subject. The paper is of very great importance generally.

Geology of Meteorites.—S. Meunier.—An extensive monograph on this subject.

Crystals of Albumen.—Dr. H. Lissagaray.—A preliminary notice on this subject, the author having been obliged to leave his laboratory for the purpose of becoming a soldier of the 83rd battalion of mobiles.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 1, 1871.

This number contains the following original papers and essays:—

Combustion Furnace for Organic Elementary Analysis.—A. Müller.—This paper, illustrated by a woodcut, required for the proper understanding of the subject, opens with the following remarks bearing upon the conditions and requirements of a good combustion-furnace for organic elementary analysis. It ought to give a heat sufficient to bring to incipient fusion difficultly fusible hard potassa glass. The tube should be uniformly heated over its entire length, and also to the same degree top and bottom. The operator should be able to control the degree of heat within all required limits, and, lastly, the combustion should be from first to last so conducted as to be, in all respects, under the observation of the operator.

Sulphanilic Acid.—E. Ador and V. Meyer.—This lengthy memoir treats chiefly on the substitution products obtainable from the acid alluded to, and on the constitutional formulæ of these bodies.

Lecture Experiment for Illustrating the Phenomena of Diffusion.—Dr. F. Wöhler.—With a woodcut.

Basicity of Oxide of Uranium, Molybdic Acid, Boric and Nitrous Acids.—C. Schultz-Sellack.—This paper contains the following sections:—Sulphate of antimony, $Sb_2(SO_4)_3$; percentically—Sb_2, 45·45; $(SO_4)_3$, 54·55. This salt is not affected by dry air, but decomposed by water, and by ignition sulphuric anhydride is given off. Sulphate of bismuth, $Bi_2(SO_4)_3$. Normal sulphate of uranium, $(UO)_2SO_4$.—This salt crystallises from its solution in concentrated sulphuric acid; the substance is amber-coloured, soluble in water, and deliquescent. Acid sulphate of uranium, HUO,SO_4. Anhydrous acid sulphate of uranium, $(UO)_2SO_4+SO_3$. Sulphate of molybdenum, $(MoO)_2SO_4$. Sulphate of boric acid, or sulphuric boric acid, $2(HBOSO_4)+SO_3$.—In 100 parts—2BO, 16·46; H_2, 0·61; $3SO_3$, 73·71. Best obtained by dissolving boric acid anhydride in fuming sulphuric acid, when crystals are formed after a while, being the compound in question.

Formation of Pyrocatechin from Carbo-Hydrates, and more especially Cellulose.—F. Hoppe-Seyler.—This paper treats on the products formed by the combustion (dry distillation) of paper and other carbohydrates at temperatures not exceeding 210°, and with or without the presence of water. The experiments on this subject, and especially the mode of formation of pyrocatechin, are not yet completed.

Tin-Triethyl-Phenyl.—A. Ladenburg.—The body just named, $Sn(C_2H_5)_3C_6H_5$, is a colourless fluid, boiling at 254° without decomposition. The vapour of this body is partly decomposed by the action of the air. The fluid is readily soluble in ether and absolute alcohol, but insoluble in water; sp. gr. at 0°, 1·2639; burns with a luminous, sooty flame, leaving metallic tin; added to alcoholic nitrate of silver solution, the result is the reduction of the silver.

Some Reactions of Stanno-Triethyl.—A. Ladenburg.—This paper is the concluding portion to an essay on the subject, commenced in another portion of this periodical.

Action of Cyanide of Potassium on Bromonitrobenzol.—V. von Richter.—This paper treats, at great length, on the action which pure cyanide of potassium exerts when heated in sealed tubes at from 180° to 200° along with bromonitrobenzol. Carbonate of ammonia and ammonia gas are formed; while the contents of the tube, after boiling with caustic potassa and precipitation with sulphuric acid, yielded two new acids, which appeared to contain bromine, but not a nitro-compound. The acids were not obtained in pure state, and the author continues his researches on this subject.

The Law of Avogadro.—A. Naumann.

Hypothesis of Avogadro.—L. Meyer.

New Derivatives from Resorcine.—P. Weselsky.—After briefly referring to a series of experiments formerly made by him, the author gives a short and preliminary account of the action of nitrous acid upon resorcine, resulting in the formation of a compound containing three molecules of resorcine and one molecule of N_2O_3. The body thus formed is reddish coloured, crystalline, yielding, when dissolved in alcohol, a purplish-red coloured solution, which, by the addition of alkalies, becomes deep indigo-blue. The author continues his researches on this subject, which, he states, promises to yield also very important industrial results.

Action of Sulphur upon Benzol.—F. Schulze.—This paper contains an account of a series of experiments made by the author, and consisting in submitting benzol and sulphur (the former being to the latter in the proportion, by weight, of from 64:78), together, to a temperature of some 400°—500°, care being taken to enclose the liquid in small thin-sided sealed glass tubes, and the solid in a combustion-tube, into which (previous to sealing it before the blowpipe) the tube containing the benzole was also placed. The combustion-tube was then put into a stout wrought-iron gas-pipe, well closed with a plug, and the contrivance submitted to strong red heat. The results of this experiment are, that some compound of sulphur and benzol is formed, but the precise definition of the nature thereof is a matter as yet not completed.

New Series of Alcohols.—C. Graebe.—This paper contains, as a preliminary notice, the account of the results of some experiments made by the author with acetyl-benzol (aceto-phenon), $C_6H_5COCH_3$, which substance, when acted upon by chlorine at a higher temperature, yields chlormethyl-benzol, $C_6H_5COCH_2Cl$, a solid body, fusing at 41°, boiling at 246°. Upon being boiled with an alcoholic solution of acetate of potassa, this body yields the acetate of acetyl-benzol-alcohol, $C_6H_5COCH_2(OC_2H_3O)$, a solid substance, fusing at 44°, boiling at 270°; this substance yields, when acted upon by alcoholic solution of caustic potassa, the acetyl-benzol-alcohol, $C_6H_5COCH_2(OH)$.

Blue-Coloured Essential Oil of Chamomile.—J. Kachler.—This lengthy essay contains the detailed account of a series of researches made not only with the essential oil alluded to, but also with the blue oil obtained by the distillation of the galbanum. It appears that the essential oil of chamomile is a very complex substance, consisting of rutinic acid, $C_{10}H_{20}O_2$, of a camphor-like body, boiling between 150° and 165°, $C_{16}H_{10}O$ of a hydrocarbon ($C_{10}H_{16}$). The author's essay also describes at length a series of products obtained by various reagents from the oil.

Metallic Ammonias of the Metallamines.—C. W. Blomstrand.—This exhaustive monograph, full of formulæ, is not suited for any useful abstraction, with due reference to the value of the memoir.

Isomorphism of Soda-Saltpetre, and Calcareous Spar.—L. Meyer.—After observing that the crystalline shape of nitrate of sodium, $NaNO_3$, very closely agrees with the crystalline form of calcareous spar, $CaCO_3$, the author states that, when a rhombohedrically-shaped piece of calcareous spar, cleaned with some dilute nitric acid, is hung by means of a silk thread into a moderately strong solution of nitrate of sodium made with hot water, there is deposited on the crystal of calcareous spar a layer of crystallised nitrate of sodium, just in the same way as when a crystal of chrome alum is hung up in a solution of ordinary alum. The centre of the new crystal first alluded to consists of calcareous spar; while the layer of nitrate of sodium growing round it so deposited, as to make it appear that there was no difference of substance.

Review of the Researches and Investigations made in the Chemical Laboratory of the University of Göttingen.—Dr. B.

Tollens.—This lengthy paper gives a brief catalogically-arranged *resumé* of the subject mentioned; but most, if not all, the subject-matter treated of has been published before this in various periodicals, and noticed therefrom by us.

Report of the Last Meeting of the Chemical Society at Zurich.—O. Meister.—Dr. Tuchschmid read a short note on azoxy-anthracen, a solid body, soluble in boiling alcohol and in benzol, and fusing at 275°; formula—

$$\left.\begin{matrix}C_{14}H_6N\\C_{14}H_9N\end{matrix}\right\rangle O.$$

The communication further includes a lengthy and concisely-written paper on—

Bichlor-Ether.—M. Abeljanz.—The author expects to be able to obtain, from the substance alluded to, the formula of which is—

$$CH_3---CCl_2---O.C_2H_5,$$

by the action of pentachloride of phosphorus, a compound—

$$CH_3---CCl_3.$$

Atomistic Size and Quantivalence of Silver.—Professor Wislicenus.

Journal für Gasbeleuchtung, first number for January, 1871.

Since the beginning of this year this periodical has been published fortnightly instead of, as heretofore, monthly. This number contains the following original paper relating to chemistry and allied sciences:—

On Hydrotimetry and German Degrees of Hardness.—E. Reichardt.—This paper contains a review of the different values assumed by various authors for expressing the degree of hardness of water, and the different modes of testing in use. The author sets forth that, instead of taking carbonate of lime as unit, it is preferable to take lime, CaO, and to make 1 part of that base in 100,000 parts of water =1 degree.

Second number for January, 1871.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

What Method of Water Analysis is the Best for Practical Use; Observations on Hydrotimetry.—Dr. A. Wagner.—This essay is written with the view to enable waterworks' engineers and managers to have a good and easily executable—and, for all practical purposes, reliable—method of testing, qualitatively and quantitatively, by gravimetrical method, the quality of the water of their works, as well as of the sources these are supplied from.

Instrument intended for the Measurement of the Size of Gas Flames.—H. Von Wolfsberger.—This paper, illustrated by a woodcut, contains the description and mode of employing a contrivance intended for the correct measurement of gas flames. The instrument may be obtained from the Société Genevoise pour la Construction d'Instruments de Physique, Chemin Courgas No. 113, Genêve, Switzerland; the price is about £4.

Arrangement of the Reservoirs and Water Supply Pipes and Hydrants in the New Opera Building at Vienna.—A. Folsch.—A valuable contribution to the best methods of guarding against fires in theatres.

Projected Plans, Specification, and Conditions upon and according to which the Communal Government of Vienna desires to take in hand the Public and Private Gas Supply of that City.—Dr. N. H. Schilling.—This lengthy document, very clearly and precisely drawn up, is worthy the notice of all who are interested in such matters, and the more so as the paper bears evidence of the very high degree of efficiency, and advanced state of Communal Administration, also, in the Austro-Hungarian Empire.

NOTES AND QUERIES.

Tin Pipe.—Can any reader of the CHEMICAL NEWS tell me where pure tin pipe is to be obtained? I want it to convey distilled water in a laboratory.—F.C.S.

Writing Ink.—Will you kindly inform me in the CHEMICAL NEWS how to make a good writing ink so that it will copy well and write of a greenish colour; I think the green shade is owing to the indigo which is put in. I wish it to appear clear when copied?—H. PITT.

Welding Steel and Iron.—Perhaps some of the contributors to your very appreciable journal might oblige one of its constant readers with the communication of a good process whereby cast-steel and iron might be welded together?—W. A. MURRAY.

Tattooed Marks.—(Reply to "Scotia.")—As far as we have been able to ascertain, by reference to what is published on tattooing, there appears to be no possibility of erasing the marks. These are usually made by such means as to penetrate to the cutis of the body, where they become indelible.

Determination of Sulphur in Cast-Iron, &c.—Will Mr. Elliott, who published a process for the determination of sulphur in cast-iron (CHEMICAL NEWS, vol. xxiii., p. 61), kindly state whether the solutions remaining in the flask and the insoluble residues were tested for sulphur, and, if so, the result?—S. G. C.

Production of Wood Spirit.—I see, in CHEMICAL NEWS, vol. xxiii., p. 91, you have given an abbreviated account of the production of wood spirit from a paper by Mr. E. T. Chapman; particulars, however, respecting the final treatment of the products of distillation are omitted. Could you kindly supply the omitted particulars?—JOHN F. EARSS, B.A.

Laughing Gas.—We read at vol. ii., p. 113 (1871), of Dr. Dobell's "Reports on the Progress of Medicine in Various Parts of the World," that the Messrs. Coxeter have constructed wrought-iron vessels of a cylindrical form, and furnished with a stopcock and proper appliances for inhaling the nitrous oxide gas contained in the iron vessels in a liquefied state. The largest vessel contains 100 gallons of the gas (liquefied, and under the pressure alluded to in our last number), although the vessel is but 12 inches long by 3¼ inches in diameter, and the weight only 9 lbs. 1 oz.

Reduction of Titanium.—In Watts's "Chemistry," and in Berzelius, it is stated that titanic oxide can be reduced to the metallic state if mixed with one-sixth of its weight of charcoal, and submitted, in a covered crucible, to the action of an intense heat. From some experiments I have made, I doubt this. In the first place, if the following equation be correct:—$TiO_2+2C=Ti+2CO$, the equivalent weight of charcoal should be 2-7ths of the titanic oxide. I have exposed mixtures in such proportions to the action of a full white heat for several hours, and obtained, as I believe from the reactions, not metallic titanium, but a mixture of TiO + C. In conclusion, I should feel obliged if any of your correspondents who may have repeated the experiments described in Watts and Berzelius would state whether their experience coincides with mine, or not.—S. T.

MEETINGS FOR THE WEEK.

MONDAY, 20th.—Medical, 8.
—— London Institution, 4.
TUESDAY, 21st.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
—— Ethnological, 8.
—— Zoological, 9.
WEDNESDAY, 22nd.—Society of Arts, 8.
—— Geological, 8.
THURSDAY, 23rd.—Royal, 8.30.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— Royal Society Club, 6.
—— London Institution, 7.30.
FRIDAY, 24th.—Royal Institution, 9. Prof. Clerk Maxwell, on "Colour."
—— Quekett Microscopical Club, 8.
SATURDAY, 25th.—Royal Institution, 3. Mr. O'Neil, "On the Spirit of the Age."

TO CORRESPONDENTS.

*** Vol. XXII. of THE CHEMICAL NEWS, containing a copious index, is now ready, price 11s. 8d., by post, 12s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our office, or, if accompanied by a cloth case, for 1s. Subscribers wishing to complete their sets of volumes are requested to apply to the publisher, who will give them information respecting scarce numbers and volumes. Vol. xxiii. commenced on January 6th, and will be complete in twenty-six numbers. READING CASES, price 1s. 6d. each post free, may also be obtained at the Office.

W. N. Evans.—We do not know Dr. Gunther's address. By applying to Asher and Co., Foreign Booksellers, Bedford Street, Covent Garden, you may perhaps ascertain if the paper has been published as a pamphlet or not.

W. J. Reynolds.—The analyses are official, and are still inserted in the CHEMICAL NEWS when they are sent.

J. Lowe.—We do not know; apply to one of the medical papers.

A. Payne.—"A Practical Treatise on Coal, Petroleum, and other Distilled Oils," by A. Gesner, M.D., published by Baillière Bros., New York; or "The Manufacture of Photogenic or Hydrocarbon Oils from Coal and other Substances, by T. Antisell, M.D., published by Appleton and Co., New York.

Just published, with 90 Engravings on Wood, 8vo., 10s. 6d.

A Laboratory Text-Book of Practical Chemistry; or, Introduction to Qualitative Analysis; a Guide to the Course of Practical Instruction given in the Laboratories of the Royal College of Chemistry. By WM. G. VALENTIN, F.C.S.

J. and A. Churchill, New Burlington-street.

Just published, price 2s. 6d.,

Sewage Irrigation; a Lecture, by W. Hope, Esq., V.C., delivered to the Ratepayers of West Derby (near Liverpool). With Plan, printed in colours.

London: Edward Stanford, 6 and 7, Charing Cross, S.W.

Now ready, price 6d.,

Notes of Professor Odling's Juvenile Lectures on BURNING AND UNBURNING, delivered at the Royal Institution of Great Britain, Christmas, 1870-71.

London: CHEMICAL NEWS Office, Boy Court, Ludgate Hill, E.C.

THE CHEMICAL NEWS.

Vol. XXIII. No. 591.

ON THE ACTION OF HYDROBROMIC ACID ON CODEIA.

By C R. A. WRIGHT, D.Sc.

It has been shown by the late Dr. A. Matthiessen, in conjunction with the writer,† that when codeia is heated with a large excess of strong hydrochloric acid the following reactions successively take place:—

Codeia. Chlorocodide.
$$C_{18}H_{21}NO_3 + HCl = H_2O + C_{18}H_{20}ClNO_2.$$

Chlorocodide. Apomorphia.
$$C_{18}H_{20}ClNO_2 = CH_3Cl + C_{17}H_{17}NO_2.$$

It appeared of interest to examine the action of hydrobromic acid under similar circumstances, and for this purpose Messrs. Macfarlane, of Edinburgh, with their wonted liberality, put a considerable quantity of pure codeia at the writer's disposal.

The aqueous hydrobromic acid employed was obtained by the action of H_2S on Br in presence of water, and subsequent rectification over pulverised KBr; it was free from SO_4H_2 and other sulphur compounds, had a sp. gr. of about 1·5, and contained about 48 per cent of HBr.

When codeia is heated with from three to six times its weight of this acid, either on a water-bath or to gentle ebullition over a flame, the liquid, which at first produces no precipitate with solution of carbonate of soda, gradually darkens in colour, and acquires the property of yielding a dense white precipitate with this reagent. No appreciable quantity of methyl bromide is evolved during the first stages of this change, but subsequently this body is produced in some little quantity.

The precipitate thrown down by carbonate of soda before this further change ensues, appears to consist of a variable mixture of at least three substances, two of which are readily soluble in ether, while the third is but sparingly soluble in that menstruum; all are bases, the one insoluble in ether, and one of those soluble containing bromine: the one apparently first formed is produced by a reaction precisely analogous to that whereby chlorocodide is generated, viz.,—

Codeia. Bromocodide.
$$C_{18}H_{21}NO_3 + HBr = H_2O + C_{18}H_{20}BrNO_2,$$

and is, therefore, termed bromocodide; this base appears to be acted on further with great ease, giving rise ultimately to the other two, the first of which has the constitution of codeia less one equivalent of oxygen, or $C_{18}H_{21}NO_2$, and is, therefore, provisionally named deoxycodeia; whilst the second has the composition of four molecules of codeia coalesced together, one of the 84 H atoms in the product being replaced by Br; it is, therefore, provisionally termed bromotetracodeia, the simplest mode of representing the simultaneous formation of these two bases being as follows:—

Bromocodide. Codeia.
$$C_{18}H_{20}BrNO_2 + 4C_{18}H_{21}NO_3 =$$
Deoxycodeia. Bromotetracodeia.
$$= C_{21}H_{21}NO_2 + C_{72}H_{83}BrN_4O_{12}.$$

Owing to the ease with which bromocodide is altered, it is a matter of some difficulty to obtain it in even an approximately pure condition, as the complete separation of deoxycodeia appears impracticable when this base has once been produced. The product of the action of three parts 48 per cent acid on one part codeia on the water-bath for from one to two hours is precipitated by excess of sodium carbonate, and the precipitate collected on filters; unaltered codeia is for the most part separated thus, being contained in the filtrate. Extraction of the mass with ether and agitation of the ethereal solution with HBr furnishes crude bromocodide hydrobromate, which may be purified by a repetition of the process, fractional precipitation being resorted to to get rid of traces of colouring-matters; the purified hydrobromate thus obtained was a viscid colourless liquid which utterly refused to crystallise, and dried up to a gum-like mass over SO_4H_2. Dried at 100°, the powdered gum gave these numbers*:—

0·3500 grm. gave 0·6340 CO_2 and 0·1580 H_2O.

0·230 grm. boiled with NO_3H and $AgNO_3$ gave 0·1900 Ag Br.

	Calculated.		Found.	
C_{18}	216	48·76	49·40	
H_{21}	21	4·74	5·01	
Br_2	160	36·12	..	35·08
N	14	3·16		
O_2	32	7·22		
$C_{18}H_{20}BrNO_2HBr$	443	100·00		

The slight excess of carbon and deficiency in bromine thus found are doubtless due to the presence of a little deoxycodeia, the hydrobromate of which requires 59·34 per cent carbon and 21·98 per cent Br. Another specimen of bromocodide hydrobromate, prepared as above from the product of three hours' digestion at 100° of one part codeia and three parts 48 per cent HBr, yielded numbers indicating 51·6 per cent carbon, 5·3 and 33·4 per cent Br; whilst a repetition of the purification process scarcely altered the numbers. Owing to the great difficulty in preparing the pure salt in quantity, no attempt to isolate and analyse the base itself was made, the more so that the precipitate thrown down by carbonate of soda from the pure hydrobromate appeared to tally in every respect with the chlorocodide formerly examined; their qualitative reactions, too, are identical.

The crude bromocodide hydrobromate obtained after five or six hours' digestion of codeia with from three to five times its weight of 48 per cent HBr deposited on standing for some days crystals not readily soluble in cold water; recrystallised several times from boiling water, minute snow-white crystals were ultimately obtained; these slightly darkened on drying over SO_4H_2, and more so at 100°, and gave the following numbers on analysis:—

0·3565 grm. gave 0·7760 CO_2 and 0·1960 H_2O.

0·3245 grm. gave 0·7045 CO_2 and 0·1790 H_2O.

0·2200 grm. burnt with soda-lime gave 0·0570 Pt.

0·1380 grm. boiled with NO_3H and $AgNO_3$ gave 0·0700 AgBr.

These numbers agree with those calculated for deoxycodeia hydrobromate, as the following comparison shows:—

	Calculated.		Found.			
C_{18}	216	59·34	59·36	59·21		
H_{22}	22	6·05	6·11	6·13		
N	14	3·84	..	..	3·69	
O	32	8·79				
Br	80	21·98	..	..	..	21·59
$C_{18}H_{21}NO_2HBr$	364	100·00				

The yield of this base from the codeia used being but small (about 4 per cent), no attempt was made to isolate the base itself; carbonate of soda throws down from the hydrobromate solution a white precipitate which is soluble in alcohol, ether, benzol, and chloroform; by exposure to air it rapidly becomes coloured, and finally acquires a very dark green tint. Its qualitative reactions are identi-

* Read before the Royal Society, March, 1871.

† *Proceedings of the Royal Society*, vols. xvii., p. 460; xviii., p. 83.

* All combustions given in this paper were made with lead chromate and finished in a stream of dry oxygen.

cal with those of apomorphia; the colour-reactions of the two with Fe_2Cl_6, NO_3H, and $SO_4H_2+K_2Cr_2O_7$, being indistinguishable when examined side by side. Its physiological effects, however, are different; three-tenths of a grain of the hydrobromate administered by the mouth to a dog producing no appreciable effect, whilst a much less dose of apomorphia produces speedy vomiting.

The third base is conveniently obtained, as hydrobromate, by treating codeia with three times its weight of 48 per cent HBr for two hours on the water-bath, precipitating the product (diluted with water) by excess of carbonate of soda, collecting on filters, and well draining from the mother-liquors, and finally extracting with ether until scarcely anything more is taken up; care must be taken to have as little watery fluid as possible present, otherwise the insoluble substance forms a sort of lather on agitation from which the ether will not separate. The insoluble substance is then dissolved in the least possible quantity of weak hydrobromic acid and fractionally precipitated by cautious addition of stronger acid; the second precipitate is dissolved up in water, in which it is readily soluble, and a few drops of carbonate-of-soda solution added. The filtrate from this yields, with strong HBr, nearly white flakes, which are wholly void of crystalline character under the microscope. These remain solid at 100° if previously completely dried over SO_4H_2; but if warmed while moist, become a more or less coloured tar. Dried at 100°, the following numbers were obtained:—

0·3440 grm. gave 0·6810 CO_2 and 0·1740 H_2O.
0·3425 grm. gave 0·6685 CO and 0·1680 H_2O.
0·5615 grm. burnt with soda-lime gave 0·1310 Pt.
0·3200 grm. boiled with NO_3H and $AgNO_3$ gave 0·1330 AgBr.
And 0·0315 Ag.

	Calculated.		Found.	
C_{72}	864	54·03	53·99	53·23
H_{87}	87	5·44	5·61	5·45
N_4	56	3·50	..	.. 3·33
O_{12}	192	12·01		
Br_5	400	25·02	..	.. .. 24·97
$C_{72}H_{83}BrN_4O_{12},4HBr$	1599	100·00		

Carbonate of soda throws down from the hydrobromate a nearly white precipitate, which rapidly darkens, and finally turns a deep green, nearly black. Dried at 100° rapidly, the product gave the following numbers, which fall below those required for the formula $C_{72}H_{83}BrN_4O_{12}$, but which agree with those required for a similar formula but containing more oxygen:—

0·3810 grm. gave 0·8460 CO_2 and 0·2080 H_2O.
0·4430 grm. boiled with $AgNO_3$ and NO_3H gave 0·059 AgBr.

	Calculated.		Found.	
C_{72}	864	60·89	60·56	
H_{83}	83	5·85	6·07	
Br	80	5·64	..	5·67
N_4	56	3·95		
O_{12}	336	23·67		
$C_{72}H_{83}BrN_4O_{12}+O_9$	1419	100·00		

It hence appears that the free base rapidly absorbs oxygen. In confirmation of this, 0·11 grm. of the hydrobromate treated with caustic potash and injected by a pipette into 15 c.c. of air over mercury absorbed 0·9 c.c. in the course of an hour, or 6 per cent of the total volume of the air; the salts, however, when dry, may be kept without alteration, and only slowly darken by exposure to air when moist.

This welding together of four molecules is not wholly without parallel in the history of the opium alkaloids; and their derivatives thus opianic acid heated* furnishes a body containing four times as much carbon as the original acid; thus—

$$4C_{10}H_{10}O_5 = H_2O + C_{40}H_{38}O_{19}.$$

The qualitative reactions of bromotetracodeia appear to be identical with those of bromo- and chlorocodide. The base itself, when freshly precipitated, is slightly soluble in water, being thrown down again by addition of strong brine; in ether and benzol it is almost insoluble, and in alcohol but sparingly soluble.

When crude bromotetracodeia, got by extraction with ether of the mixture of bases thrown down by carbonate of soda, is dissolved in weak hydrochloric acid and precipitated twice or thrice by excess of stronger acid, nearly white flakes are ultimately obtained, resembling in all their physical properties the bromohydrobromate of tetracodeia. These flakes, however, contain no bromine, the absence of this element being ascertained by the negative results obtained on examining with chlorine-water and ether the acidified solutions of the lime-salts got by combustion with quicklime, and of the sodium-salts got by boiling with NO_3H and $AgNO_3$, and fusing with carbonate of soda the silver-salts thus got. Dried over SO_4H_2 and finally at 100°, this body gave numbers indicating a base of constitution analogous to that of bromotetracodeia; it may, therefore, be termed chlorotetracodeia.

Specimen A.
0·3880 grm. gave 0·1970 AgCl.
0·3645 grm. gave 0·8395 CO_2 and 0·2120 H_2O.
0·3940 grm. burnt with soda-lime gave 0·1080 Pt.

Specimen B.
0·4460 grm. gave 0·0150 CO_2 and 0·2560 H_6O.
0·2350 grm. gave 0·1250 AgCl.

	Calculated.		Found.	
			Specimen A.	Specimen B.
C_{72}	864·0	62·77	62·81 ..	62·07
H_{97}	87·0	6·32	6·46	6·38
N_4	56·0	4·07	.. 3·90	
O_{12}	192·0	13·94		
Cl_5	177·5	12·90	12·56 ..	.. 13·16
$C_{72}H_{83}ClN_4O_{12},4HCl$	1376·5	100·00		

Specimen A had been three times precipitated by HCl in large excess, while specimen B had only been thrown down twice, and probably retained a trace of bromotetracodeia.

Specimen A converted into platinum-salt gave the following numbers after drying at 100°.

0·4215 grm. gave 0·0810 Pt = 19·22 per cent.

The formula $C_{72}H_{83}ClN_4O_{12},4HCl,2PtCl_4$ requires 19·18 per cent.

Like bromotetracodeia, the free base appears to absorb oxygen with avidity. Dried as rapidly as possible at 100°, the precipitate thrown down by carbonate of soda gave these numbers:—

0·3880 grm. gave 0·9190 CO_2 and 0·2230 H_2O.
0·3100 grm. gave 0·0330 AgCl.

	Calculated.		Found.	
C_{72}	864	64·74	64·59	
H_{83}	83	6·22	6·38	
N_4	56	4·20		
$O_{18\frac{1}{2}}$	296	22·18		
Cl	35·5	2·66	..	2·64
$C_{72}H_{83}ClN_4O_{12}+O_{6\frac{1}{2}}$	1334·5	100·00		

In all its physical and chemical properties chlorotetracodeia closely resembles bromotetracodeia: their qualitative reactions are identical; they have an intense bitter taste and apparently but slight physiological action, at any rate in small doses.

My thanks are due to Mr. I. L. Bell, in whose laboratory the above experiments were carried out.

* Matthiessen and Wright, *Proceedings of the Royal Society*, vol. xvii., p. 341.

ON THE

CHANGE EXPERIENCED BY METALLIC IRON AND BY IRON OXIDES WHICH HAVE SERVED TO DISSOCIATE CO.*

By I. LOWTHIAN BELL.

Expt. 331.—A weighed quantity of pure Fe_2O_3 was placed in a boat in a porcelain tube, heated by a Hofmann's furnace. By means of a T-tube, furnished with screw pinch-cocks, either H or CO could be admitted at pleasure from their respective sources—viz., zinc and sulphuric acid for the one, and ferrocyanide of potassium and sulphuric acid for the other. Precautions were employed for purifying and drying the gases, and which, therefore, may be assumed as arriving at the Fe_2O_3 in a perfectly pure state. All air was expelled from the apparatus, CO generator included, by a long-continued stream of hydrogen; the porcelain tube was then heated, and the H_2O produced collected and weighed. During this time the CO generator was giving off CO, but the gas was permitted to escape through a side valve. When the ferric oxide was completely reduced (known by no more H_2O being produced after several minutes' passage of H), the H was shut off, and the CO turned on.

	Grms.
5·487 grms. of pure Fe_2O_3, exposed to H, gave H_2O	1·858
The theoretical quantity of H_2O being	1·852

CO was then passed over the reduced iron for 4½ hours at a temperature of melting zinc—the residual mass was dissolved in HCl, and the carbon left weighed 0·78 grm., equal to 20·3 per cent of the Fe present.

Expt. 332.—3·497 grms. of pure Fe_2O_3, heated in H, yielded, in one hour, of H_2O.. .. .. .. 1·189

In ten minutes more, additional ..	0·001
,, ,, ,, ..	*nil.*
Total	1·190
Theoretical water being ..	1·1802

CO passed over it for one hour at a very dull red heat, gave, of carbon, 0·0584, equal to 23·8 per cent on the Fe.

Expt. 333.—3·497 grms. of pure Fe_2O_3 gave same results as above when treated with H, no more H_2O being produced after twenty-five minutes' passage of gas. The spongy iron was then exposed to CO at a low red heat for two and a half hours. The carbon deposited was determined by combustion in oxygen, with a tube in front, containing red-hot CuO, to prevent loss by formation of CO. The CO_2 corresponded to 0·313 grm., equal to 12·3 per cent of carbon on the Fe present.

In this last instance, the higher temperature has materially lessened the carbon deposition, inasmuch as, although the operation lasted two and a half times as long as the previous trial, the C obtained is a mere fraction above half that in Experiment 336.

Believing it improbable that the metallic spongy Fe should have been instrumental in resolving CO into C and CO_2, without itself suffering some change, the various steps of the process were narrowly watched. In some experiments, after the spongy iron had been exposed to CO for a known time, H was again passed through the tube by means of the arrangements of pinch-cocks and T-tube, formerly described. In all these cases, there was a formation of H_2O, the oxygen of which could have only come from CO. In other experiments, the CO_2 formed by the action of the CO on the spongy iron was collected and weighed, and the residual carbon was likewise determined. This latter was always in excess of the former, showing that a hypothetical splitting-up of 2CO into C and CO_2 would not alone account for the observed facts. In other instances, again, the tube was allowed to cool in a stream of CO, the residual mass weighed, and the deposited carbon determined; the iron present being known from the weight of the pure Fe_2O_3 used. It was always found that the Fe and C together fell short of the actual weight of the residual substance, showing that it must have contained oxygen in addition.

Expt. 334.—2·448 grms. spongy Fe from Fe_2O_3 reduced by H:—

	Grm.
CO_2, collected during passage of CO for one hour at a very dull red heat..	0·214
CO_2 produced on combustion of carbonaceous mass in oxygen	0·237
Difference	0·023
This represents, of carbon	0·006
Originally associated with, of oxygen	0·008

This amount of oxygen must therefore have been taken up by the spongy iron; in confirmation of this, 0·0065 grm. of H_2O were collected, on passing hydrogen a second time over the carbonaceous mass (previous to its combustion in oxygen), corresponding to, of oxygen, 0·006 grm.

The slight discrepancy between this number and that got before, 0·008, arises from the impossibility of collecting the CO_2 contained in the large excess of CO without slight loss, either from deficient absorption, or from the removal of traces of aqueous vapour from the potash bulbs.

Expt. 335.—The residual iron oxide left in the last experiment, after burning off the carbon in oxygen, was again thoroughly reduced in H, then exposed to CO at a low red heat for two and a half hours.

	Grms.
H_2O, produced by second passage of H, 0·023 grm., representing of oxygen	0·0204
Weight of residual iron and carbon	2·7590
Weight of iron in 3·497 Fe_2O_3	2·4480
Leaving, for carbon deposited	0·3110
And the carbon, as determined by combustion, was	0·3130

Expt. 336.—The weight of residual Fe+C+O obtained after reducing the 5·487 grms., Fe_2O_3 mentioned in Experiment 331, and then exposing it to CO for four and a half hours, was .. .. 4·707 grms.

Fe in 5·487 Fe_2O_3 is	3·841	grms.	—
C left on solution in HCl ..	0·780	,,	4·621 ,,
Leaving for O taken up by Fe			0·086 ,,

These last experiments yield the following numbers, calculated upon 100 parts of metallic iron present:—

	A.		B.	C.
Time of exposure to CO	1 hour.		2½ hours.	4½ hours.
Temperature	Very dull red.		Low red heat.	Melting zinc.
Oxygen in reformed iron oxide.. ..	0·34	0·24	0·84	2·24
Carbon deposited ..	2·64		12·7—12·8	20·3

It would, therefore, seem that the amount of reformed oxide increases with the quantity of deposited carbon. This increase, however, may not be in a perfectly corresponding ratio, owing to the power of CO to reduce the newly-formed oxide, varying at different temperatures. The proportion of oxygen absorbed by the iron for each 100 parts of carbon deposited, is for—

A.	B.	C.
10.	6·6.	10.

So that, as the temperatures at which the operation has been carried on approach each other, as happened in the case of A and C, the ratio between carbon deposited and oxygen absorbed are the same, viz., about 7 equivalents of the former to 1 of the latter, and the equation would stand thus, provided the oxide of iron were protoxide, which is not certain:—

$$13CO + Fe = 7C + FeO + 6CO_2.$$

As the temperature rises, spongy iron exerts but little action on CO. Precipitated Fe_2O_3, after complete reduc-

* From the "Chemical Phenomena of Iron Smelting."

tion by H, was exposed to a stream of CO, at a bright red heat: after one hour's exposure, the composition of the substance was—

	Fe.	C.	O.
Expt. 337.	99·20	0·32	0·48
And after three hours more ..	99·34	0·30	0·36

In such cases as the present, either the CO_2, generated by the splitting up of CO quickly gasefies at high temperatures the deposited carbon directly, or, what is more probable, the acid itself is resolved into CO by yielding O to the metal, and the oxide of iron, so formed, is again reduced by carbon previously precipitated.

The presence of CO_2 in the CO of course intensifies the oxidising influence of the mixtures, and although some carbon may be deposited, the absorption of oxygen predominates.

(To be continued).

ON THE CULTIVATION OF MADDER IN DERBYSHIRE.*

By JOSEPH SIDEBOTHAM, F.R.A.S.

Several attempts have been made to cultivate madder in England and Ireland, but the records of the experiments are very meagre and unsatisfactory, and one can only judge their want of success from the fact that they are not repeated.

Being desirous of ascertaining the capabilities of our soil and climate for this branch of farming, and having suitable land at our disposal at Strines, Mr. Nevill and I determined to try the experiment, the results of which I have now the honour to lay before you.

In order to make the matter plain to those who do not understand the different qualities of madder, it is necessary to give a few words of explanation.

Madder is the root of *Rubia tinctoria* by some authorities supposed to be a mere cultivated variety of a plant indigenous to this country, and found wild in many places in the limestone districts on rocks and walls. This plant is cultivated for the purpose of dyeing in many parts of Europe and in India.

Its qualities vary much; that from Holland, called Dutch madder, will dye red, but not purple, and the colour is not fast; that from Italy, call Naples madder, dyes good reds and purples, but the colour is also loose; that from Turkey dyes good reds and purples, and is very fast; from France we get two qualities, called respectively rosés, from their dyeing beautiful reds and pinks, and paluds, the latter being a name given because the roots are grown on marshy land. The latter yield, besides the fine reds, also a good purple, nearly allied to that produced by Turkey roots.

For the purpose of the experiment we selected a piece of rich land, near the river, at Strines, a little less than an acre, and having prepared it in the usual manner, we had it sown with seed from fine palud madder, early in the spring of 1868. The weather was unusually dry and the ground produced a crop of remarkably fine *Polygonum aviculare*, which almost choked the young madder seedlings. (I am inclined to think the seed of this *Polygonum* were mixed with the madder seed.) In the autumn the madder plants came into flower, and the roots of some pulled up measured 13 inches in length.

The field was weeded, and the plants came up in the spring of 1869, very strong and healthy, and so on until August, 1871, when we had them dug up. To produce the best results the roots should have remained another year in the ground, but for the purpose of our experiment this growth was considered sufficient. As to yield, the quantity produced was small, probably owing to the very dry season after sowing; in appearance and size the roots were about equal to fine French roots, but on breaking them, instead of the deep red colour in the best French roots these were orange, or yellow.

The dyeing properties were of a very disappointing nature: out of the dye the colours looked full, but on being cleared with soap they were found to be loose, and precisely in character like Dutch madder, the reds and pinks being weak and loose, and the purple element entirely wanting. From a single experiment it would be unwise to do more than hazard an opinion, as more extended experiments might lead to other results; but I think it probable that the deficiency of colouring matter in these English madder roots is owing to a deficiency of sun and heat. It would not be easy in this country to select a more likely soil for the purpose than that of Strines, and the seed was obtained from a district where the best quality of French madder is grown.

It is said to be a fact that French seed, when sown in Holland does not produce a French quality of roots, but one similar in every way to the usual Dutch madder. This, if correct, would support my opinion.

I have here to illustrate this subject, specimens of the various madders in the root and ground state, also the colours produced by each, and the relative degree of fastness exhibited by portions of each being subjected to boiling soap.

* Read before the Manchester Literary and Philosophical Society, February 7th, 1871.

THE ACTION OF SULPHUROUS ACID ON PHOSPHATES.

By Dr. B. WILHELM GERLAND.

The researches which this paper describes lead to the following conclusions:—

(1). An aqueous solution of sulphur dioxide acts upon several phosphates, not by decomposing them, like other strong acids, but by combining with them, forming soluble compounds. Basic phosphates require from 4 to 6, and neutral phosphates 2 mols. of sulphur dioxide for solution. These solutions part less readily with their sulphur dioxide than the simple aqueous solution of the latter, and those of the neutral phosphates more easily than those of the basic phosphates. From some of these solutions the original phosphate can be again obtained, from others a less basic salt, but the decomposition in the solutions of this class does not proceed to the formation of phosphoric acid.

The following phosphates belonging to this class have been examined.

(*a*). Tricalcium phosphate is abundantly soluble in water and sulphur dioxide. The concentrated solutions undergo a slow decomposition at temperatures above 18° C., and form besides calcium sulphite, dicalcium- and monocalcium-phosphate. Both concentrated and dilute solutions deposit mixtures of calcium sulphite and dicalcium hydric phosphate by addition of alcohol, by exposure in vacuum, or by boiling under reduced pressure. Boiling under atmospheric pressure, on the other hand, causes the formation of the new compound: tricalcium phosphate sulphite, $Ca_3P_2O_8,SO_2,2H_2O$, as a crystalline precipitate which is distinguished from the above-mentioned mixtures of dicalcium phosphate and calcium sulphite by its great stability. It claims more general interest as being an active manure and disinfectant. The unusual composition of this substance made it desirable to prepare corresponding compounds of other metals, but all attempts in that direction have been unsuccessful.

Dicalcium hydric phosphate is readily soluble in water charged with sulphur dioxide. From the solution the original phosphate can be easily obtained.

* Read before the Manchester Literary and Philosophical Society March 7th, 1871.

(*b*). Trimagnesium-, dimagnesium-, and magnesium-ammonium- phosphate are dissolved in large quantities by water charged with sulphur dioxide; the first two without decomposition, but if an excess of the latter has been used, dimagnesium hydric-phosphate is left undissolved. All these solutions have a great tendency to deposit dimagnesium hydric-phosphate in crystals.

(*c*). Tri- and di- manganese phosphate are very soluble in sulphur dioxide and water. Both solutions give crystals *in vacuo*, consisting principally of dimanganese phosphate, but by boiling, precipitates of trimanganese phosphate are formed.

(*d*). Copper phosphate is soluble, although in smaller quantity, in an aqueous solution of sulphur dioxide without decomposition. The solution deposits at a summer temperature in course of time crystals of cuprous and cupric-sulphite, and by boiling, cupric phosphate.

(*e*). Uranium phosphate is very slightly soluble in water charged with sulphur dioxide. The phosphate of the original composition separates again from the solution after the removal of the sulphur dioxide.

(*f*). Crystals of trisodium phosphate absorb sulphur dioxide in such quantity that it would suffice to convert all the sodium present into sodium hydric sulphite. However, alcohol separates sodium dihydric phosphate from the solution, and less than 5-6ths of the sulphur dioxide are expelled by boiling. The concentrated solution obtained by saturating the crystals with the gas, shows the peculiar phenomenon of separating into two distinct liquids by gravitation; agitation unites these again to a perfectly homogeneous liquid.

(2). Sulphur dioxide in aqueous solution has no action upon bismuth-, stannous-, stannic-, and metastannic-phosphate.

(3). Sulphur dioxide and water act upon some phosphates in the same manner as other strong acids by forming a sulphite and phosphoric acid. The phosphates of barium, silver, and lead have been observed to undergo this decomposition.

(4). Calcium arsenite, calcium arseniate, and cupric vanadate are dissolved like the first group of phosphates, without decomposition by sulphur dioxide and water. The solution of the first forms calcium sulphite by boiling, the second begins soon to deposit calcium sulphate, owing to the reaction of arsenic acid on sulphurous acid, and the solution of the vanadate on boiling deposits beautiful golden coloured scales, which are, probably, copper vanadite sulphite.

(5). Calcium oxalate is dissolved, in very minute quantity, by water charged with sulphur dioxide, and is deposited unchanged after expulsion of the gas.

THE MAXIMUM DYNAMIC EFFECT OF A GALVANIC BATTERY.

By the Rev. H. HIGHTON, M.A.

I FIND that the views I have advocated in the CHEMICAL NEWS and *Quarterly Journal of Science* on this subject, have, as might have been expected, been attacked in America by Prof. Phin, in the *Technologist*, and ably, and in my opinion very successfully, defended by a writer who signs himself "Dynamics." But I find the same confusion prevailing on the other side of the Atlantic as on this, on the subject of the advantages of the equality of the external and internal resistances of a battery. Let me, then, briefly tabulate the true theory on this point.

1. If we have a given external resistance, and a given number of cells, or given area of (say) zinc and platinum, the greatest *intensity* of current is gained by so arranging the cells, or cutting up the area of zinc and platinum into so many parts, that the external resistance shall be equal to the internal. Also this arrangement gives the greatest heat in the *external* circuit, but not in the *total* circuit.

(2). The greatest *heat* in a given time is produced in the *whole* circuit (on the same data) with the cells arranged wholly for intensity, or the zinc and platinum cut into as many pieces as possible.

(3). If we have a given battery working in a certain manner, the greatest *intensity*, and also the greatest heat in a given time in the whole circuit is produced by the external wire being as short and thick as possible.

(4). On the same data the greatest *heat* in a given time is produced in the external part of the circuit when the external and internal resistances are equal.

(5). The greatest *magnetic power* is produced when the external wire is as long and thick as possible.

I will not go through the mathematical proofs of these five rules, but will leave it for mathematical electricians to work them out each for himself.

ON THE VARIOUS TINTS OF AUTUMNAL FOLIAGE.

By H. C. SORBY, F.R.S., &c.

IN the following paper I shall endeavour to explain the production of all that variety of colour which imparts such a charm to woodland scenery in autumn. I must, however, frankly admit that very much yet remains to be learned. The complete study of the question would involve very much research, and I see the importance of examining the colouring matters in spring and summer now that it is too late. Still, however, I trust that I shall be able to give a tolerably satisfactory general account of the subject, and, perhaps, little more is desirable on this occasion, since that may interest many who would not care to occupy their time in studying in detail the optical characters of the colouring matters found in leaves. These are certainly very numerous, and I have even so far established the existence of about a score, though I have examined only a few dozen plants with the requisite care. These have, however, been chosen in such a manner as to show the more striking phenomena, and probably a more extended examination would merely reveal a greater number of different colours, without materially altering the general results.

In the first place, I must say that it appears very desirable to divide the various colouring matters into different groups or genera, each of which includes a number of distinct substances or species having some well-marked peculiarity in common. I shall not attempt to give anything like a complete account of the characteristic difference of the various species, since that would involve a long and tedious description of minute particulars, and shall confine my remarks to such prominent facts as are of importance in the subject more especially before us, and can be described without illustrations or very technical notation. I scarcely need say that such an inquiry could not possibly be carried out by any other than the spectrum method. Chemical analysis would be of very little use, and might easily lead us to conclude that different substances were the same, and the same different. It also is especially useful in studying the complicated mixtures with which we have to deal, since particular substances can be easily recognised when it would be quite impossible to obtain them in a separate state. For a more complete description of this method of research I beg to refer to what I have already published on animal and vegetable colouring matters,* and on some technical applitions of the spectrum microscope.† I may also say that, spending, as I do, several hours nearly every day amongst the woods, fields, and moors, I have had good opportunity for studying the application of such inquiries to the subject before us.

* *Proceedings of the Royal Society*, vol. xv., p. 433.

† *Quarterly Journal of Microscopical Science*, New Series, vol. ix., p. 358.

The group of colouring matters which first of all claims our attention is that which may be distinguished by the term *chlorophyll*. It has often been treated as if it were one simple substance, but optical examination proves the existence of a number of separate species. The leaves of most plants are coloured green by a mixture of two or more of these. One kind occurs in a state of comparative purity in the small aquatic plants allied to *Oscillaria*, and the green leaves of trees appear to contain this along with one which gives special absorption-bands. Another is the product of the action of acids on these, and occurs naturally in some leaves, especially when turned brown in autumn, and this gives rise to a very special spectrum with numerous bands. A fourth, found in faded *Confervæ*, is closely related to the last, but differs in gradually turning to a deep blue colour, when hydrochloric acid is added to the alcoholic solution. All these have the following peculiarities in common—they are insoluble in water, but soluble in alcohol, or bisulphide of carbon : the spectra have all a very well-worked absorption-band in the red, but the green more or less completely transmitted, so that the prevailing tint is a more or less modified green.

The second class of colouring matters may be described as the *xanthophyll* group. These are characterised by being insoluble in water, but soluble in alcohol and in bisulphide of carbon ; the spectra show absorption at the blue end, often with more or less well-marked narrow-bands, but the red, yellow, and yellow-green are freely transmitted, so that the general colour is clear yellow or orange. The different species are distinguished by the character and position of the absorption-bands, which are best seen when the colour is dissolved in bisulphide of carbon. A considerable number are found in various fruits, flowers, and roots, but only two are so commonly met with in leaves as to claim attention in this paper. These appear to be the same as the two which give rise to the difference in the colour of the yellow interior and the orange exterior of some carrots. They may be obtained by dissolving in hot alcohol, and agitating the cold solution with excess of bisulphide of carbon, which subsides to the bottom with more or less of the colour, and leaves in the alcohol all other substances soluble in water. Both give spectra with two rather obscure absorption-bands, which lie further from the blue end in the case of colour from the external layer of the carrot, and the colour of this is orange, and of the other yellow. This latter is the kind most commonly met with in yellow leaves, from which it may be obtained in the manner just described, and when nearly pure it is of the same tint as gamboge. The orange colour is more rare, but occurs in leaves fading to a deeper and more orange-yellow, as, for instance, in those of the india-rubber tree, to which it gives a tint closely corresponding to that of Indian-yellow, It also occurs in a more pure state in the ripe envelope of the fruit of the common winter cherry (*Physalis Alkikengi*), to which it gives a still more orange-coloured tint, approximating to that of the exterior layer of the carrot. There may be some other colours besides these having bands in intermediate situations, but, on the whole, I am disposed to regard them as variable mixtures of the two just described.

Since the name of *erythrophyll* has been already applied to the red colour of leaves in autumn, it will be best to adopt it as that of a group containing a number of different species. These may be said to be characterised by their more or less red colour, which is made more intense by acids, and more purple, blue, or green by alkalies. This is because there is a strong absorption in the green part of the spectrum, and the broad band is raised towards the blue end by acids, and lowered towards the red by alkalies, which also often increase the absorption at the blue end, so as to make the colour green, though I am much inclined to believe that, in most cases, this is due to the presence of a second yellow-coloured substance, so that a mere difference in colour is no proof that the red colours differ. Usually, but not invariably, they are soluble in water and aqueous alcohol, but not in bisulphide of carbon. Very many species are met with in fruits, flowers, and roots, distinguished by their spectra, either in their natural state or when acted upon by various reagents, and so far I have found at least six in leaves. That which gives rise to the red patches in the beautiful, variegated leaves of some of the geraniums of our gardens, is the same as that met with in the flowers of particular species. The purple colour of the leaves of turnips is the same as that of the purple flowers of the common garden stock. The colour of red cabbage has well-marked peculiarities, and so has that of the root and leaves of the beet. The dark leaves of *Tamus communis* contain another distinct colour, and so do those of the purple beech, but all these are normal constituents of the young leaves of particular varieties of the plants, and not simply developed towards autumn. It is, however, impossible to draw a line between the two cases, since the colour which gives rise to the dark brown tint of heath in autumn appears to be the same as that of the purple beech, and that which occurs in the dark leaves of ivy seems to correspond with the fine deep pink colour developed in many leaves only in autumn, so as to give rise to the splendid red and scarlet, which produce such a fine effect on certain kinds of scenery.

In order to obtain these red colouring matters in a satisfactory state for experiment, the leaves should be boiled in alcohol, which dissolves chlorophyll, xanthophyll, and the reds ; but, as I have already described in previous papers, the alcoholic solution of most of them rapidly fades, so that the solution is only of a dirty green or yellow tint. On evaporating it to dryness, the splendid red colour chiefly collects round the edges, and the chlorophyll and xanthophyll are deposited more in the centre, so that we can immediately see that there is a mixture. By re-dissolving in water, the chlorophyll and xanthcphyll are left insoluble, and the erythrophyll is obtained in solution, and on gentle evaporation is left in the state of a dry gum, which in some cases remains almost unchanged for months or even years. It must, however, be borne in mind that, as thus prepared, the erythrophyll must necessarily contain a variable amount of the colours described below, and it is no doubt to their presence that some of the reactions are due, which both myself and others have referred to the red colour itself. Thus, for instance, when they are slightly oxidised, the broad absorption-band is lowered towards the red end, and by further oxidation, the colour becomes more or less orange-yellow, just in the manner that the colour of dark grapes is changed into that of new wine, and this in time to that of very old, as described by me in a paper already cited, but I am now inclined to believe that this further oxidation, which destroys the main absorption-band, extending over the yellow and green, does really completely destroy the colour of the red substance, and that the more or less orange-yellow is due to the oxidisation of a pale yellow colour previously obscured by the deeper red. This fact is of considerable importance in the subject before us, since it explains why intensely red leaves fade to almost, if not exactly, the same tint as those of the like kind which were previously not at all red ; that colour being so completely destroyed as to produce no effect on the tints subsequently developed.

The fourth group of colours is composed of those soluble in water and aqueous alcohol, but insoluble in bisulphide of carbon, which have a sufficiently decided gold yellow-colour to justify my distinguishing them by the term *chrysophyll* group. They vary somewhat in tint from a little more yellow, to a little more red, than yellow ochre. They are made darker and more orange by oxidisation, and thus are in an unoxidised condition as compared with the colours of the next group. In order to prepare them, the leaves should be boiled in alcohol, and after evaporation to dryness at a gentle heat, the soluble portion re-dissolved in water. I have so far met with at least four different species, distinguished by the spectra which they yield on partial oxidisation. The most satisfactory method is to dissolve some of the colour in a small quantity of water,

dilute this with alcohol, and then to add a little nitrite of potash and hydrochloric acid. In some cases this gives rise to one or more well-marked absorption-bands, and changes the colour from yellow to pink. In others no bands are developed, but the colour is altered from pale-yellow to deep orange-red. On evaporating to dryness, we obtain colours which vary in tint from that of "light red" to those of burnt umber and raw sienna.

The fifth group of colours consists for the most part of various browns, and, therefore, I propose to distinguish it by the term *phaiophyll* group. In most cases they are due to the oxidisation of chrysophyll or other previously existing soluble compounds, as may be proved artificially. There must be, at all events, several colours of this group, but their accurate determination is difficult because they do not give well-defined absorption-bands. On the whole they may be said to be soluble in water and not in bisulphide of carbon, but, in some cases, water alone dissolves them very sparingly, and they are more soluble in dilute alcohol, along with an acid.

When leaves pass into complete decay they turn dark brown, and ultimately become nearly black. This is evidently due to the formation of dark coloured substances allied to humus, but their accurate determination would be very difficult, and I have not yet studied them very much. Though it may be convenient for our present purpose to separate these more or less black colours from the brighter browns of the phaiophyll group, yet I am by no means convinced that there is any actual distinction between them. They are no doubt produced by the decomposition of most varied compounds, both soluble and insoluble; and since it is, perhaps, impossible to obtain these in a pure state, it is difficult to ascertain the exact connection between the various unaltered and altered products.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

March 16th, 1871.

Professor Williamson, F.R.S., President, in the Chair.

Mr. C. H. Piesse was elected a Fellow.

Mr. C. Haughton Gill read some notes "*On the Examination of Glucose-Containing Sugars.*" Those engaged in the examination of low sugars and molasses have frequently had to complain of obtaining quite unintelligible results. It is well known that the solution of the sugary body is decolourised and clarified by the addition of basic lead acetate before submitting it to optical examination; but Mr. Gill finds that the power of invert sugar to rotate a ray of polarised light is so greatly altered by the presence of this reagent, that the results obtained by the so-called polarisation of syrups containing much invert sugar are worthless when the clarification has been effected in the ordinary way. The alteration of rotatory power of pure invert sugar by basic lead acetate is shown by the following experiments:—

15 c.c. of a solution of invert sugar made up to 50 c.c. by water, observed with a Soliel's saccharometer* in tube of 20, read −28·25 at 24° C.

15 c.c. of a solution of invert sugar with water and 2 c.c. of saturated solution of basic lead acetate, observed with a Soliel's saccharometer in tube of 20, read −24·7 at 24° C.

15 c.c. of a solution of basic acetate solution alone, observed with a Soliel's saccharometer in tube of 20, read +57 at 25° C.

* The readings of this instrument $\times \frac{25{\cdot}8}{100}$ = angular degrees.

These results have been confirmed by many other observations.

The alteration producing this reversal of rotatory power takes place only on the *levulose* of the liquid; the *dextrose* suffers no change of optical properties.

A solution of pure *dextrose* prepared from invert sugar, and reading 60·3, made up to 2 vols. by strong solution of basic lead acetate, read 30·5.

A solution of nearly pure levulose prepared by Dubrunfaut's method, and reading −44 at 20° C, made up to 2 vols. by solution of basic lead acetate, read +6 at 20° C.

The alteration of the rotatory power of levulose is not permanent. On removal of the lead, or on acidifying the liquid, the original rotatory power is restored. The alteration is not due to the alkalinity of the lead solution as regards alkalinity alone, for weak soda or ammonia produce no such change till they begin to decompose and destroy the sugar. It is probable that a soluble lead compound of levulose possessed of dextro-rotatory power is formed.

Now, when a sugar solution containing invert sugar is clarified by basic lead acetate, the invert sugar loses, in part or in whole, its levo-rotatory power, and the first direct reading is too high. When the liquid is acidified and inverted by heat, the original invert sugar has its true levo-rotatory power restored and added to that of the invert sugar proceeding from the cane sugar, thus producing a greater "difference" in the readings than that due to the cane sugar alone, and consequently leading to too high a result.

The remedy for this difficulty is to remove the lead and acidify the liquid before making the first reading. For this purpose Mr. Gill uses a strong solution of sulphuric dioxide, which possesses the advantages of removing the lead and bleaching the liquid at the same time, while it is incapable of inverting cane sugar in the cold even in twenty-four hours. The decolourising effect is so great that even the worst treacles give liquids of a pale straw colour when thus treated, and, moreover, "inversion" can afterwards be performed without any fear of spoiling the colour, whereas by the ordinary method the liquid frequently becomes too red to allow of optical examination.

Another error also arises from the use of the lead-salt as a clarifier for those sugar solutions in which glucose is to be estimated by the use of Fehling's copper solution. The presence of lead here leads to a result much too low, since it also becomes partly reduced, and thereby necessitates the use of a greater volume of the saccharine solution which is called on to reduce lead as well as the known amount of copper. Sulphuric dioxide serves to remove the lead, while excess of the reagent exerts no other action on the copper solution than that of facilitating the subsidence of the cuprous oxide.

As illustrating the extent of the error which may be introduced by the presence of lead, the following experiment, selected from many others, may be taken (solutions of invert sugar of the same strength used in each case):—

Volume required to precipitate cuprous oxide from 10 c.c. of Fehling's liquid—

(1). Free from foreign bodies, 10 c.c.

(2.) Containing 10 per cent of its volume of solution of basic lead acetate, 17 c.c.

Mr. D. Howard made some remarks "*On the Boiling-Point of a Mixture of Amylic Alcohol and Water.*" The mixture boils at a lower degree than pure water; thus illustrating the rule anounced by a German chemist (Kopp ?), according to which a mixture of two liquids boils at a lower temperature than either of the component liquids separately. In the experiment above mentioned, Mr. Howard ascribes it to the circumstance that, probably, the alcohol and the water form, when heated, two separate layers.

Mr. W. H. Perkin, F.R.S., briefly announced that he had succeeded in obtaining bromacetic acid by digesting and heating bromine with acetic anhydride, subsequent addition of water, and then distillation.

Mr. Warington then made a verbal communication on the "*Commercial Estimation of Sulphocyanides.*" Sulphocyanide of ammonium was frequently present in commercial salts of ammonia, and, being injurious to vegetation, its estimation was often of considerable importance. The reaction of sulphocyanides with ferric salts suggested itself as a possible means of determination; but the depth of colour produced was so influenced by the proportion of iron present, by the proportion of free acid, and, indeed, by the kind and quantity of *all* the salts present in the solution, that, at first sight, a method founded on this reaction seemed hopeless. Many of the commercial ammonia salts, were, however, so nearly constant in composition that it was possible to make use of the reaction referred to. The method adopted was to dissolve in one beaker a known weight of the ammonia salt to be examined, and in a second beaker a quantity of a pure ammonia salt—being the amount of pure salt the sample was supposed to contain. To each beaker was then added equal volumes of ferric chloride and hydrochloric acid. The conditions in the two beakers being now approximately the same (saving the presence of sulphocyanide in the first beaker), a standard solution of sulphocyanide of ammonium was added to the second beaker until the colour in the two beakers appeared equal. The method was, of course, of limited application, but where it could be used possessed the advantage of great speed.

In answer to Dr. Müller, Mr. Warington said that the highest percentage of cyanogen he had observed in the "patent ammonias" was four per cent.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 7th, 1871.

The first paper was on "*The Action of Sulphurous Acid on Phosphates*," by Dr. R. W. Gerland. (See p. 136.)

"*Further Observations on the Strength of Garden Nails*," by J. P. Joule, LL.D., F.R.S. (Will appear in our next).

Dr. Joule exhibited three photographs of the sun taken on the 1st of December, 1858. The images, 0·43 inch diameter, were produced by the achromatic object-glass of a telescope with half-inch stop. Exposure, by means of an apparatus completely detached from the camera, during a small fraction of a second. He had been induced to examine them after seeing the beautiful photograph of the late eclipse by Mr. Brothers. On examining the three images a nebulosity is observed, very similar to that in Mr. Brothers' photograph. In all three, taken at an interval between each of about a minute and a half, the nebulous appearance appears situated on three quarters of the limb, the remainder being quite free. There are also indications of a radial structure, so that he thinks it highly probable that the representations are actually those of the corona.

Since communicating the above, he has carefully examined the two other photographs of the sun which he possesses, and which were taken early in the month of November, 1858. These, one of which must have been exposed at about two hours twenty minutes after the other, present nothing remarkable to the naked eye; but when viewed through a glass of moderate power, a thin crescent-shaped envelope is observed on each, with this remarkable circumstance, viz., that in the two it appears on opposite limbs, suggesting the idea of a semi-revolution in the above interval of time at a velocity not much less than that due to Kepler's law of planetary motion. In one of the photographs there is, under the crescent and apparently on the rim of the sun itself, a narrow band, in breadth about 1-300th of the diameter of the disk, and of at least double the intensity of the sun. This may probably be referred to the actinic action of the chromosphere and the red flames.

"*On Anthraflavic Acid, a Yellow Colouring Matter accompanying Artificial Alizarine*," by Edward Schunck, Ph.D., F.R.S., V.P. (In our next).

The President stated that in looking over the Memorandum Book of Mr. Walker, one of the original members of this Society, kindly presented by Mr. Green, he had met with some interesting facts connected with the cotton trade a century ago. At that time the only places from which Manchester received cotton, except from continental ports, were Turkey, the West Indies, Brazil, and Demerara. Several extracts from the book were given.

NOTICES OF BOOKS.

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By Richard A. Proctor, B.A., F.R.A.S. Longmans, Green, and Co.

The labours of the Herschels, Schwabe, Carrington, Secchi, De la Rue, Stewart, Huggins, Fraünhofer, and Lockyer in astronomy and spectroscopic chemistry are known to the popular reader but by name only, and works are much wanted which shall render their additions to the knowledge of the grand movements beyond our own globe familiar to the ordinary reader. This familiarity can only be gained by the avoidance of technicality. The general reader seldom has the time, and, perhaps, the inclination to make himself acquainted with technical terms. Such a view Mr. Proctor, it seems, had in composing his work on "The Sun," considered as "Ruler, Fire, and Life of the Planetary System;" but while dispensing with technicalities, or defining those absolutely necessary, he has aimed at pleasing the advanced student as well as the general reader.

The work is an admirable compilation of the particulars of discoveries and of modern theories advanced with regard to the centre of our planetary system. Discarding the imaginative theories of the ancients as being truly unworthy of his space, the author opens his work with a history of the methods used in estimating the distance of the sun from the earth. How the present measurements have been obtained is explained in a most lucid and entertaining chapter. But by far the largest and best portion of the work is devoted to the spectroscopic analysis of the sun's rays; and in this branch Mr. Proctor not only details the labours of others, but suggests valuable improvements in the arrangement of a twice-acting double series of prisms on Browning's automatic system. This and the remainder of the work is carried out very fully; even the method of calculating the velocity of approaching or receding bodies by the differentiation of their spectra is rendered intelligible on the most superficial perusal. Why should not the work have been entirely in this manner, and the cramming of valuable information into foot-notes expunged. The considerations of the transits of 1874 and 1882 are gone into with great expectation of what astronomers by their means look for in the correction of the present distance measurements. Finally, the author presents "reasons for considering that the most important work science has to accomplish is to show how the sun's action can be more fully utilised than it is at present, so that before the earth's stores of force are exhausted (as they must some day be), resources which can never be exhausted—because unceasingly restored—may be rendered available. . . . . Nearly all our motive force is obtained from *stored* sun-force. The machine draws upon the earth's *garnered* stores, while the living worker draws upon the earth's *periodical supplies* of force. In the former case, that is being used up which

cannot be replaced; in the latter, what is consumed will be restored in the ordinary course of nature. We are now utilising (in the products of our coal-fields) what Professor Tyndall calls the sun of the carboniferous epoch: our descendants will have to employ the sun of their own epoch. The heat of the solar rays—mayhap also their light and their actinic energy—must one day be applied to work our machinery." In all, to the scientific student this book cannot convey much, but to the general reader it may be the key to a wider course in the all-engrossing subject of which it treats. A word must be spared to commend the photolithographs with which the matter is interspersed.

CORRESPONDENCE.

PHOSPHATE PROCESS FOR THE UTILISATION OF SEWAGE.

To the Editor of the Chemical News.

Sir,—Mr. Forbes, in his paper on the "Phosphate Process for the Utilisation of Sewage," which appeared in the Chemical News of the 10th inst. (vol. xxiii., p. 112), quotes Dr. A. Voelcker's analysis of the dried sewage deposit prepared by this process, and valued by him at £7 7s. per ton.

Would Dr. Voelcker kindly say whether any, and how much, of the 28·52 per cent of phosphoric acid is combined with the 29·95 per cent of alumina and oxide of iron, and if so, whether the value of the deposit as a manure would not be greatly reduced thereby.—I am, &c,,

Superphosphate.

Royal College of Chemistry, London, W.
March 20, 1871.

THE DEFINITION OF ALLOYS, &c.

To the Editor of the Chemical News.

Sir,—The student who considers the class of bodies known under the name of alloys, amalgams, and such mixtures as acids or alcohol, &c., with water, cannot rest satisfied with the definition that they are chemical compounds, as they do not answer the law of combination in multiple proportions.

That they cannot be called mechanical mixtures, the fact that only the forces at the command of the chemist can separate them into their constituents fully proves. By no merely mechanical means could alcohol be separated from water or the zinc from brass; heat would, in both cases, play the principal part—and this force belongs to the domain of chemistry.

Seeing, then, that these bodies will mix in any proportions, but still, in some respects, retaining the character of chemical compounds, can any objection be raised to their being defined as chemical mixtures? The term seems a fit one to rank between chemical compounds and mechanical mixtures—the place always assigned to the combined bodies in question.

By calling them mixtures the idea is at once discarded of their components being in any definite proportions, and the term chemical fully expresses the intimate nature of the mixture which only chemical forces can separate.

The idea of "chemical mixtures" may not be new, but never having met with the term myself (possibly others of your readers may be in the same case), and if these remarks draw out any opinions throwing light on a still somewhat obscure subject their object will have been fully attained.—I am, &c.,

F. J. R. C.

MISCELLANEOUS.

The Chemical Society.—The Anniversary Meeting of this Society will take place on Thursday next, March 30th, at 8 p.m., when Officers and Council for ensuing year will be elected, and the retiring president, Dr. A. W. Williamson, F.R.S., will deliver the annual address.

Quekett Microscopical Club.—The annual *conversazione* of this Club took place at University College on Friday evening, and was very largely attended, as it usually is. The objects provided by the Club for the entertainment of its guests comprised all the optical novelties of the year, and the Members, as well as the leading opticians, did all in their power to exhibit objects worthy of the position the Club holds in the encouragement of microscopical science. Photography was on this, as at the last annual *soirée*, well represented. A large and interesting series of photographs of Indian temples and scenery was kindly lent by the India Office; also frames of photographs were lent by Mr. J. Van Voorst, Mr. John Foster, Mr. E. Kiddle, and Mr. A. Shapcott. Mr. Apps exhibited at frequent intervals the marvellous electrical effects produced by means of his well-known induction coil. In the midst of so many attractions, it is difficult to single out for especial mention any one feature of interest; but that which seemed possessed at this time of surpassing interest was an exhibition on the screen, by the oxyhydrogen light, of a series of transparent photographs illustrative of the scenery of the late lamentable Franco-Prussian war, contributed by the London Stereoscopic Company, with an explanatory lecture by Mr. James Martin, and which commanded crowded audiences all the evening.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus des Séances de l'Académie des Sciences.

We are now able to resume our abstracts of this weekly periodical, which was regularly printed during the siege of Paris. These numbers contain the following papers relating to chemistry and collateral sciences;—

September 19, 1870.

Salubrity of Soil and of Water.—Professor Chevreul.—This lengthy memoir contains, in aphorismatical style, a series of propositions on the real conditions required for keeping arable soil and all plant-bearing soil, in such a state as to admit of its being the proper medium for plants to grow in, the main point being the possibility of abundant access of oxygen, and of its permanent presence between the solid particles of which the soil is made up; while, as regards water, when considered as a proper medium for the organised beings living therein (plants or animals), the same holds good, and only while containing a large excess of oxygen is that liquid suitable for drinking and other domestic purposes.

September 26, 1870.

The greater part of this number is devoted to a discussion which arose in reference to a paper on the—

Alimentation (Means of Food Supply) to the Inhabitants of an Invested and Besieged Town.—G. Grimaud.—The paper itself, and the ensuing discussion, contains many interesting, and chiefly historical, facts relating to the antiquity of panification and the varieties of food made from grain (oats, barley, rye, rice, wheat) in different countries.

Aurora Borealis of September 24th, 1870.—M. Chapelas.—An account of observations made on this phenomenon as seen at Paris.

October 3, 1870.

New Compounds Resulting from the Union of Cyanic Acid and of Different Cyanic Ethers with the Ethers of Amidated (Amidés) Acids of the Aromatic Series.—A. Cahours and H. Gal.—After referring to the experiments of Dr. P. Griess, and also of the first-named of the joint authors of this paper, made some twelve years ago (see *Ann. de Chimie et de Physique*, 3rd series, vol. lxiii., p. 322), an account is given in this lengthy paper of the mode of preparation, constitution, and properties of very complex substances obtained by the reaction of cyanic acid upon the ethers of the amidated acids, and also of different cyanic ethers with the other ethers just mentioned. As the authors promise, in a subsequent paper, a complete and full account of their labours, we will not give further details now.

Observations on Panification.—M. Mège-Mouriès.

Best Means of Applying Wheaten Flour to the Preparation of Food other than Bread yet Possessed of Nutritive Quality.—L. Aubert.—Both these papers contain valuable hints relating to domestic economy and the making the most of victualling materials.

October 10, 1870.

This number is entirely devoted to the subjects of food supply and preservation of food. An excellent *résumé* of what has been done in this direction.

October 17, 1870.

Difference and Analogy of the à posteriori Experimental Method as Applied to Concrete Sciences and to Moral and Political Sciences.—Prof. Chevreul.—A lengthy lecture by this eminent *savant*.

Process Employed by the Indians of the Territory of the United States for the Preparation (viz., Tanning, Currying, Tawing) of the Hides of Bisons, Deer, and other Animals met with in that Extensive Country.—J. Simonin.—This paper records some curious historical facts bearing upon the primitive, yet effective, methods resorted to by the semi-savages alluded to.

The subjects of aërostatics and air balloons are fully treated in this number.

October 24, 1870.

Total Eclipse of the Sun of December 22nd, 1870.—M. Janssen.—A letter to the President of the Academy.

An Experiment which Confirms the Double Hypothesis set forth by Ampère concerning the Existence of a Closed Electric Current in every Molecule of a Magnetical Substance and in the Earth.—P. Le Cordier.—A mathematico-physical essay.

Relation between Astronomy and Geology.—S. Meunier.

October 31, 1870.

The Use that can be made of Osseine as Food.—E. Fremy.—The author calls osseine the organic matter of bones after the lime salts and other mineral matter contained therein has been removed. The quantity of osseine in fresh bones of slaughtered animals amounts to about 35 per cent of their weight. Osseine should not be confused with gelatine, which latter is a product of the prolonged action of heat and water upon bones, and also differs from osseine as not having an organised structure.

Aurora Borealis of October 24—25, 1870.—Three papers, respectively written by MM. Chapelas, Salicis, and A. Guillemin.

Rapid Method of Tanning Hides and Skins, as carried on in Mexico.—Virlet d'Aoust.—This paper contains an account of a method of tanning goat and other skins by the Indians (Aborigines) of the country alluded to. It appears that they form, with the brains of the animal, and certain vegetable products locally called *cascalotè* and *huizachè* (substances containing a large proportion of tannic acid), an emulsion which is forced into the skins by means of squeezing between the hands, or, more rapidly, by stamping with the bare feet. In some parts of Mexico, 4000 or 6000 goats are killed at one time simply for the skin and for boiling down for fat, the blood, flesh, bones, and offal simply being applied as manure.

Common Origin of Serpentine and Chantonnite.—S. Meunier.—The contents of this paper really bear upon the analogy existing between serpentine and a mineral of meteoric origin, called chantonnite, from the name of Chantonnay (France), where a meteorite containing the mineral fell in the year 1812.

This number also contains several papers relating to aërostatics and air balloons, and a curious contribution to historical zoology, on the—

Animals Employed by the Ancient Egyptians for the Purposes of Catching Game.—F. Lenormant.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 2, 1871.

Mathematical Foundation of the Law of Avogadro.—F. Mohr.—This essay and the following, by the same author, on the—

Unequal Conductivity of Gases for Heat—Are, notwithstanding their intrinsic value, not well suited for abstraction.

Sensitiveness for Light of Ferricyanide of Potassium.—H. Vogel.—After briefly referring to the well-known fact that the salt alluded to, as already observed by its discoverer, the late Dr. Gmelin, does not keep well in aqueous solution, the author states that (organic matter apart) the decomposition of the aqueous solution of ferricyanide of potassium is chiefly due to the light, the yellow rays excepted; since, if the solution is kept in yellow-coloured glass bottles, or absolutely in the dark (provided the water be free from organic matter, no decomposition takes place. As a positive proof of this fact, the statement is made that even an exposure of the solution of the salt to direct sunlight for thirty seconds is sufficient to cause the salt to yield with perchloride of iron a blue precipitate, and with salts of uranium the well-known brown colour of ferrocyanide of uranium. The author states that he has tried, with moderately good success, to apply this property of ferricyanide of potasssium for photographic purposes.

Lecture Experiments.—V. Wartha.—Illustrated by a woodcut.

Naphthyl-Purpuric Acid and its Derivatives.—E. von Sommaruga.—The large number, great length, and complexity of the formulæ, essential to the proper understanding of this paper, render its abstraction impracticable.

Action of Chloride of Sulphur upon Aniline in the Presence of Sulphide of Carbon.—A. Claus and W. Krall.—This paper contains the account of the continued researches of the authors on the above-named subject. It appears that the reactions first described by them must be somewhat modified. In the first phase, 2 molecules of chloride of sulphur, 4 of aniline, and 1 of sulphide of carbon, become decomposed according to the undermentioned formula, and formation of sulpho-carbanilide, hydrochlorate of aniline, and separation of sulphur—

$$4(C_6H_7N)+2SCl+CS_2=C_{13}H_{12}N_2S+2(C_6H_7N.HCl)+3S.$$

In the second phase of the reaction, the as yet undecomposed chloride of sulphur acts upon the first created sulpho-carbanilide, giving rise to the formation of triphenyl-guanidine and phenylsenfoil according to the following formula:—

$$2(C_{13}H_{12}N_2S)+2SCl=C_{19}H_{17}N_3.HCl+C_7H_5NS+HCl+3S.$$

Researches on the Ether Derivatives of the Polyatomic Alcohols and Acids.—L. Henry.—The sixth portion of this lengthy memoir, treating on the action of pentachloride of phosphorus upon chloral-ethyl-alcoholate.

Isomorphism of Calcareous Spar and Nitrate of Soda.—G. Rose.—This paper contains a few observations on the researches and experiments on this subject by M. L. Meyer, already given in the CHEMICAL NEWS. The eminent *savant* points out that, however correct M. Meyer's observations be, they are not new, since as far back as 1854 (*Comptes Rendus*, vol. xxxviii., p. 105), Dr. Sénarmont made the same experiments with the same result, and the late Prof. Mitscherlich repeated the experiment, not, however, with calcareous spar, but with a piece of dolomite from Traversella. The preparation made by the last-named *savant* is now in the Museum of Minerals at Berlin.

Constance of Colour and Intensity of the Light Emitted by the Clouds in its bearing upon Chromometry.—A. Müller.

By-Product of the Manufacture of Artificial Alizarine.—C. Liebermann.—The author states that, if the action of the potassa, during the fusion along with anthrachinon sulpho-acid, is modified by mixing with the alkali, in different substances, salt and chalk, there is obtained a body containing C, H, and O, entirely different from alizarine, but possessed of the property of being converted into that body by fusion with pure caustic alkalies. This compound is always present to some extent in all artificially-made alizarine, from which it can be separated by baryta water, wherein it dissolves, imparting to it a reddish brown colour. This substance, precipitated from the solution in baryta water by means of HCl, yields, after purifying by solution in glacial acetic acid, a crystalline yellow-coloured matter. According to the researches made by M. Caro, this substance resembles, in its properties, Rochleder's isoalizarine, but analysis proved the substance to be mono-oxyanthrachinon—

$$C_{14}H_7O_2OH$$

Compounds of Sulphuric Anhydride.—C. Schultz-Sellack.—This lengthy essay contains the following sections:—Combinations of sulphates; combinations of the haloid salts with nitrates; combinations of selenium, tellurium, and iodine.

New Method of Separating Gold and Silver from each other on the Large Scale.—F. Gutzkow.—Reserved for full translation.

Preliminary Notices.—L. Darmstädter.—A brief, and as yet incomplete, account of some experiments on the action of nitric acid upon bichromate of potassa.

Hydrate of Sulphide of Carbon.—M. Ballo.—The main gist of this lengthy paper is, that the statement made of the existence of solidified sulphide of carbon (frozen sulphide) is not correct, and is explained by the existence of a hydrate of the body alluded to, CS_2+H_2O containing about 19·14 per cent of water.

Preliminary Notice on the Action of Chloride of Zinc, Nitrous Acid, Bleaching Powder, and Hydrochloric Acid upon Morphia, and the Action of Chloride of Zinc upon Papaverine.—E. L. Mayer.

Journal für Praktische Chemie, No. 2, 1871.

Constitution of Ultramarine.—W. Stein.—This essay treats on the mode of combination in which sulphur is present in artificially

made ultramarine. While admitting the difficulties of fully elucidating this point, the author states that his aim is to prove that blue ultramarine contains sulphurous, not hyposulphurous acid, which former, however, is not an essential constituent; and further, that ultramarine contains only sulphuret of aluminium, there being no sulphuret of sodium at all. To give the experimental proof of these statements, the essay treats at length the following points:—Testing for hyposulphurous and sulphurous acids; testing for sulphurets; existence of sulphuret of aluminium in two different modifications. The conclusions drawn by the author from his experiments are—Ultramarine consists mainly of a white mass, through which and with which black sulphuret of aluminium is most intimately and molecularly incorporated; the blue colour of ultramarine is, therefore, not due to its chemical composition, but to the optical relation of its component substances. The essay winds up with some observations on white and green ultramarine; the latter contains less soda than the blue-coloured pigment, and that, again, less soda than the white pigment. The quantity of sulphur contained in blue-coloured ultramarine is less than that contained in green-coloured ultramarine.

Chinon-Like Derivatives from Thymol.—E. Carstanjen.—This exhaustive memoir contains the following sections:—On thymoïle; action of bromine on thymochinon; mono-bromo-thymo-chinon, $C_{10}H_{11}Br(O_2'')$; oxythymo-chinon, $C_{10}H_{11}(HO)(O_2'')$; constitution of thymol and of its chinon-like derivatives; diamido-thymol.

Contribution to the History of the Knowledge of the Constitution of Diglycolic Acid and of the Di- and Tri-Glycolamid Acids.—W. Heintz.—This paper and the following—

Observations on M. Heintz's Foregoing Essay—Dr. H. Kolbe.—Are, however valuable, not well suited for abstraction.

New Method of Converting the Fatty Acids into the therewith Corresponding Alcohols.—A. Saytzeff.—This paper is divided into the following sections:—Introduction, treating on the isomerism and constitution of alcohols, more especially butylic alcohols; conversion of chloracetyl into ethylic alcohol; conversion of chlor-propionyl into normal propylic alcohol; conversion of chlor-butyl into normal butylic alcohol.

Behaviour of Normal Iodide of Butyl towards Alcoholic Caustic Potassa Solution.—A. Saytzeff.—The first, and as yet incomplete, portion of an essay on this subject.

Polytechnisches Journal von Dingler, first number for February, 1871.

Simple and Ready Method for Determining the Magnifying Power and the Range of Vision of a Spy-Glass and Telescope.—Dr. A. von Waltenhofen.—A lengthy mathematico-optical essay illustrated with engravings.

Precipitation of Small Quantities of Phosphoric Acid by Molybdate of Ammonia, and some Remarks on the Yellow-Coloured Silico-Molybdic Acid Precipitate.—Dr. E. Richters.—Reserved for full translation.

Application of some Fluorine Compounds for the Purposes of Rendering Glass Opaque, especially for Photographic Use.—E. Siegwart.—This paper contains a review of all that has been hitherto done as regards the application of hydrofluoric acid and other fluorine compounds to the technology of glass.

Materials and Facts for Judging on the Quality of the Air in Public Buildings.—Dr. H. Dorner.—This essay bears more particularly upon the condition and quality of the air in some public buildings at Hamburg.

Caloric Capacity of the Air.—F. Ritter von Schwind.—An algebraico-physical essay.

Objective Treatise of the Stroboscopic Disc.—Dr. E. Zizmann.—Illustrated with woodcuts.

Black-Coloured Shining Pigment on so-called Sugar-Paper.—Dr. Kielmeyer.—After referring to a rather clumsy method, now in use in Austria, of making the paper, or rather of painting it on one side only. The author gives a receipt for performing the operation by means of the continuous paper-making machinery.

So-called Dry Purification and Cleaning of Wearing Apparel.—Dr. M. Reimann.—Illustrated with engravings and woodcuts.

Application of Soda and Alkali Works' Refuse for the Purpose of Making the Embankments and Permanent Ways of Railways.—M. Schaffner.—The author calls attention to the fact that the refuse alluded to, and remaining after having been desulphurised, consisting mainly of carbonate and sulphate of lime and some sulphite of lime, is an extremely useful material to be employed instead of sand for the above-named purpose; also on account of the preservative property of the constituents of the material for the wooden sleepers.

Preparation of a Fine Blue-Coloured Ink.—Dr. Dingler.—Take, of yellow prussiate of potassa, 10 parts, dissolve in 160 parts of pure distilled water; gradually, and while stirring, add to that solution a mixture of five parts of a solution of perchloride of iron (sp. gr. 1·480) and 160 parts of water. The ensuing precipitate is collected on a filter, and washed with distilled water until the wash-water begins to assume a blue colour, after which the precipitate, which has then, by the bye, become completely soluble in distilled water, is dissolved in 400 parts of that water,

NOTES AND QUERIES.

Percentage of Anthracen.—Can any reader of your valuable journal inform me of the commercial mode for testing the percentage of anthracen, and oblige.—Anthracen.

Determination of Suphur in Cast-Iron, &c.—(Reply to S. G. G).—The solutions in the flask were repeatedly tested for sulphuric acid, and none ever found. The insoluble residues contain mere traces of sulphur which I believe to exist as sulphate.—A. H. Elliott.

Obtaining Silver from Sea-Water.—Is anything known of I. H. Steele's process for this (16, Windmill Street, Gravesend)? Does it succeed? What is its nature, requiring some instrument capable of being used by any seaman? Each capable of yielding some hundreds of pounds a year.—K.

Plattner's Charcoal Blocks, &c.—Will anybody in London undertake to make Plattner's charcoal blocks, small clay roasting capsules &c., at a moderate price? Especially the ordinary qualitative blocks. Good charcoal is almost non-existent in England. Mr. Bates, of 20, Litchfield Street sells it. I have just bought up all worth having.—Per Mare Per Terram.

Tattooed Marks.—Let "Scotia" consult Tardieu's paper in the *Am. d'Hyg. Publique*, 1855, p. 171, &c. In a successful attempt at removal the mark was first well rubbed with a salve of pure acetic acid and lard, then with a solution of potash, and finally with hydrochloric acid. Further particulars are contained in the paper, and in the English translation of Casper's "Handbook of Forensic Medicine," vol. i., p. 108.—T. Stevenson.

Writing Ink.—(Reply to H. Pitt).—The greenish tinge is not due to indigo. We quote the following receipts from Wagners's work on "Technology":—Take of decoction of logwood (one part of wood to eight of water) 1000 parts, add thereto one part of neutral (lemon-yellow) chromate of potassa, and a few grains of mercuric chloride (corrosive sublimate); This ink writes greenish, but becomes very black. Take of nutgalls 42 parts, madder three parts, and exhaust with water, so as to obtain, after filtration, 120 parts; next add of sulphate of indigo 1·2 parts, protosulphate of iron 5·2 parts, and 2 parts of the commercial solution of pyrolignite of iron. For copying ink, make either of these two more concentrated, and add some gum and sugar.—Chemicus.

MEETINGS FOR THE WEEK.

Monday, 27th.—Medical, 8.
—— London Institution, 4. Mr. R. A. Proctor, F.R.A.S. "On Astronomy."
—— Geographical, 8.20.
Tuesday, 28th.—Royal Institution, 3. Dr. Foster, "On Nutrition of Animals."
—— Civil Engineers, 8.
—— Ethnological, 8.
Wednesday, 29th.—Society of Arts, 8.
Thursday, 30th.—Royal, 8.30.
—— Royal Institution, 3. Dr. Odling, "On Davy's Discoveries."
—— London Institution, 7.30. Professor Bentley, "On Economic Botany."
—— Chemical, 8. Anniversary.
Friday, 31st.—Royal Institution, 9. Professor Max Müller, on "Solar Myths."
Saturday, April 1st.—Royal Institution, 3. Mr. O'Neil, "On the Spirit of the Age."

TO CORRESPONDENTS.

*** Vol. XXII. of The Chemical News, containing a copious index, is now ready, price 11s. 8d., by post, 12s. 2d., handsomely bound in cloth, gold-lettered. The cases for binding may be obtained at our office, price 1s. 6d. Subscribers may have their copies bound for 2s. 6d. if sent to our office, or, if accompanied by a cloth case, for 1s. Subscribers wishing to complete their sets of volumes are requested to apply to the publisher, who will give them information respecting scarce numbers and volumes. Vol. xxiii. commenced on January 6th, and will be complete in twenty-six numbers. Reading Cases, price 1s. 6d. each, post free, may also be obtained at the Office.

E. Kernan.—The publication of *Les Mondes* is resumed; we have received numbers for March 2nd and 9th.

E. W. Parnell.—Thanks for your communication; it shall appear in an early number.

R. Carter Moffat, Ph.D.—Received.

G. B. Buckton.—Received.

Manning Prentice, jun.—Your request shall receive attention.

C. S. Overy, J. and E. Sturge, G. Rogers.—Our publisher will communicate with these correspondents shortly.

R. Warington.—Thanks for your enclosure.

S. Wilkinson.—Bunsen's "Gasometry" will answer your purpose.

T. Chartres White.—Thanks for the Report.

G. Stock.—We will write by post.

J. H. C.—You will probably obtain cryolite from some of the manufacturing chemists in the Lancashire district, Newcastle, or Glasgow

G. A. Keyworth.—Next week.

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II. The Theory of Atmospheric Germs. By Arthur Ernest Sansom, M.D., Member of the Royal College of Physicians. With Seven Woodcuts.
III. Molecules, Ultimates, Atoms, and Waves, by Mungo Ponton, F.R.S.E.
IV. The Great Pyramid of Egypt, from a Modern Scientific Point of View. By C. Piazzi Smyth, F.R.S., Astronomer Royal for Scotland.
V. Steam Boiler Legislation. By Sir William Fairbairn, Bart., LL.D., F.R.S. With Page Woodcut.
VI. The Eclipse of Last December, with special reference to the Interpretation of the Solar Corona. By Richard A. Proctor, B.A. Cambridge. With Page Plate and Two Woodcuts.

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THE CHEMICAL NEWS.

VOL. XXIII. No. 592.

FURTHER OBSERVATIONS ON THE STRENGTH OF GARDEN NAILS.*

By J. P. JOULE, LL.D., F.R.S., V.P.

THE author thought it desirable to ascertain how far hardness had to do with the strength and elasticity of these small specimens of cast-iron. For this purpose he plunged some of them at a heat near the melting point into water, then selecting those which had been hardened sufficiently to resist the action of the file. Others he cooled slowly from a bright red heat. The experiments were conducted in the manner described in the last number of the proceedings.

	No. of Experiment.	Length of Nail between Supports.	Breadth of Nail at Fracture.	Depth of Nail at Fracture.	Deflection.	Breaking Weight. lbs.
Hard Nails.	1	1·00	0·110	0·122	0·0067	129
	2	1·04	0·120	0·120	0·0037	84
	3	1·00	0·120	0·122	0·0028	81
	4	1·02	0·143	0·102	0·0077	129
	5	1·10	0·138	0·130	0·0071	203
Average		1·032	0·1262	0·1192	0·0056	125·2
Soft Nails.	6	1·00	0·112	0·117	0·0088	141
	7	1·05	0·139	0·114	0·0087	150
	8	1·02	0·130	0·138	0·0051	176
	9	1·04	0·117	0·090	0·0101	101
	10	1·04	0·121	0·108	0·0073	113
Average		1·03	0·1238	0·1134	0·0080	136·2

Reducing to a length of three feet and one inch square section, and making a deduction of one-sixth from the deflections, on account of the taper of the nails, the above results, along with those in the last number of proceedings, become—

	Breaking Weight.	Deflection.
Nails in original state	2673	0·922
Hardened ditto	2002	0·677
Softened ditto	2448	0·924

ON THE ESTIMATION OF PHOSPHORIC ACID.

By E. W. PARNELL.

IN making accurate determinations of phosphoric acid, by precipitation as magnesium ammonium phosphate, an allowance is generally made for the solubility of the precipitate in the ammoniacal liquid, one milligramme for every 54 c.c. of the filtrate and washings being added to its weight after ignition. Some results that I have recently obtained in analyses, and subsequent experiments, have led me to conclude that this correction is unnecessary.

With a view of ascertaining the accuracy of the molybdic acid method of estimating phosphoric acid, I weighed out two portions of powdered crystallised sodium phosphate; the phosphoric acid in one I determined by direct precipitation, as magnesium ammonium phosphate. To the other I added a solution of molybdic acid in nitric acid, and proceeded with the estimation precisely as recommended by Fresenius, in both cases making the usual correction for the solubility of the precipitate. The first determination gave phosphoric acid 24·55 per cent; the molybdic acid method 24·98 per cent. The precipitate in the latter case proved to be quite pure, free from molybdenum and excess of magnesia. As the proportion of filtrate to precipitate was much larger in the latter case than in the former, too large an addition for the filtrate would cause the molybdic acid method to show the higher result; I therefore proceeded to ascertain by experiment the extent of correction necessary. A filtrate, measuring about 500 c.c., was evaporated to dryness, ignited to expel ammoniacal salts, the residue dissolved in a small quantity of hydrochloric acid, and the liquid neutralised with ammonia. The volume, after addition of ammonia, measured about 10 c.c. I should, therefore, obtain a precipitate that 490 c.c. had held as a saturated solution. No precipitate appeared even after several days; this would indicate that no correction was necessary,—making no allowance.

The first estimation shows 24·30 per cent phosphoric acid.
The molybdic acid method, 24·26 „ „ „
results which agree satisfactorily.

The solubility of the precipitate in a solution of chloride of ammonium and ammonia is beyond question; the only conclusion, therefore, to be drawn was that it was insoluble in presence of excess of magnesia. This proves to be the case. I prepared a saturated ammoniacal solution of the salt: to a portion I added an ammoniacal solution of magnesia; this produced an immediate precipitate of the double salt. To another portion I added a solution of sodium phosphate; this singularly also produced a precipitate. From this, in estimating magnesia as the double phosphate, when an excess of phosphoric acid is present in the liquid, the usual correction of one milligramme for every 54 c.c. is too high. From weighed quantities of pure magnesia I have obtained the theoretical quantity of magnesium phosphate without any correction for the filtrate.

Attention was called a few weeks since, in the CHEMICAL NEWS, to the difficulty of obtaining a pure magnesium ammonium phosphate by precipitating a solution of phosphoric acid with a solution of magnesia, an excess of the latter base being invariably carried down. The remedy suggested was that the precipitate should always be redissolved, and re-precipitated with ammonia. I find that this excess is more liable to fall when the solutions are concentrated; with solutions not strong enough to produce an immediate precipitate, but from which the double salt separates out after a few minutes' standing, a perfectly pure precipitate may be obtained. I have sometimes found the ignited precipitate to contain an excess of magnesia to the extent of eight per cent. In such cases, resolution in hydrochloric acid and re-precipitation with ammonia failed to give a pure precipitate, the excess of magnesia being merely reduced to about four per cent. In order to ensure the purity of the precipitate. I now raise the two ammoniacal solutions to the boiling point; on mixing them, and stirring, a bright crystalline precipitate appears, which, so far, has never failed to be perfectly pure. This plan is especially advisable in presence of citrate of ammonia; from cold solutions, citric acid, with probably magnesia, is almost always carried down with the precipitate, and after long ignition, the latter will be sometimes found to enclose a considerable quantity of carbon.

ON THE REACTIONS OF VARIOUS OILS WITH H_2SO_4.

By G. P. CLARKE.

HAVING found that the tables of these reactions which are given in various text-books differ considerably, and that the account of each particular reaction is very incomplete, I made a very careful series of experiments on the

* Read before the Manchester Literary and Philosophical Society, March 7th, 1871.

subject. The specific gravity of the oils, which were, as far as possible, perfectly pure, was taken by weighing, at a temperature of 15° C. I found that the addition of H_2SO_4 brought out the characteristic odour of the oil better than by heating it. The proportion of oil and acid was 25 drops to 1 drop. Subjoined is the result:—

Oleic Acid (commercial) from Distilled Palm Oil.—Instantly forms a brown-red spot, with a dark outer ring; small brown particles appear, and dart about on surface of oil. On being stirred, becomes uniformly chocolate colour, which gradually grows streaky and deposits a white film. Sp. gr., 902·346. Solidifies at 4° C.

Oleic Acid (commercial) from Distilled Cotton Seed Grease.—Instantly forms a deep red spot, with dark concentric rings. On being stirred, becomes purple-red, does not become streaky, and keeps the same colour for a considerable time. Sp. gr., 915·924. Solidifies at 5° C.

Bleached Neutral Olein from Palm Oil.—Spot at first greenish-yellow, with a slight nucleus; a number of cells appear, these converge, and form a large brown nucleus, which revolves rapidly, and throws off brown fibres which form concentric rings. On being stirred, it separates into brown patches and transparent streaks, giving the oil a marbled appearance. Sp. gr., 914·381. Solidifies at 10° C.

Neutral Olein from Tallow.—Light yellow oval spot; nucleus forming a straight line, having a brush at each end; this throws off pink rays, till in a short time the whole of the oil is coloured light pink. On being stirred, the oil becomes a dark orange-colour. Sp. gr., 917·159. Solidifies at 5° C.

Lard Oil.—Spot greenish yellow, non-nucleated; a dark brown ring skirts the edge of the pool of oil, throwing off brown streaks towards the centre; the top of the oil then becomes covered with a red-brown crust. On being stirred, the oil becomes full of brown specks, which revolve rapidly and evolve bubbles of gas. Sp. gr., 920·245.

Ground Nut Oil.—Yellow spot, which moves from side to side, and throws off an orange crust on the surface of the oil. On being stirred, the oil becomes streaky brown, and gives off gas. Sp. gr., 921·788. Solidifies at −2° C.

East India Castor Oil.—Faint yellow spot, with several transparent nuclei. On being stirred, the oil becomes almost colourless, and gives off bubbles of gas. Sp. gr., 963·757.

Doddee Seed Oil.—Oval spot, with brown-red concentric rings, each increasing in darkness as it nears the centre; has a black nucleus which revolves rapidly. On being stirred, becomes a brown-black mass which rapidly thickens. Sp. gr., 932·897.

Sperm Oil.—Brown spot, with rays from a black nucleus to the circumference, the spot surrounded by a very faint outer yellow ring. On being stirred, the oil became light purple, then dark purple, and finally brown-red. Sp. gr., 885·682.

Pale Seal Oil.—Orange spot, which revolves rapidly, and sends off purple streaks forming a large ring. On being stirred, becomes a bright red, which quickly darkens, and becomes full of dark spots. Sp. gr., 926·108.

Gallipoli (Olive) Oil.—Spot at first colourless, then becomes faint orange, and sends off grey crust on to surface of oil. On being stirred, becomes first green, then grey, and becomes a streaky mass with a dark nucleus. Sp. gr., 919·628.

Niger Oil.—Spot at first colourless, then slowly darkens, and dark rings pass to the centre from the circumference; these form a dark nucleus, which elongates itself, and becomes a straight line, throwing off a grey crust on to surface of oil. On being stirred, becomes first olive, then red-brown, and finally dark green. Sp. gr., 928·577.

Rosin Oil.—A small blood-red spot, with well-defined edge, which remains perfectly stationary, and does not expand. On being stirred, becomes a fine orange red colour, afrer standing some time blood-red patches appear. Sp. gr., 990·606.

Best Olive Oil.—Spot at first colourless, then becomes yellowish-green. On being stirred, becomes olive-green. Sp. gr., 917·776.

Refined Rape-Seed Oil.—Bright yellow spot, with red rings; spot gradually grows dark brown, and throws off a brown crust on to oil. On being stirred, forms a black-brown mass. Sp. gr., 919·011.

Poppy Oil.—Motionless mustard-yellow spot, with orange rings. On being stirred, becomes buff, and finally red-brown, and gives off gas. Sp. gr., 926·232.

Mustard Oil.—Dark yellow spot, with fibrous orange streaks; nucleus revolves rapidly. On being stirred, becomes reddish brown. Sp. gr., 918·270.

Cod Liver Oil.—Orange spot, with dark red nucleus, sending off a purple crust on to oil, which soon becomes brown. On being stirred, becomes bluish-purple, then red-purple, and lastly, red brown, with a streaky appearance. Sg. gr., 924·565.

Almond Oil.—Spot at first colourless, then green yellow with an orange nucleus. On being stirred, becomes olive and then brown, with a marbled appearance, gives off large quantity of gas. Sp. gr., 919·010.

Each experiment lasted ten minutes, and the sample being tested, was carefully observed with a large magnifying glass during the whole time. I have in hand some experiments on mixtures which I hope to be shortly able to lay before you.

ON THE

CHANGE EXPERIENCED BY METALLIC IRON AND BY IRON OXIDES WHICH HAVE SERVED TO DISSOCIATE CO.*

By I. LOWTHIAN BELL.

(Continued from p. 136).

Expt. 338.—A mixture of 100 vols. of CO and 9 vols. of CO_2 was passed over spongy iron for 2½ hours; at the end of this time its composition was—Fe, 91·42; C, 0·33; O, 8·25 = 100.

Before leaving the subject of carbon deposition on pure spongy iron, it may be interesting to quote the results of a simultaneous exposure of this substance, and oxides of iron differently prepared in the lead bath arrangement, during a period of nine hours, to a temperature of melting zinc:—

Substance employed.	C. deposited per 100 Fe present.
Expt. 339.—Pumice-stone, saturated with Fe_2O_3 from $FeSO_4$	808
,, 340.—Precipitated Fe_2O_3 anhyd.	207
,, 341.—Fe_2O_3 from $FeSO_4$	206
,, 342.—Pure, spongy iron.	158

So that these figures do not indicate that iron, in its metallic state, possesses any advantage over its oxide in separating C from CO.

To compare the effect produced by heated CO on iron of a porous texture with that when solid iron was used:—

Expt. 343.—Thirty feet of fine iron wire (one foot weighing 0·59 grain) was well cleaned with sand paper, cut into short lengths, and introduced in a half-pound flask provided with a thermometer, and also with outlet and inlet tubes, A rapid stream of CO was driven through the apparatus for one hour; the gas being previously thoroughly purified, by passing through 2KHO bottles, $AgNO_3$, and KHO bulbs. It showed no turbidity in being passed through lime water for twenty minutes. Heat was now applied by the air bath, and observations made of the issuing gas on lime water.

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* From the "Chemical Phenomena of Iron Smelting."

Time. hr. min.	Temperature. °C.	°F.	Effect on lime water.
1 0	18 rising to 204	(65 to 400)	*nil.*
0 30	204 ,, 227	(400 ,, 440)	do.
0 25	227 ,, 243	(440 ,, 470)	do.
0 45	243 ,, 271	(470 ,, 520)	do.
0 30	271 ,, 282	(520 ,, 540)	No opalescence.

So far, then, as the lime water indicated, there was no action; but next day, on examining the wire, almost the whole was turned blue, and the remainder, straw colour; and when strong HCl was poured upon it and almost immediately decanted off, the solution gave an intense blue with ferrocyanide of potassium, showing a film of oxide of iron, higher than FeO, had been formed, and proving that a portion of the CO had been decomposed, but in too minute quantity to produce a perceptible effect on lime water.

Expt. 344.—The same wire was next exposed to a higher temperature, viz., bright red, but previously to turning on the CO, H was passed over the wire, in a heated state, to reduce any rust which might be adhering to its surface.

The CO was obtained from sulphuric acid and ferrocyanide, passed through a bottle containing a solution of potash, then, through two tubes containing $AgNO_3$; three tubes, containing dry KaO; and one, containing pumice-stone, saturated with HSO_4. Air was expelled by passing this gas through the apparatus for 1¾ hours; heat was then applied and maintained at a bright red for four hours, the current of CO being continued until the whole was cooled.

The wire was blue on the surface, and the 40 grains used had increased 0·12 grain.

The potash bulbs behind the apparatus had increased in weight 0·27 grain.

According to the action of $2CO = C + CO_2$, 0·27 grain of carbolic acid collected in the potash bulbs ought to represent 0·07 grain of deposited carbon, but the gain in weight being 0·12 grain, the difference of a portion of it must be due to oxidation of the iron, indications of which were manifested by the change of colour in the wire itself.

The wire, which had served in the experiment, was then dissolved in one portion of HCl, and a similar quantity, which had been exposed to the hydrogen current alone, was similarly treated by another portion of HCl. The solution from the first had floating about in it some whitish flocculent particles, and a few small black flakes. In the second, an extremely minute trace of the whitish matter appeared in two days, but no appearance of the dark coloured flakes could be perceived.

This experiment, therefore, justifies the belief that compact iron slowly acts on CO by resolving it into C and CO_2, while the metal itself acquires a minute trace of oxygen.

This view of the effect of iron, in its dense form, on oxide of carbon is further confirmed by the appearance of the inside of the iron vessel which served to contain the specimens exposed to the action of CO in the lead bath. It was noticed, after this apparatus had been in use a few times, the interior was coated uniformly with an impalpably fine black dust—no doubt C, precipitated from the CO, to which the surface had been exposed.

Expt. 345.—The faculty of metallic iron, particularly in a state of fine division, to precipitate carbon from oxide of carbon, being now proved beyond a doubt, steps were next taken to ascertain its effect upon the gas, at lower temperatures.

All the precautions, previously described, to ensure, as far as practicable, the absolute purity of the CO were observed, and, as in one or two cases, the $AgNO_3$ was blackened by the passage of the gas, to prevent any CO_2, which might thereby have been generated, the CO, before being admitted to the reduced oxide, was freed from such possible contamination by being passed through two tubes, containing KHO in its solid form, which also retained any moisture, avoiding thus the introducton of any impurity by the use of SO_4H_2 and pumice-stone. The gas, thus purified, was passed through the apparatus for two hours, before the Liebig's tube, which contained spongy iron, was attached, or heating commenced.

The gas was tested, previous to immersing the tube in the air bath with the following results:—

5·75 volumes exploded with O gave

	Contraction.	CO_2.
Theoretical for pure CO	2·900 =	5·80
	2·875 =	5·75

The spongy iron was ascertained to contain 99·9 per cent of Fe, by permanganate, harpsichord wire being 99·5. After it had been exposed for five and a half hours, at a temperature of 238° to 243° C. (460° to 470° F.), the iron had a black, apparently carbonaceous layer, on the surface, and yielded a distinct quantity of CO_2, on burning in a stream of oxygen. The metal, moreover, had increased in weight 0·3 per cent.

Expt. 346.—In order to observe the rate at which CO_2 was given off by pure spongy iron acting on CO, the KHO bulbs were weighed at various intervals, but, of course, the weights so obtained do not represent the CO_2 generated by the carbon deposition alone, but also that due to the oxidation of the $Fe_xCO + Fe = FeO_x + xCO_2$.

Thermometer range.	Time of exposure.	Increase of KHO bulbs.
227° to 254° C. (452° to 470° F.)	1 hour	0·011 grm.
232° ,, 240° ,, (450° ,, 472° ,,)	¾ ,,	0·009 ,,
232° ,, 240° ,, (450° ,, 472° ,,)	1¾ ,,	0·034 ,,
232° ,, 221° ,, (450° ,, 420° ,,)	1¼ ,,	0·017 ,,
227° ,, 238° ,, (452° ,, 460° ,,)	1 ,,	0·008 ,,
243° ,, 238° ,, (469° ,, 460° ,,)	1¼ ,,	0·023 ,,

The spongy iron had increased in weight, and contained traces of carbon—0·009 per cent.

The behaviour of spongy iron when enveloped in the earthy constituents found in an ore, is similar in every respect to the metal when presented in its isolated form to the action of CO, as described in the experiments immediately preceding, only the diluted condition in which the Fe is found, for example in calcined Cleveland ore reduced by H, interferes, as one might expect, with the rapidity of the action.

A quantity of this ore was completely reduced by H, when it was found to consist of:—

Fe	50·58
Earthy matters	49·42
	100·00

(To be continued.)

ACCOUNT OF A FEW

EXPERIMENTS MADE FOR THE PURPOSE OF PRESERVING RAW MEAT.

By Dr. BAUDET.

SINCE I had obtained, by a lengthy practice, some considerable experience as regards the antiseptic and preservative properties of a substance which I term *spyrol* (carbolic acid), for being applied to the tanning, tawing, and currying operations, I felt induced to try some experiments as regards the use of that body for the preservation of meat.

First process: *By immersion in phenic water at from* 5-10,000*ths to* 1-1000*th.*—On the 18th of October last year, I took four wide-mouthed stoppered bottles, and placed in each 250 grms. of raw horseflesh, slightly moistened with phenicated water in the following proportions:—No. 1, solution at 4-1000; No. 2, solution at 3-1000; No. 3, solution at 2-1000; No. 4, solution at 1-1000. To the contents of every bottle I added a few small pieces of well-burnt charcoal, with the view to absorb any gaseous matter which might be evolved from the meat; after having

hermetically closed the bottles, I have kept these for thirteen weeks in a room constantly heated at from 15° to 20°. On inspecting the bottles after the lapse of time just mentioned, I found that the liquid which covers the meat had in all bottles become slightly rose-red coloured. The state of the meat, on examining it, was found as follows:—No. 1. The meat had become somewhat blackish-coloured, but was not spoiled at all. No. 2. Meat very well preserved, colour light rose-red. No. 3. Meat perfectly well kept, with the natural colour of fresh meat. No. 4. Meat has quite well kept; its colour has greatly improved considering that raw horse-flesh is naturally deep-coloured. A few days after, having inspected and noted down, as described, the contents of each bottle, I have taken a portion of the meat of No. 3 bottle, and, without having it washed or drained, have fried it, and dressed as a beef-steak; on partaking of it, in company with several other parties, we found the meat excellent, having only acquired a slight taste similar to that of cured ham and bacon, but by no means disagreeable. I have kept at the same temperature as indicated above, and under the same conditions, the meat in the bottles, well-closed, and have not observed, up to the middle of February last, any other change in the meat than an external drying and shrivelling up, and deeper colour, but internally the natural colour remains. From the foregoing experiments, I conclude that phenicated water, in the proportion of from 1-1000th to even 5-10,000th, might be applied to keep raw meat fresh and sweet, without imparting to it either any perceptible smell or taste, provided the meat be kept in well-closed vessels, be they casks, tinned iron canisters, or other vessels.

Second process: *By means of vegetable charcoal coarsely broken up, and saturated with phenicated water at from* 5-10,000*ths to* 1-1000*ths.*—This process is applied as follows:—I cover the meat with a thin woven fabric, in order to avoid its direct contact with the charcoal, which might penetrate into the fibre of the meat, which is placed next into barrels, care being taken to place therein first a layer of the phenicated charcoal, then a layer of meat, and so on, alternately, until the barrel is quite filled, and all interstices properly taken up by the charcoal. As regards the importation of raw meat, preserved by this method, from South America, I would suggest that the meat, first covered with any thinly-woven fabric, be placed in bags made of raw caoutchouc, very abundantly obtainable in the country alluded to; so that the importation of raw meat and the importation of caoutchouc might go, as it were, hand in hand. The mode of filling in alternate layers of phenicated charcoal and meat would, of course, remain the same; and there would be no difficulty of hermetically sealing up bags made of caoutchouc, either by soldering the seams together, or by placing a cap of caoutchouc over the mouth of the bag, and soldering the cap on hermetically.—*Moniteur Scientifique.*

ON THE VARIOUS TINTS OF AUTUMNAL FOLIAGE.

By H. C. SORBY, F.R.S., &c.

(Concluded from p. 139).

Having given a general account of the various colouring matters met with in foliage, I will now proceed to show how they serve to give rise to the almost endless variety of autumnal tints. These are usually due to varying mixtures of colours belonging to two or more groups. It is very doubtful if any leaves are coloured by one single substance, and generally they contain not only colours belonging to several groups, but even more than one of the same group.

Unfaded green leaves are coloured mainly by chlorophyll, but the tint is very much modified by xanthophyll, and by colours of the chrysophyll group. The presence of these, in varying relative and absolute amount, explains in a most satisfactory manner all the various brighter and darker greens met with in different leaves in different conditions. It is doubtful if chlorophyll has ever been seen free from xanthophyll. On heating green leaves with alcohol, a bright green solution is obtained. On agitating with bisulphide of carbon, this sinks to the bottom with the greater part of the chlorophyll and some xanthophyll in solution, whilst the alcohol retains most of the xanthophyll and some chlorophyll. After agitating this with a little fresh bisulphide, evaporating to dryness, and dissolving out the chrysophyll by water, when dry, the impure xanthophyll may be dissolved in bisulphide of carbon. The solution of chlorophyll in the bisulphide first obtained may be somewhat purified by agitating with fresh alcohol, but even then the spectrum clearly shows the absorption-bands due to xanthophyll. Still, on comparing the two different products, we can see at once that an approximate separation has been effected, and that for an equal amount of chlorophyll one contains six or eight times as much xanthophyll. This is a mere green-yellow, and the other a bright green; but, since it must be made considerably brighter by the xanthophyll, pure chlorophyll is no doubt of a darker and bluer green colour. The tint of many green leaves is also much modified by various colours of the erythrophyll group, which give rise to more or less green browns, and, in some cases, almost to black; for the green chlorophyll absorbs the blue and red rays, and the erythrophyll the green, so that all light is extinguished. If the erythrophyll preponderates over the chlorophyll, we have a red or even purple green, as in the case of the copper and purple beech; and thus, independent of any change, there is a considerable variation in the tints of normal growing leaves. It is, however, in autumn, when the chlorophyll has disappeared, that the brighter and more definite colours are produced. The amount of xanthophyll which is found in green leaves is so considerable, that probably the yellow colour of faded leaves is quite as much, or even more, due to that which previously existed than to any specially developed in the change, and the alteration which may be said to consist chiefly in the disappearance of the chlorophyll. The result of this is that a deep green is changed into a bright yellow, and the general change in the spectrum is that there is no longer any absorption at the red end. Probably, however, few yellow leaves are coloured merely by xanthophyll, and the tint of many depends quite as much on the chrysophyll, and is also very much modified by colours of the erythrophyll and phaiophyll groups.

As I have already named, many leaves contain colour of the erythrophyll group, even when young and healthy, but the production of a red colour is more common in autumn, when their vitality is diminished. In some cases it takes place whilst the chlorophyll is unchanged, or only partially altered into the browner modification, and then its production merely gives rise to a dark brown which does not attract the eye. The deep brown colour of heath in autumn is an example of this, and on careful examination it may be seen that the brown shade is almost entirely confined to the side of the plant which is exposed to strong light. The red colouring matter is so disguised by the green chlorophyll that one would scarcely expect to extract, by the method already described, a colour quite equal in beauty to carmine, and of almost exactly the same tint. Very many other illustrations might be given of the same general fact, but the colour does not attract attention until the chlorophyll fades, and then the mixture of the previously existing red with a more or less pure yellow gives rise to scarlet. This may be seen to great advantage in the leaves of the common bramble and many other plants. It may then be asked why we never see a fine scarlet in the case of heath or purple beech. The explanation seems to be that in them the red colour is not the same as in the case of the numerous plants which turn scarlet, and is so much more easily decomposed that

it entirely fades before the chlorophyll is altered. The spectrum method indicates that the colour of these two plants is the same, but differs from that red colour which occurs in most of those turning to a fine scarlet, as, for example, in the leaves of the bilberry, bramble, hawthorn, Berberis, cherry, apple, and guelder-rose. In other plants the red colour is not specially developed, whilst the chlorophyll is unchanged, but is produced at the time of that change, as if in some way dependent on the same cause. I have especially studied this point in the case of the leaves of the common sorrel, and find that the production of the red colour depends in some way on exposure to light, and on loss of perfect vitality. I had long known that most of the bright red leaves met with in the fields were those which had been broken off from the plant, and yet when dried in the house, or even kept in the dark with their stalks in water, the green leaves fade to dull yellow. I therefore placed in my garden detached leaves with their stalks in the earth, some with the upper, and some with the lower surfaces exposed to the light, some in the sun, and some in the shade, and I found that those which turned to a fine red were those exposed to the sun with the lower surface upwards. I have also noticed that the red colour is often produced in spots where the leaf has been injured by insects; and in the case of other plants I have remarked that the leaves on partially broken twigs show this colour to unusual advantage. I am, therefore, disposed to attribute the formation of the red colouring matter to some change which takes place when the leaves are not actually dead, but in a state of very low vitality. Of course this is scarcely applicable to those in which a red colour is a normal constituent; but, at the same time, even then it may indicate more or less of the same kind of condition; for I have remarked that in those branches of the bilberry which are of a fine scarlet in the early part of the year, the form of the leaves departs considerably from the usual type, as if they were not altogether in a healthy state. Perhaps the fading of green leaves to yellow, and the normally yellow state of some leaves may be referred to a somewhat similar low vitality, which either permits the chlorophyll to become changed or prevents its formation, as in the case of plants growing in the dark. I may here say that there seems every reason to conclude that on further change the red colour so completely fades away as to produce little or no effect on the general tint, since faded red leaves cannot be distinguished from those of the same kind which were not red, and the artificial oxidisation with nitrite of potash, of the substances soluble in water, extracted from scarlet leaves of the bilberry, gives rise to exactly the same colour as in the case of the yellow or green leaves. I may also here say that when scarlet leaves are digested in hot water the red colour is easily removed, and the green, yellow, or brown colours left as the case may be.

In studying the further changes which occur in leaves in autumn, it is most important to understand the properties of the various colours allied to the chrysophyll group, since it is to them that we must attribute a great part of the more prevailing tints. On the whole, clean scarlets are uncommon in this country in the case of large trees, and simple bright yellows are not very abundant, or only last for a short time; since the chlorophyll seldom disappears entirely before the chrysophyll is more or less changed. Much remains to be learned with respect to the various kinds of chrysophyll, and the connection between each and the species or special variety of the plant, and the circumstance in which it is placed. As far as my present knowledge enables me to judge, there is some decided connection between the kind of colour and the species of tree, but, at the same time, I have met with entirely different colours in the same species growing in other situations, and I am even disposed to think that there may be individual differences, analogous to what is so common in the colour of the hair of animals. It is this complication of facts which makes it very difficult to explain the cause of some of the results, but, at the same time, this variation is in complete agreement with the varied tints of different trees of some species.

One of the most striking kinds of chrysophyll which has much influence on autumnal tints is that contained in yellow beech leaves. It is of pale yellow colour, but when dissolved in alcohol and oxidised by means of nitrate of potash and hydrochloric acid, it is changed to deep pink-red. When dissolved in water the oxidisation gives rise to a very copious precipitate of the same colour, which is, therefore, apparently only imperfectly soluble in water, but more easily in acid alcohol. This kind of chrysophyll appears not to be formed till the time when the leaves begin to turn yellow, since I found that green beech leaves contained much of another, which is often associated with the one just described in other trees. The yellow leaves contain the yellow xanthophyll, whereas the orange-brown leaves contain the orange modification, as though, perhaps, derived from the other by partial oxidisation. I have met with this kind of chrysophyll in the leaves of the bilberry, and in some varieties of plane, and in a less pure state in many others. Another species of chrysophyll found in many leaves may be procured from some varieties of the common elm. I have found that leaves of large size and loose texture give it in the most pure state, but in some it is mixed with much of the kind found in the beech, and in others is almost entirely replaced by a third colour, which is but little altered by nitrite of potash. The colour to which I wish, however, to call especial attention turns to a pink-orange when thus oxidised, and gives a spectrum with a sufficiently well-marked narrow absorption-band, in the centre of the green, and a fainter nearer to the blue; but after a while these bands fade, and the colour becomes orange-yellow. I have met with this colour in the leaves of the poplar, Spanish chesnut, alder, apple, and oak, and, as far as I am able to judge, there are very many trees whose leaves contain variable mixtures of this with that found in yellow beech.

I have met with a very special kind of chrysophyll in the leaves of a plane tree, which turns to a very fine yellow. This gives on oxidisation by nitrite of potash, a pink colour, with a very well-marked absorption-band in the green, nearer to the red end than in the case of that met with in the elm. In the green leaves of other planes of the same species I found only that colour so common in yellow beech leaves, and I have noticed that such plane leaves do not turn yellow, but to an orange-brown, modified by the continued presence of chlorophyll, though when this has been dissolved away by means of alcohol, the former colour can be very clearly seen.

It is very probable that other kinds of chrysophyll will be found on more extended examination, and that a more complete knowledge of their properties will serve to explain many facts which are still obscure. I have, indeed, even now good reason for believing in the existence of some others, but their characters do not differ sufficiently from those described to materially modify the general results.

The alcoholic solutions of all these various kinds of chrysophyll resemble one another in being changed by oxidisation with nitrate of potash and hydrochloric acid to pink-red substances, which alter more or less slowly into orange colours. When thus changed, and kept dry, or in solution with a slight excess of ammonia, they are further modified into various brown substances. This is well seen in the case of the red colour obtained from beech leaves, which, on the addition of ammonia, shows a well-marked absorption-band in the orange. This slowly disappears, and, after keeping for awhile, when evaporated to dryness, it is nearly as brown as burnt umber, and the addition of hydrochloric acid to the solution does not restore it to the original fine pink-red, but we have a decided brown colour, to some extent absorbing the red end of the spectrum, which was previously quite clear. All these modified colours, when dissolved in sulphuric acid diluted with an equal bulk of water, and still further oxidised by means of chlorate of potash, merely fade, and

are thus in that state of oxidisation which is characterised by a maximum depth of colour. These different changes may be simply due to an alteration of the chrysophyll, but, at the same time, there are cases which seem to indicate that almost, or quite, colourless compounds may contribute to the production of deep colours. For example, when the fresh leaves *Acuba japonica* are digested in cold alcohol, the solution evaporated to dryness, and treated with water, a clear yellow solution is obtained, which, when evaporated at a gentle heat, turns dark brown, on account of the formation of an insoluble substance of that tint. Water then extracts the apparently unaltered yellow substance: and, though these facts are an exception to the general rule, they seem to show that a dark colour may be formed independent of the previous existence of a colouring matter of the chrysophyll group.

Though some of the various phaiophyll colours are soluble in water, the proper solvent to extract them from the leaves is moderately strong alcohol, to which a few drops of hydrochloric acid have been added. Since they are almost insoluble in hot neutral alcohol, it is well to digest the leaves first in it, to remove some of the xanthophyll, and any other colour soluble in the liquid. After evaporating to dryness the solution in hot acid alcohol, any colour soluble in water should be dissolved out, and the insoluble portion digested in a cold mixture of alcohol and water, in equal parts, with a little hydrochloric acid, which dissolves much of the phaiophyll, and leaves an impure xanthophyll. After evaporating the clear solution to dryness, strong neutral spirit dissolves the more pure colour, which is always darker and browner when dry than when in solution. As thus obtained, it may be, and often is, a mixture of various coloured substances, and it is only by comparing those from different specimens of leaves that we can arrive at a satisfactory conclusion respecting them. For example, beech trees are occasionally found whose leaves turn in autumn to a very deep colour, almost exactly like burnt sienna. These yield a fine red phaiophyll colour which, when dry, is redder than burnt sienna, and almost exactly like so-called "light-red." When dissolved in alcohol it is of a fine pink-red colour, and corresponds in every particular with that obtained by oxidising the chrysophyll of yellow beech leaves as described above. The leaves of other beech trees turn to an orange-colour, like burnt sienna mixed with raw sienna, and these yield what appears to be a mixture of the above red colour with the browner modification into which the red passes, as already explained; and those leaves which have remained some time on damp ground contain still less of the red and more of the brown, which, when approximately pure, has a tint like that of a mixture of burnt sienna with burnt umber. Besides these, neutral alcohol or water extracts from the leaves a colour which closely corresponds with the orange modification already mentioned; and thus it will be seen that the actual tint of the leaves is the result of a mixture sometimes of at least six different colouring matters.

I have not been able to obtain from the leaves of the elm, chesnut, poplar, or oak, any pink-red colour corresponding to that first formed when the chrysophyll is artificially oxidised, but only the brown modification of burnt umber tint which corresponds almost exactly with what is formed on keeping the artificially oxidised in a dry state. This absence of the redder colour appears to be because it passes into the brown modification much more rapidly than the analogous colour in beech leaves, so that whilst they remain comparatively unaltered for weeks, those of the other trees just named often turn from light yellow to brown in the course of a day or two. The correspondence between the artificial and natural colours in these various cases is extremely close; and though the tint of many leaves is very much modified by the presence of other colours, yet I trust that the examples I have described will sufficiently well illustrate the fact that the most characteristic brown or orange shades are mainly due to the various kinds of phaiophyll, derived from the oxidisation of previously existing soluble compounds more or less characteristic of particular species or varieties of trees. The two principal kinds are those seen to advantage in the beech and in the Spanish chesnut or elm, but it seems as if they very commonly occurred mixed in very variable proportion, not only in different sorts of trees but in different leaves of the same, so as to give rise to every shade of redder or yellower brown, made more or less dull by the presence of more or less of the dark humus. Another very important modification of the tints is that dependent on the continued presence of chlorophyll, after the chrysophyll has been completely altered. This entirely prevents the development of any brilliant tints, since it makes what would otherwise be a fine orange-brown merely a dull brown-green, like that commonly met with in the faded leaves of the alder. In such cases, the chlorophyll is sometimes found to have been completely changed into the dull green modification produced by the action of acids on the brighter green kind. I need scarcely say that many leaves are variably and sometimes very curiously mottled, on account of the changes I have described having occurred partially and in patches. There seems to be a connection between some of these facts and the conditions under which the trees grow, and we cannot therefore be surprised that differences in climate, and variations in the weather of particular years, very materially modify the character of the prevailing tints. On this account, perhaps, some of the illustrations I have chosen may not be appropriate in all cases, being chiefly derived from the district with which I am most familiar.

The following table will serve to show the general relation of the various groups of colouring matters, their prevailing tint when alone, and the varying shades which result from the mixture of varying quantities of any of the two connected by brackets. I have also inserted on the left hand side the condition of the leaves of which, on the whole, the colours may be considered characteristic, commencing with perfect vitality, when carbonic acid is decomposed and oxygen set free, and ending with death and decomposition when the opposite change occurs.

Complete vitality and growth ..	Chrysophyll (gold yellow)	More or less bright green.
	Chlorophyll (deep green)	More or less green-brown.
Low vitality and change	Erythrophyll (crimson-red)	More or less red-scarlet.
	Xanthophyll (bright yellow)	More or less bright orange-brown.
Death and decomposition ..	Phaiaophyl (brown-orange)	Less or more dull brown.
	Humus (brown black)	

According to these principles, we must look upon the production of the fine tints of autumn as evidence of the diminished vital powers of the plants. I presume that this can admit of no doubt, and it agrees with the fact of unhealthy branches of a tree turning yellow whilst the rest remain green. The subsequent development of more sombre tints is evidence of more complete death. Perhaps some of my readers may think that such an explanation robs the fading leaves of autumn of much of their poetry, but, at the same time, I trust that the facts I have described may tend to explain many of the beautiful and varied tints which delight us so much in autumn, and that a knowledge of such general laws will compensate for any loss of poetic sentiment.—*Quarterly Journal of Science.*

Death of Dr. Wetherill.—With regret we announce the death of Dr. Charles M. Wetherill, of Bethlehem, Pennsylvania. Articles from his pen have often appeared in our columns; one of the most recent being "A Note on Itacolumite," in our issue of December 2nd, 1870. He had been a pupil of Liebig, aud his American fellow-workers speak of him as one of the most active chemists America possessed.

FIRST REPORT OF THE ROYAL COMMISSION ON SCIENTIFIC INSTRUCTION AND THE ADVANCEMENT OF SCIENCE.

To the Queen's Most Excellent Majesty.

May it please your Majesty,—

We, the commissioners appointed by Your Majesty to make inquiry with regard to scientific instruction and the advancement of science, humbly beg leave to present to Your Majesty the following first report:—

1. We have heard the evidence of witnesses in reference to the following subjects, forming part of our inquiry, viz.: the Royal School of Mines, the Geological Survey of Great Britain and Ireland, the Mining Record Office, and the Museum of Practical Geology, at present located in Jermyn Street; and also concerning the Royal College of Chemistry, at present lodged in a building in Oxford Street, which institutions are under one head, entitled Director-General of the Geological Survey of Great Britain and Ireland and Director of the Royal School of Mines.

2. There is no necessary connection between the direction of the Geological Survey of Great Britain and Ireland and the government of the Royal School of Mines.

3. The Royal School of Mines and the Royal College of Chemistry, which practically constitute one school of pure and applied science, are not organised in such a manner as to enable them to perform efficiently the work for which they were originally, or are, at present, intended. We base this conclusion upon three grounds: (*a.*) the absence of a chair of mathematics; (*b.*) the absence of physical or biological laboratories in which students can receive practical instruction; (*c.*) the insufficiency of accommodation in the Royal College of Chemistry.

4. The officers of the Geological Survey are greatly hindered in their work by want of accommodation; for although their number has been quintupled during the last 20 years, the space originally allotted to them has not been increased.

5. The space allotted to the Mining Record Office is already insufficient for the proper reception and arrangement of the valuable series of documents accumulated there, and for the accommodation of the public who desire to consult them.

6. The collections in the Museum of Practical Geology require greater space for their proper display than is at present afforded.

7. In order to provide a remedy for the inconveniences which have been enumerated, we recommend: (*a.*) That the building in Jermyn Street be given up to the Survey and to the Museum, with the reservation that the lectures to working men be delivered as heretofore in the theatre; (*b.*) That the building in Oxford Street be vacated by the Royal College of Chemistry; and (*c.*) That the Mining Record Office be lodged with the statistical department of the Board of Trade, or, failing accommodation there, in the building now occupied by the Royal College of Chemistry.

8. Without expressing any opinion, at present, as to the policy of government schools of science, your commissioners, having to deal with the Royal School of Mines and the Royal College of Chemistry as institutions which have existed for 20 years, and which, during that period, have turned out a large number of well-instructed students, consider that such steps should be taken as may be necessary to render their teaching thoroughly efficient.

9. With this object we recommend that the two institutions be consolidated; that mathematics be added to the courses of instruction now given; and that sufficient laboratories and assistance for giving practical instruction in physics, chemistry, and biology, be provided.

10. The institution thus formed (hereinafter called the "Science School") may be conveniently and efficiently governed by a council of professors, one of that body acting as dean.

11. We have further heard evidence concerning the buildings at South Kensington, now nearly completed, and intended for the reception of a projected school of naval architecture and science; and we recommend that the science school should be accommodated in these buildings. We have given careful attention to the considerations in favour of the retention in Jermyn Street of the technical instruction in certain branches, but we are of opinion that these considerations are outweighed by the great advantages to be derived from concentration.

12. We have further heard evidence concerning the Royal School of Naval Architecture and Marine Engineering, now conducted at South Kensington; and we recommend that the theoretical instruction of that school should in future be given in the science school, the general instruction in mathematics, physical science, and mechanical drawing, thus becoming common to both schools. We also recommend that no additional buildings, and no reconstruction of the temporary buildings at present occupied by the Royal School of Naval Architecture and Marine Engineering, should be undertaken until a further report has been received from this commission.

13. We have further heard evidence concerning the system of teaching elementary science under the science and art department; and we are of opinion that the quality of the instruction given under this department would be greatly improved if the teachers received practical instruction in elementary science. Such instruction has, indeed, already been given with marked advantage, although only to a limited extent. The science school will be available for the instruction of many science teachers throughout the country; but we reserve for a further report any expression of opinion as to the precise character of such instruction, and as to the conditions under which it shall be accessible.

14. The organisation of, and accommodation required by, the science school (including its technical branches), and the Royal School of Naval Architecture, will be dealt with in detail in a further report.

All which we humbly submit for your Majesty's gracious consideration.

(Signed)

Devonshire.	W. Sharpey.
Lansdowne.	Thomas H. Huxley.
John Lubbock.	G. G. Stokes.
J. P. Kay-Shuttleworth.	Henry J. S. Smith.
B. Samuelson.	J. Norman Lockyer, (*Sec.*)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, February 3rd, 1871.

Sir Henry Holland, Bart., M.D., D.C.L., F.R.S., President, in the Chair.

"*On some Experiments on Successive Polarisation of Light made by Sir C. Wheatstone*," by W. Spottiswoode, Esq., Treas. R.S. and R.I.

The experiments which formed the subject of this discourse were made by Sir Charles Wheatstone some years ago, but the pressure of other avocations delayed their publication. The term "successive polarisation" was applied by Biot to denote the effects produced when a ray of polarised light is transmitted through a plate of rock crystal cut perpendicularly to the axis. The plane of polarisation is found to be changed on emergence and through a different angle for each homogeneous ray. The

introduction of instrumental means for converting the plane polarisation of the ordinary apparatus into successive, or, as it is more commonly called, circular polarisation, and the explanation of the phenomena thence arising, constitute the main purpose of the communication.

Polarised light is distinguished from common light by the presence of certain peculiarities not ordinarily found; but the peculiarities in question cannot be discerned by the unassisted eye, and require special instrumental appliances for detection. A simple mode of bringing light into the condition in question is by allowing it to pass through a plate of the crystal called tourmalin, cut parallel to the axis; and if the light be then examined by causing it to pass through a second similar plate, held parallel to the former and caused to revolve like a wheel in its own plane, it will be found that the intensity alternately diminishes and revives, being zero for two positions 180° from one another, and a maximum for two positions at 90° from each of the former. Thus combination of tourmalins constitute, in fact, a polariscope, which in general consists of two parts, counterparts of each other, the first for bringing common light into the condition in question, in other words, for *polarising*; the second for examining or *analysing* the light.

The explanation of this fundamental phenomenon is as follows:—

The vibrations upon which the sensation of light depends, may, in ordinary light, take place in any direction in a plane perpendicular to the ray. By the process of polarisation they are all brought into one direction, still, however, perpendicular to the ray; so that throughout the entire ray they lie in one plane. On this account the polarisation here considered is called plane polarisation. There are other kinds of polarisation, such as circular and elliptic, whose names are derived from the curves, or orbits, described by the vibrating particles.

There are also other methods for producing plane polarisation beside that above described; *e.g.*, reflexion at particular angles from the surfaces of transparent media, transmission through parallel plates of glass, &c.; but as they all agree in reducing common light to the same condition, it is unnecessary for the present purpose to allude to them more in detail.

If a ray of polarised light fall upon a plate of doubly-refracting crystal, it is divided into two, whose vibrations lie in planes perpendicular to one another. These rays traverse the crystal with different velocities, and, therefore, emerge with a difference of phase. On entering the analyser the vibrations of both rays are resolved into one plane. If the plane of vibration of the analyser be parallel to one of those of the crystal, one ray will be cut off, the other will be transmitted without change. In any other position of the analyser the transmitted portions of the two rays will interfere so as to produce colour; and if the analyser be then turned through 90°, the portion of the original light cut off in the first position will be transmitted, and *vice versâ*.

Of this theory the following are the experimental results:—If a plate of doubly-refracting crystal, *e.g.*, selenite, be placed between the polariser and analyser, and turned round in its own plane, it will be found that in certain positions at right angles to one another, no effect is produced. These may be called neutral positions. In all other positions the field is tinted with colour, which is most brilliant when the plate has been turned through 45° from a neutral position. If the analyser be turned, the crystal remaining still, the colour will fade, and entirely vanish when the angle of turning amounts to 45°. From this position the complementary colour will begin to appear, and will be brightest when the angle of turning amounts to 90°. The colour depends upon the thickness of the crystal, so that by a suitable preparation any arrangement of colours may be produced.

So far for plane polarisation. The principle of circular or successive polarisation, as regardst hep resent purpose, is as follows:—

If two sets of rectilinear vibrations lying in planes perpendicular to one another meet and combine, the resulting vibration will be curvilinear, whose form and position depends upon the difference of phase of the components. If the second set be in advance or in rear of the first by a quarter of a wave length, the resulting vibration will be circular; but the motion will in one case be direct (like the hands of a watch), in the other it will be reverse.

If two sets of circular vibrations in opposite directions meet and combine, the resulting vibrations will be rectilinear, and the position of its plane will depend upon the difference of phase of the components. If the second set advance upon the first, the plane of resultant vibration will undergo direct rotation; if it recede, it will undergo reverse rotation.

If in such an experiment white light be used, the vibrations of the different component prismatic colours will (on account of their different wave lengths) undergo different retardation; and, consequently, the resulting vibrations will lie in different planes, arranged in prismatic order. The order will be from red to violet, or *vice versa*, in accordance with the law stated above.

If a ray of plane polarised light fall upon a metallic reflector, it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector; and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined at an angle of 45° to the plane of incidence, the two rays into which it is divided have nearly the same intensity. At an angle, nearly 45° which varies with the metal employed, but which is perfectly definite, the intensities become accurately equal. And further, if the angle of incidence have a particular value dependent upon the nature of the metal (for silver 72), the retardation will amount to a quarter of a wave length. These two rays, on leaving the reflector, will re-combine, and in accordance with the laws above given, will, in the last-mentioned circumstances become a circularly polarised ray. Lastly, the direction of motion in this circular ray will depend upon the side on which the original plane of vibration is inclined to the plane of incidence; if, when it is inclined on one side, the circular ray becomes right-handed, then, when it is inclined on the other, it becomes left-handed.

Reverting then to the phenomena of double refraction produced by a plate of crystal cut parallel to the axis on a plane polarised ray; let the crystal be placed in such a position that the planes of vibration of the two resulting rays are inclined at angles of 45° on the two sides respectively of the plane of incidence; and let there be interposed between the crystal and the analyser a silver plate at an angle of 72° to the direction of these emergent rays. Each of these rays will, in virtue of the principles enunciated above, be converted by reflexion into a circularly polarised ray; but one will be a right-handed and the other a left-handed ray; and the difference of phase produced by the doubly-refracting plate will be undisturbed by the reflexion. This difference of phase depends, as is well-known, upon the wave length; in other words, upon the colour of the light. So that the two circular rays will combine to form a plane polarised ray, whose plane of vibration depends upon the difference of phase, *i.e.*, upon its colour. And if, finally, the light be then examined by an analyser in the usual manner, we shall have all the phenomena of circular or successive polarisation.

From what has been stated above, it appears that the direction of motion in the two circular rays, and, consequently, the order of colours produced, depends upon the position (to the right or left of the plane of incidence) of the ray which has been most retarded in the passage through the crystal plate. If, therefore, the plate being in a given position, the colours appear in an ascending order, then on turning the plate through 90° in its own plane, or on turning it over about an axis in the plane of incidence, the swifter and the slower rays will change position, and the order of colours will be reversed.

The reversal of the order of colour may be exhibited in

another way. Unianal crystals are divided into two classes: one, called positive (*e.g.*, quartz), in which the extraordinary ray moves more slowly than the ordinary; the other, called negative (*e.g.*, Iceland spar) in which the ordinary ray is the slowest. If, therefore, a plate of quartz placed with its axis at 45° on one side of the plane of incidence give the colours in one order, a similar plate of Iceland spar similarly placed will give them in the reverse order.

The same principles apply to the case of bianal crystals cut parallel to a plane containing the two optic axes. A ray of plane polarised light transmitted through such a plate is divided into two, whose vibrations respectively bisect the angles formed by the two axes; the line which bisects the smallest angle is called the intermediate section, and the line perpendicular to it the supplementary section; and the order of the colours depends upon the relative velocity of the two rays. In selenite the ray whose vibrations lie in the supplementary section is the slowest; in mica it is the swiftest. Hence these two crystals will, all other circumstances being alike, give opposite orders of colour, and may be regarded as positive and negative respectively, like quartz and Iceland spar.

The phenomena by which these principles may be illustrated are very numerous and varied, but are better seen than described.

NOTICES OF BOOKS.

Aunt Rachel's Letters about Water and Air. Longmans, Green, and Co. 1871.

One of the most important results of the present consideration of education is the improved tone of the text-books issued to the public. Works dealing with elementary science have long been such as afforded but poor introduction to the higher branches; written for children, they have been too childish. Aunt Rachel's Letters to Ned and Annie about Water and Air are eminently calculated to place before young minds the most important facts, even of the latest date, with regard to heat in relation to these substances. What Aunt Rachel has to tell about latent heat, the composition of water, the principles of the air-pump, thermometer, and the chemistry of combustion, is well told, in simple language; and, by placing it in the power of children to perform a few experiments themselves, an interest more than theoretical is likely to be engendered in their minds. It is to be hoped there will be found many aunts purchasing their sister's little work to give to their nephews and nieces.

Our Meat Supply from Abroad. A paper read before the Liverpool Literary and Philosophical Society, January 9th, 1871. By T. J. Hutchinson, F.R.G.S., F.R.L.S., &c., H. B. M. Consul for Callao, Peru. Liverpool: D. Marples. 1871.

Our readers are doubtless acquainted with bisulphite of lime, a substance which, twenty-five years ago, was employed by Professor Melsens to prevent the rapid alteration of saccharine liquids. The main subject of this excellent lecture is the use of bisulphite of lime as prepared according to the patent of Messrs. Medlock and Bailey, being a preservative solution made of 1 gallon of bisulphite of lime solution, sp. gr. 1·050, ½-pint of common salt, and 4 gallons of water, for the purpose of preserving meat in a good and fresh state, not simply for a few days, but for several months, and even for years. The experience of Consul Hutchinson while at Rosario (Argentine Republic), confirmed by that of a large number of disinterested parties both in that and in this country, prove, as set forth in this pamphlet, the great value of the preservative solution alluded to. We need not enter into more details on this important subject, which, we hope, will soon lead to the importation, on the large scale, of meat of good appearance, nutritious quality, free from taste of chemicals, and, as stated by the above-named author, for sale here at from 4d. to 5d. per pound. The realisation of the first and second of these propositions the author states to have been proved, while the third may be considered very probable.

CORRESPONDENCE.

CONGELATION BY MEANS OF BISULPHIDE OF CARBON.

To the Editor of the Chemical News.

Sir,—Last winter, when using bisulphide of carbon with a spray producer, a hoar-frost concretion was rapidly formed on anything held close to the tube, so much so, as soon to close up the orifice. This is really frozen moisture entangled with bisulphide of carbon which communicates a strong smell and taste.

When the spray producer is used merely to force a jet of air into bisulphide of carbon, a film of hoar-frost immediately forms on the surface, and is, by the agitation, continually increased and mingled with the fluid so as to make it opaque. If the tube be now removed, these films start up to the surface and disappear, assuming curious linear forms.

On again directing the jet of air upon the fluid, the films soon unite and form small white frozen masses, and the sides of the vessel became incrusted therewith. Finally, both fluid and solid contents disappear. Here also, as at first, the concretions really consist of frozen moisture, tasting and smelling of the bisulphide of carbon with which it is entangled.—I am, &c.,

G. A. Keyworth.

MISCELLANEOUS.

Royal Institution of Great Britain.—The following are the arrangements for the Lectures after Easter, 1871:—On Tuesdays, April 18th and 25th, and May 2nd, William Pengelly, Esq., F.R.S., F.G.S., will deliver three lectures "On the Geology of Devonshire, especially of the New Red Sandstone." On May 9th and 16th, Charles Brooke, Esq., M.A., F.R.S., will lecture "On Force and Energy." On May 23rd and 30th, and June 6th, the Rev. Professor Haughton, M.D., F.R.S., will lecture "On the Principle of Least Action in Nature, Illustrated by Animal Mechanics." On Thursdays, April 20th to June 8th, Professor Tyndall, LL.D., F.R.S., will deliver eight lectures "On Sound;" and on Saturdays, April 22nd to June 10th, J. N. Lockyer, Esq., F.R.S., will deliver eight lectures "On Astronomy." The lecture hour is three o'clock. The following are the probable arrangements for the Friday Evening Meetings after Easter, 1871, to which Members and their friends only are admitted:—April 21st, Professor Blackie, F.R.S.E., "On the Pre-Socratic Philosophy." April 28th, Professor Odling, F.R.S. May 5th, W. R. S. Ralston, M.A., Trinity College, Cambridge, "On Russian Folk-Lore." May 12th, Professor Huxley, F.R.S. May 19th, Colonel Jervois, R.E., C.B., Secretary of the Defence Committee, and Deputy Director of Fortifications, "On the Defence of the United Kingdom." May 26th, Sir J. Lubbock, Bart., M.P., F.R.S., M.R.I., "On Relationships." June 2nd, Professor Thomas Andrews, F.R.S., Principal of Queen's College, Belfast, "On the Gaseous

and Liquid States of Matter." June 9th, Professor Tyndall, LL.D., F.R.S., M.R.I.

Glasgow.—Supper of Dr. Moffat's Chemistry Students.—The first supper of Dr. Moffat's Chemistry Students took place on Friday evening, the 17th inst., in the Waverley Hotel, Sauchiehall Street, Dr. Moffat in the chair, supported by Principal McCall, Drs. Charteris, Russell, Robertson, Colonel Shaw, Alex. Duncan, Esq., B.A., Secretary of the Faculty of Physicians and Surgeons, Glasgow, Messrs. Whitesmith, Stevenson, Bannerman, and others. Upwards of one hundred students were present, and, after a sumptuous supper, the chairman gave "The Queen" and other loyal toasts. Dr. Moffat, in replying to "The Chemistry Classes," referred to his commencing to teach chemistry in Glasgow six years ago; five years ago six students enrolled for laboratory work during the entire session. This session already there have enrolled no less than 132 laboratory students, and, with the veterinary pupils, eighty at lectures. There have been thirteen excursions to Chemical Works attended by nearly 400 students. The musical talent of the students was developed in a remarkable manner; Messrs. Maclaren, Batty, M'Kenzie, Fisher, Miller, Stevenson, and many others specially distinguishing themselves. A very happy evening was spent.

Scientific Curiosity Connected with the War Indemnity France is to Pay to Germany.—In order to give our readers the real gist of the following we retain some French words, explaining first that a milliard means a thousand millions. On the 31st of December next, there will not have elapsed a milliard of minutes of time since the beginning of the Christian era; that milliard of minutes will not be complete before the date of 28th of March, 1901. If, consequently, there had been put in a safe a five-franc piece every minute since the beginning of the era alluded to, the indemnity of 5 milliards of francs would not be paid off in capital—interest exclusive—before midday of the 28th of March, 1901. The five-franc piece has a weight of 25 grms., and the 5 milliards will, therefore, weigh 25 millions of kilos., a weight which, if loaded on railway trucks, each containing 5000 kilos. (5 tons), would require 5000 trucks; estimated in copper, the weight alluded to would be 500 millions of kilos., and would require 100,000 railway trucks for being conveyed. The diameter of the five-franc piece is 37 millimetres; if, therefore, 1 milliard of these pieces are laid down so as to join quite closely, this would give a length of 37 million metres = 37,000 kilometres, equal to 74 times the distance from Paris to Strasburg, which is 500 kilometres, and more than 32½ times the distance from Paris to Berlin, = 1134 kilometres. It would, therefore, be possible to pave, with 1 milliard of five-franc pieces, a road from Paris to Strasburg, which road would have a width of 74 five-franc pieces = 2·738 metres; or a similar road might be made from Paris to Berlin, and have a width of nearly 33 of the same pieces, that is, 1·20 metre. In order to cover a surface of a square metre, 730 five-franc pieces are required; 1 milliard of these pieces will, therefore, cover 136 hectares, 98 ares, 63 centiares; that is to say, nearly three times more than the surface occupied in the Champ de Mars at Paris by the Exposition of 1867, which only occupied a space of 46 hectares, but the indemnity to be paid is 5 milliard of francs which, put together, would cover a surface of 322 hectares, 10 ares, 75 centiares; that is, seven times more than the space occupied by the Exposition just named. When three pieces of five francs are placed upon each other, the height of the pile is equal to 8 millimetres; the height attained by piling upon each other 1 milliard of these pieces would be 2,666,666 metres, 66 centims., that is to say, if placed edgewise, flat on the ground, the height would be within 158½ kilometres, which is very nearly the distance from Paris to St. Petersburg. The kilo. of one and of five-franc pieces contains, in each case, 900 grms. of pure silver.—*Indépendance Belge.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus des Séances de l'Académie des Sciences, November 7, 1870.

This number contains the following original papers relating to chemistry and collateral sciences:—

Early Aërostatics and Aëronautics.—J. Dumas.—In his capacity as permanent secretary, this eminent *savant* read a portion of a report by Lavoisier, being appended to the *procès-verbaux* of the first meeting of a committee appointed by the old Academy of Sciences (as existing before the Revolution of 1789) to report on the aërostatic machines. The Committee held its meetings at the Hotel de la Rochefoucauld (the word hotel is here, as in many instances, simply used to indicate a large building, either public or private property—*e.g.*, Hotel de la Monnaie, Hotel de la Préfecture), on December 27th, 1783, the members present being the Duke de la Rochefoucauld, MM. Le Roy, De Condorcet, Tillet, L'Abbé Bossut, Lavoisier, Brisson, Berthollet, and Coulomb. As might be expected from the celebrated Lavoisier, his report sets forth very concisely, and with great precision, the principal points which should guide the Committee in its labours. It appears that a M. Meusnier had about that time invented certain apparatus for ascending in the air and for regulating the ascent, and, to some extent, direction of a balloon filled with gas (hydrogen), which apparatus is named after the so-called swimming bladder present in some kinds of fish.

Quantitative Spectral Analysis.—J. Janssen.—However valuable, this essay is not well suited for any useful abstraction.

Description of Telescope Specially Constructed for Military Purposes.—A. Cazin.—The detailed account of a contrivance whereby it is possible to observe, from behind a wall or earthworks, an army outside these means of defence.

Some Documents bearing upon the Domestic Economy and Food used in Ancient Egypt under the Ptolemies.—Dr. Egger.—This paper deserves attention on account of the scientific curiosities brought to light by the deciphering of rolls of papyrus used for writing certain records, in this instance, relating to the subjects above named.

November 14, 1870.

Reasons why the Food of Men and of the more highly-organised Animals ought to be of a Complex Chemical Nature. E. Chevreul.—This very lengthy and, as might be expected from the author, excellent essay, does not admit of any useful abstraction.

Tridodecuple or Decemdiurnal Period of the Atmospheric Phenomena, and on the Influence they Exert upon the General State of Health.—Ch. Sainte-Claire Devllle.—This essay, the first part of a lengthy monograph on this subject, contains mainly a review of the bearing of facts meteorogically observed upon the general state of health of the inhabitants of a country.

Movements Executed by Birds while on the Wing.—Dr. Marey.—A mechanico-physiological paper illustrated by a series of woodcuts.

Thermo-Chemistry: Heat of Formation of Nitrogen Compounds.—Professor Berthelot.—This paper contains a series of tabulated results exhibiting the heat of formation of—binoxide of nitrogen; nitric acid; nitrates of potassa, soda, ammonia, lead, and silver; and of nitrates.

Aërostation: Experiments with the Giffard System, and Description of a Steam Air Balloon.—H. Giffard.

Nitroglycerine and Divers Kinds of Dynamite.—MM. Ch. Girard, A. Millot, and G. Vogt.—This paper contains the account of a series of experiments made with the substances alluded to; also on the best and safest means of preparing nitroglycerine on the large scale; and, lastly, lengthy details on the explosion, by percussion mechanically produced, of nitroglycerine mixed in various proportions with inert substances. It appears that dynamite, if left exposed to air for a length of time, becomes inactive.

November 21, 1870.

This number opens with a continuation of the monograph mentioned in the previous number by M. Ch. Sainte-Claire Deville. The other papers and memoirs relating to chemistry and collateral sciences are—

Dynamite, and its Applicability to Artillery and Warlike Purposes.—P. Champion.

Principle of a New Steerage System for Air Ballons.—Dr. Sorel.—Illustrated with a woodcut.

Preservation of Meat.—E. Pelouze.—This brief paper does not contain the description of the process by which the preservation is effected. It appears that the author has re-discovered a process for the preservation of meat which was invented about the end of last century by M. Vilaris, a pharmacist at Bordeaux, who had succeeded in preserving meat, while exposed to air, for several years, while analysis has never been able to detect in the meat any preservative agent. Considering the great importance of this subject, M. Pelouze, after consulting with M. Dumas, considered it best not to publish, at present, the particulars of the process, but to deposit with the Academy the samples and memoir under seal.

Influence which Coffee and Cacao exert as Food.—Dr. Rabuteau.—This paper contains the account of some experiments made with dogs, to which the author gave diets in one case consisting daily of 20 grms. of bread, 10 grms. of fresh butter, and 10 grms. of sugar; in the other case, 20 grms. of cacao, 10 grms. of sugar, and an infusion of 20 grms. of well-roasted coffee. From these experiments the author draws conclusions leading him to consider coffee and cacao as simply preventing de-nutrition. This view was objected to at the meeting by MM. Payen, Dumas, and Chevreul, whose lengthy discussions on this subject are reproduced. As regards cacao (commonly, but erroneously, in this country named cocoa), there can be no doubt that, containing as it does from 17 to 20 per cent of albuminous matter, with from 10 to 12 per cent of starch, from 40 to 50 per cent of fat, and among its mineral matter phosphates, it is food. M. Chevreul very properly observes, among other matters, the existence of idiosyncracy, and its influence on the individual tastes, and hence also more or less on the action of various alimentary substances, pointing out that he himself has, from his earliest years, an invincible repugnance against wine, milk, fish, and various vegetables, none of which he ever partakes of, but for all that it would, of course, be absurd to deny the nutritive properties and value of these substances.

Historical Notice on the Domesticated Cat of the Ancients, more especially Egyptians.—F. Lenormaut.

Stratigraphical Relation existing between divers Meteoric Rocks.—S. Meunier.

Effect of Various Phenic Acid Preparations against Small Pox.—Dr. Bobœuf.—The author recommends the internal and external use of *phénol sodique*, phenate of soda, as safe, and as also preventing pitting.

Les Mondes, March 2, 1871.

This number, the first we have received after the publishing of this periodical has been resumed, opens with an address especially addressed to the French readers. Among the original communications we notice the following:—

Extract from a Letter from M. C. Woestyn to the Editor.—This contains the following particulars bearing upon the beet-root sugar industry:—In Italy, MM. Tomassini, Guarieri, and Castellani have obtained a concession from the Government to manufacture, for a period of fifteen years, beet-root sugar, to be free from excise duty. At Valenciennes, M. Margueritte's process for the extraction of sugar from molasses by means of alcohol was carried on last winter on the large scale, and has given excellent results, since the quantity obtained has been 36 per cent greater than the laboratory assays. The Seyfert process (sulphurous acid) has been eminently successful at Arlowetz and in Belgium, and is also adopted in several sugar refineries at New York.

Death of Professor Morren.—Our readers will hear with regret of the demise of this *savant*; he died at Marseilles in October last, in the 69th year of his age.

Powerful Centrifugal Pumps.—MM. Neuf and Dumont.—An account of some work effectually performed by simple and readily manageable machinery under very exceptional conditions—viz., the filling with water of dry moats of a total length of more than 6 kilometres, and requiring the lifting of nearly 4,000,000 cubic metres of water (equal to as many tons' weight), to a height of 8 metres, a work actually performed in a few days. Some of the pumps made by the firm named discharge a cubic metre of water (1 ton weight) every second of time.

This number contains the index for the numbers published for last year.

American Journal of Pharmacy, March, 1871.

Leaving the papers more particularly bearing upon pharmacy, we notice the following:—

Glycerine, its Quality as it Exists in Commerce.—J. P. Remington.—This paper contains the account of a series of researches made by the author on six different samples of American glycerine. The specific gravity varied from 1·254 to 1·240. Three samples were colourless, two yellowish, and one quite dark. Odour when warm varied from none at all to fatty and empyreumatic. Precipitate with nitrate of silver—2 none, 1 heavy white, 2 rose colour. Action of sulphuric acid—4 discoloured, 2 slightly so; none contained sulphate of lime; 4 were quite free from all lime. Action of ferrocyanide of potassium—1 a precipitate, 2 opalescent, 1 clear, 2 slight precipitate; with hydrosulph. of ammon.—1 a slight precipitate; with chloride of barium—3 no precipitate, 1 slight precipitate, 1 precipitate, 1 opalescent. All samples free from sugar; and on being tested for ethyl-butyrate—5 slight odour and 1 very slight. Therefore these samples are, on the whole, good specimens of the article as sold in the United States.

Presence of Manganese in Beech Nuts.—Dr. J. E. De Vry.—After referring briefly to a sentence in the introductory address of the chairman of the last Pharmaceutical Conference, bearing upon the presence of manganese in the ashes of the beech and birch, the author states that, while studying in Germany, now some thirty years ago, he collected beech-nuts in the vicinity of Giessen, and found the ashes of these nuts to contain a rather large percentage of manganese, which was readily explained by the fact of that mineral being present in that locality rather abundantly. In 1847, being at the meeting of the British Association at Oxford, the author gathered some unripe beech-nuts in Blenheim Park, which, on being afterwards tested, were found to contain a relatively large amount of manganese, although grown in a very different soil. A third analysis of the ashes of beech-nuts, collected in the wood of the Hague, confirmed the same fact.

Freezing-Point of Mixtures of Glycerine and Water.—C. Bullock.—The glycerine used was common, sp. gr. = 1·250 = 29° Baumé; quantity of water in all experiments, 1 gallon. Temperatures Fahr., with ½ pint, fuses at 30°, 1 pint at 24°, 1½ pints at 18°, 2 pints at 10°, 3 pints remain fluid at 3°.

Annalen der Chemie und Pharmacie, February, 1871.

The following original papers and memoirs are contained in this number:—

Researches on Substituted Phenols; Part I. Chlorphenol-Sulpho Acids.—T. Petersen and R. Baehr-Predari.—This part of an exhaustive monograph contains the following sections:—Chlorphenol; ortho-chlorphenol-sulpho acids; α chlorphenol-sulpho acid (ortho-mono-chlorphenol-meta-monosulpho acid) salts; potasssium salts, illustrated with several crystallographic diagrams; ethyl-α-chlorphenol-sulpho acid (α-chlorphenetol-sulpho acid); isomers of the α-chlorphenol-sulpho acid; β-chlorphenol-sulpho acid (ortho-mono-chlorphenol-para-mono-sulpho acid); chlorphenol-disulpho acids; action of concentrated nitric acid on the chlorphenol-sulpho acids; dinitro-chlorphenol fusing at 80·5° (α-chlor-dinitro-phenol of MM. Faust and Saame); dinitro-chlorphenol fusing at 114°; mono-nitro-dichlorphenol fusing at 106°; dinitri-chlorphenol fusing at 69°; constitution of the nitro-chlorphenols; monochlor-mononitro-phenols; monochlor-dinitro-phenols; trichlor-mononitro-phenols and trichlor-dinitro-phenols; review of the bodies treated of with their formulæ.

Researches on the Consititution of Diamylen.—W. von Schneider.—This lengthy essay is so full of lengthy and complex formulæ, all bearing upon the elucidation of the constitution of diamylen ($C_{10}H_{10}$), that the contents of the paper, however valuable, are not suited for useful abstraction.

Juice contained in the Muscles of the Phocæna communis.—Dr. O. Jacobsen.—This paper contains the detailed account of a series of experiments made with the juices of the flesh of the dolphin, and compared with that of horses. 10,000 parts of the former gave—Kreatine, 6·10; sarkine, 1·05; xanthine, a trace; inosite, 0·08; lactic acid, 7·45. Of the latter—Kreatine, 7·6; sarkine, 1·28; xanthine, 0·11; inosite, 0·30; lactic acid, 4·47; taurine, 0·70.

Investigation of Indian Geranium Oil.—Dr. O. Jacobsen.—The essential oil alluded to exhibited a faintly acid reaction to test-paper; colour, greenish yellow; sp. gr. at 20°, 0·887. The author treats at length on the products yielded by fractional distillation of this oil; next on geraniol, $C_{10}H_8O$, a liquid, sp. gr. at 15° = 0·8851, optically inactive; and on the combinations of that body with chlorine, $C_{10}H_{17}Cl$, bromine and iodine, cyanogen, sulphur, and lastly on geraniol ether, $C_{20}H_{34}O$.

Investigation of a Very Hard and Compact Swedish Peat.—Dr. O. Jacobsen.—Sp. gr. of the peat—1·07; hygroscopic moisture, 11·5 per cent; the dry substance contained 5·02 per cent of ash. Composition, in 100 parts—Ash, 5·02; carbon, 51·38; hydrogen, 6·49; nitrogen, 1·68; oxygen, 35·43.

Some of the Combinations of Chloral with Alcohol and with Amides.—Dr. O. Jacobsen.—This essay contains the following sections:—Chloral hydrate; chloral methyl, amyl, alcoholate; chloral alcoholate; chloral acetyl alcohol; chloral acetamide; chloral benzamide; chloral urea.

Polytechnisches Journal von Dingler, second number for February, 1871.

Combination of the Interference Scale with the Photographic Spectrum Scale.—Dr. J. Müller.—An optico-algebraical essay.

Calcination of Sulphur-Containing Ores, and Description of a Newly-Devised Calcination Furnace.—R. Hasenclever and W. Helbig.—This excellent essay bears upon the best method of utilisation of pyritic ores for the purpose of sulphuric acid manufacture; but, since the paper is illustrated by several engravings required for the proper understanding of the description, we cannot enter into any details here.

Account of some Experiments made with the view of Utilising Burnt Iron Pyrites for the Production of Metallic Iron.—Dr. E. Richters.—Reserved for full translation.

Pyrometrical Assay of a Kaolin found near Strehlen, in Silesia, and Chemical Analysis of the Material obtained from it by Schlamming.—Dr. C. Bischof.—After describing the locality and other local conditions under which the mineral alluded to has been discovered, the author gives a detailed account of the pyrometrical testing of the mineral as found, and of the same schlammed. Both materials were heated up to the melting-point of platinum; in each instance the materials withstood this high temperature without fusion,

and showed also a white colour. The results of the analysis are, in 100 parts—Alumina, 37·54; silica in chemical combination, 44·08; silica as sand, 2·62; magnesia, 0·23; lime, 0·08; peroxide of iron, 0·94; potassa, 0·84; chemically-combined water, 12·60; hygroscopic water, 0·66;—total, 99·53. Formula, $13{,}10(Al_2O_3{,}1.42SiO_3)+RO$.

Constituents of the Water of the Rhine at Cologne, and on the Use that can be made of that Water for Domestic Purposes.—Dr. H. Vohl.—The introduction to this excellent paper contains a *resumé* and general review of the water of rivers. The purest known to exist at present is the water of the small Swedish river Loka, which, in 100,000 parts, contains only 0·434 part of mineral substances; while the Thames at London Bridge contains, in the same quantity of water, from 69 to 70 parts; the Seine, in Paris, 23 to 24; and the river Jordan from 130 to 131 parts of mineral substances in the same quantity of water. The paper next contains, in full details, the analysis made of the water of the Rhine opposite Strasburg (1848), by Sainte-Claire Deville; the water of the Rhine at Arnhem (Netherlands), by Dr. J. W. Gunning (1853); and also three former analyses of the Rhine water at Cologne, drawn from that river at different localities opposite the city alluded to, made in 1853, 1855, and 1861. The analyses of the author of this paper are no less than twelve in number, the water being drawn at three different places and at various periods, and are by far too extensive and lengthy to be here quoted in full. We can only observe that the quantity of solid matter present in the Rhine water at Strasburg, as compared with that quantity at Arnhem, shows a decrease of 31·242 per cent for the water at the last-named place, a phenomenon due to loss of carbonic acid. 10,000 parts of Rhine water at Strasburg contain, of total solid mineral matter, 2·317; at Arnhem, 1·593; at Cologne, varies from 2·503 to 2·443 and 2·30.

Description of a Stove which admits of Very Precisely Regulating the Consumption of Fuel and the Desired Degree of Temperature Required in Rooms.—Dr. Meidinger.—Illustrated with woodcuts.

Annalen der Physik und Chemie, von Poggendorff, No. 1, 1871.

Relation Existing between the Hemihedric Crystalline Form and the Thermo-Electric Condition of Iron Pyrites and Cobalt Glance.—Dr. G. Rose.—This lengthy monograph, illustrated by a series of engravings, is, notwithstanding its intrinsic value, not well suited for abstraction.

Designation of Hemihedry in Application to the Stereographic Projection.—E. Reusch.—A mathematico-crystallographical paper illustrated with a series of engravings.

Electrical Oscillations in Induced Conductors.—J. Bernstein.—This monograph, illustrated by a series of engravings, is divided into the following sections:—Description of apparatus; course of the induced current in the secondary coil of an induction apparatus; electrical condition of the secondary coil when the primary coil is disconnected; condition of the primary coil at the time of disconnecting it from the source of electricity.

Influence Exercised by the Density and Temperature on the Spectra of Gases Heated to Redness.—F. Zöllner.—Algebraico-physical essay.

Mineralogical Contributions.—E. E. Schmid.—This crystallographico-mineralogical essay, illustrated by engravings, contains the following chapters:—On whewellite and forms connected therewith; on desmin—

$$\left(\begin{matrix}CaO\\NaO\end{matrix}\right\} = SiO_3)\Big) + (Al_2O_3 + 3SiO_3) + 6HO\ ;$$

on mesolite.

Cooling and Conduction of Heat in Gases.—Dr. F. Narr.—This lengthy memoir is divided into the following chapters:—Introduction; description of apparatus, of the mode of experimenting, and of the method of calculating the results; results of experiments in a tabulated form; theoretical deductions drawn from the experiments.

A New Experiment, and some Observations on the Theory of Leidenfrost's Drop.—E. Buelde.—Illustrated by woodcuts.

Anomalous Dispersion of Light on Bodies Superficially Covered with Colour.—A. Kundt.—An algebraico-optical essay.

Observing of the Sun's Protuberances in Monochromatic Light.—Dr. W. Zenker.

Bulletin de l'Académie Royale des Sciences, des Lettres et des Beaux Arts de Belgique, No. 12, 1870.

This number contains the following original papers and memoirs bearing upon physico-chemical and collateral sciences:—

Aurora Borealis observed at Brussels on the 19th of November, 1870.—A. Quetelet.

Periodical Appearance of Meteorites in November, 1870.—A. Quetelet.

Description of a Pocket Stereograph.—J. Plucker.

Review of the Progress Mineralogical Science has made in Belgium during the last thirty-five years.—G. Dewalque.—A lengthy lecture on this subject, containing, in condensed form, all that has been done in the country alluded to in scientific, as well as practical geology, mineralogy, mining, and quarrying, with quotation of the titles of a very large number of books, maps, and other printed and published works.

NOTES AND QUERIES.

Ferricyanide of Potassium in Photography.—In Mr. Vogel's paper, published in No. 2 of the "German Chemical Society's Report!' for 1871, and extracted by you on the 24th inst., I read that this gentleman has just discovered a method of applying the action of ferricyanide of potassium in photography. I beg to mention that, in the year 1865, I used a paper prepared with that salt for the same purpose, and obtained, on exposure under a negative, very good results, the image being subsequently intensified by treating with a mixed solution of perchlorides of iron and tin, and fixed by simple washing with water. Some photographs prepared by this process I still have by me.—Rowland J. Atcherley, Ph.D.

Valonia.—Some time since, a letter appeared in your "Correspondence" from C. A. Cameron, of Dublin, enquiring why Smyrna valonia commanded a higher price than Greek, as by analyses he had ascertained the amount of tannic acid in both to be nearly alike. This may be accounted for in two ways. First, the prejudice is certainly on the side of Smyrna, as both in brightness of colour and strength it generally exceeds the Greek. The majority of tanners are unable to prove, by chemical tests, the percentage of tannic acid, and are dependent alone on the use of the hydrometer, which simply gives the density. Secondly, the season is a remarkable one, the crop of valonia, being very deficient in tannic acid. This especially refers to Smyrna, the majority of the Greek being about the usual average in strength. My own experience corroborates Mr. Cameron's analysis; one sample of Greek, worth, or rather selling price, £14 10s., contained 20·47 per cent of tannic acid. When a sample of Aidin, the best valonia, selling now for £18, contains only 22·92 per cent of tannic acid. Two or three years since, the best Aidin valonia stood 35·22 per cent. This will, I hope, explain the apparent disproportion in the price of the two valonias.—W. N. Evans.

Anthracene.—(Reply to "Anthracen.")—According to Dr. J. R. Wagner (*Jahresberichte über die Fortschritte der Chemischen Technologie*, vol. xv., p. 598), the following process may be used:—Estimate first the point of fusion of the *green grease*; next, take from 5 to 10 grms. thereof, place it between a thick layer of sheets of filtering paper, and press strongly between previously well warmed metal plates, and weigh the material after that. Boil next with alcohol; repeat this with precisely the same bulk of alcohol twice, or oftener; cool, filter after cooling, wash the residue with cold alcohol, and weigh the residue as being nearly pure anthracen. After that, estimate again the point of fusion of the residue, which point should be 210°.

Anthracene.—In reply to "Anthracen," there does not appear to exist any process by which the amount of anthracene can be determined in the commercial article with any exactitude. A process, however, can be given by which comparative results may be obtained of sufficient accuracy for most commercial purposes. The melting-point should first be determined, and, if the substance be in a pasty form, a certain weight is taken and pressed in blotting-paper between two plates previously warmed; but, if it be in a powder or solid, this may be dispensed with. It is now well washed with cold alcohol, and if a series of determinations are to be made the same amount of alcohol must always be used. If the distillation of the tar has not been pushed beyond a certain point, the substance should be nearly pure anthracene, and if the melting-point be from 210° to 215° it may be weighed as such. This process will yield tolerably fair comparative results. If, however, the distillation has been carried further, hydrocarbons higher in the series are obtained, and which will be present in the residue left by washing with alcohol. The existence of chrysen may be known by the bright yellow colour of the residue. To separate this substance, recourse must be had to carbon disulphide, in which it is only slightly soluble. The difference between the weights before and after treatment with the carbon disulphide will give the amount of anthracene. The chrysen is left as a pure yellow powder fusible at about 240° C. To take advantage of the different degrees of solubility of the various hydrocarbons seems to be the only way as yet of estimating their amounts; and it may be assumed as a general rule that they are soluble in their more common solvents inversely in proportion to the weight of their melting-points, chrysen being much less soluble than anthracene, and the latter than naphthaline. In Dingler's *Polytechnisches Journal*, vol. cxcvi., p. 543, there is an interesting paper by Dr. Gessert, "On the Manufacture of Anthracene," from which some of the above facts are taken. Dr. Gessert states that 100 parts of alcohol, benzol, or carbon disulphide dissolve, respectively, 0·6, 0·9, and 1·7 parts of anthracene, and that the homologues higher than that hydrocarbon are prejudicial to the manufacture of alizarine. It would be interesting to know if the same remark applies to naphthaline; will some of your correspondents give information on this point?—Eureka.

MEETINGS FOR THE WEEK.

Monday, April 3rd.—Medical, 8.
—— London Institution, 4.
Tuesday, 4th.—Zoological, 9.
—— Civil Engineers, 8.
Wednesday, 5th.—Microscopical, 8.
—— Pharmaceutical, 8.
—— Geological, 8.
Thursday, 6th.—Chemical, 8. W. Mattieu Williams, "On Burnt Iron and Burnt Steel." Henry E. Armstrong, "On the Formation of Sulpho-Acids."
—— London Institution, 7.30.

THE CHEMICAL NEWS.

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ON ANTHRAFLAVIC ACID,

A YELLOW COLOURING MATTER ACCOMPANYING ARTIFICIAL ALIZARINE.

By EDWARD SCHUNCK, Ph.D., F.R.S.*

The artificial formation of alizarine is a process of so much importance both theoretically and practically, being, in fact, the first instance in which a natural colouring matter has been produced by artificial means, that everything connected with it must in the eyes of the chemist possess more or less importance, especially when we consider that it is chiefly to alizarine that madder owes its valuable dyeing properties. The process itself, as described by its discoverers, Gräbe and Liebermann, seems exceedingly simple, and consists in the conversion of the hydrocarbon anthracene, $C_{14}H_{10}$, by the action successively of an oxidising agent, of bromine or sulphuric acid, and of caustic alkali, into alizarine, $C_{14}H_8O_4$. Nevertheless, the product obtained on a large scale for the use of dyers and printers by this process is very far from being pure alizarine; so far, indeed, that some persons are inclined to doubt its perfect identity with the natural substance. Its solution in caustic alkali, for instance, has not the fine violet colour of a solution of pure alizarine, but is more or less purple or even red, and it differs in other respects. Now, though I have never entertained much doubt as regards their identity in the main, it might, I fancied, be interesting to ascertain whether the differences observed between the natural and artificial products were due to impurities accompanying the latter or not, for though these impurities, if present, might not cause any injury or inconvenience during the dyeing process, they might possibly be formed at the expense and take the place of alizarine, and thus be a source of loss to the manufacturer. Now a few simple experiments are sufficient to prove that artificial alizarine as ordinarily prepared is always accompanied by other substances, some of which are coloured while others are colourless or nearly so. My object on the present occasion is to describe one of these substances and to point out the relation in which it stands to alizarine.

My attention was first directed to this part of the subject by the results of some experiments made on a small scale to obtain alizarine from anthracene according to the directions of Gräbe and Liebermann. I was surprised to find that in spite of all the precautions taken I always obtained besides alizarine a notable quantity of another body also crystalline, but dissolving in alkalies with a yellow colour. This body bore so strong a resemblance in some of its properties to several of the rubiacine class of colouring matters, substances which are contained in madder along with alizarine, that my curiosity was excited. Having communicated this fact to Mr. Perkin, who, as is well known, is engaged in the manufacture of alizarine on a large scale, he kindly sent me for examination a specimen of the residue obtained by him in evaporating the mother liquors of alizarine. This residue was a crystalline, reddish-brown mass, soluble in alkalies, with a cherry-red colour. I found it to contain in addition to alizarine a quantity of a substance apparently identical with that I had previously obtained directly from anthracene. I afterwards found the same body in commercial alizarine, both in that manufactured by Mr. Perkin and in a sample from a continental firm. I therefore requested Mr. Perkin to supply me with a quantity of his alizarine sufficient to enable me to prepare a pure specimen of this body, a request to which he very kindly acceded.

This alizarine, which was a yellow, almost amorphous powder, was in the first place treated with dilute caustic soda, in which it dissolved for the most part, yielding a dark purple solution. A small qnantity of a pale yellow powder was left undissolved, which was filtered off, washed, dried, and heated, when it yielded crystals of anthraquinone. To the purple liquid there was added an excess of acid, which produced a bulky brownish-yellow precipitate. This was filtered off, and treated with boiling alcohol until the whole was dissolved. The alcohol on cooling deposited a quantity of almost pure alizarine in small mica-like scales. The mother liquor of course contained alizarine, and in order to separate it acetate of lead was added, which gave a bulky purple precipitate of the lead compound of alizarine. The filtered liquid, which had a dark yellow colour, was evaporated, when it left a yellowish-brown residue, consisting for the most part of the yellow colouring matter or acid. In order to separate the latter from the impurities accompanying it the residue was treated first with water, and then with cold alcohol. It was then dissolved in dilute caustic soda, and to the boiling solution chloride of barium was added. The filtered liquid deposited on cooling a mass of small shining crystals of the barium salt of the acid. These were purified by re-crystallisation from boiling water, and then treated with hydrochloric acid. The lemon-yellow flocks left by the acid were filtered off, washed, and dissolved in a little boiling alcohol. This on cooling deposited a quantity of yellow silky needles, consisting of the acid, which I have named *anthraflavic acid*, in order to indicate its source and its most obvious external property.

The chief properties of this acid are these:—When crystallised from alcohol and dried, it has the appearance of a dark lemon-yellow silky mass, which under the microscope is seen to consist of slender four-sided prisms. When heated on platinum foil it gives off copious yellow fumes and then burns with a luminous flame without leaving any residue. When cautiously heated in a tube or between two watch-glasses, it may be almost entirely volatilised, yielding a vapour which condenses in the form of a yellow sublimate. This sublimate consists of small lustrous crystalline plates, which, examined under the microscope, exhibit very regular outlines. The acid is only slightly soluble in boiling water, and almost insoluble in cold. It is more soluble in alcohol and ether, but insoluble in boiling benzol and sulphide of carbon. It dissolves readily in concentrated sulphuric acid even in the cold, forming a dark yellow solution, from which it is precipitated by water in yellow flocks. It is not much affected by dilute nitric acid even on boiling. With fuming nitric acid it yields a so-called nitro-acid, to which I shall return presently.

It is the fact of this substance yielding with bases compounds of well-defined character, some of them being regularly crystallised, that entitles it more especially to be classed among acids. When an alcoholic solution of anthraflavic acid is mixed with an alcoholic solution of potash, it assumes a dark yellow colour, and deposits on standing long orange-coloured needles arranged in stars and possessed of considerable lustre. The sodium compound prepared in the same manner crystallises in needles and resembles the potassium salt, but is lighter in colour. The ammonium salt may be obtained by dissolving the acid in boiling absolute alcohol and adding a slight excess of ammonia; on cooling, the solution deposits dark yellow lustrous crystals. These crystals, however, after a short exposure to the air, lose the whole of their ammonia, leaving a yellow residue consisting of the acid itself. This inability to retain ammonia even at the ordinary temperature of the atmosphere is a proof of the feeble nature of the acid. The potassium and sodium salts are also rather unstable compounds, for if it be attempted to re-crystallise either of them from boiling water a portion

* Read before the Manchester Literary and Philosophical Society, March 7th, 1871.

of the acid separates, the solubility of the base in water being sufficient to overcome its affinity for the acid. Anthraflavate of barium may be obtained by dissolving the acid in boiling baryta water, or by adding chloride of barium to a solution of the substance in caustic alkali. It is deposited from its watery solution in small shining plates, and after being filtered off and dried has a fine maroon colour. Under the microscope it is seen to consist of small crystals with very regular outlines. It may be re-crystallised from water without decomposition. The strontium salt is very similar, being soluble in boiling water and crystallising in long needles. The calcium salt is, however, insoluble in water, and is precipitated in orange-coloured flocks on the addition of chloride of calcium to an ammoniacal solution of the acid. On adding sulphate of magnesium to a solution of the acid in ammonia no precipitate is produced, but on standing some time the magnesium salt is deposited in dark yellow crystalline plates and needles arranged in star-shaped clusters and possessed of much lustre. The aluminium compound, when prepared in the same manner, appears as a yellow deposit consisting of microscopic crystals. The ammoniacal solution gives with acetate of lead a voluminous orange-coloured precipitate, with acetate of copper a light brown, and with nitrate of silver a reddish-brown precipitate. The other compounds are of no particular interest.

All the compounds of the acid which are soluble in water yield yellow solutions; none are red. It is chiefly the presence of this acid in crude alizarine which affects the colour of the alkaline solution, changing the violet due to alizarine into purple, or when present in larger quantity, into red. For the same reason an alkaline solution of crude alizarine does not show the absorption-bands in the spectrum so distinctly as one of pure alizarine. Alkaline, as well as alcoholic solutions of anthraflavic acid, absorb the blue end of the spectrum very powerfully, though no bands are visible, even with very dilute solutions. A solution of the acid, in concentrated sulphuric acid, if not too dark, shows, however, a broad but well-defined absorption-band at the extreme edge of the blue, bordering on the green, accompanied by a total absorption of the violet as seen with the other solutions.

Anthraflavic acid dissolves very readily in fuming nitric acid even in the cold, yielding a deep yellow solution, which, on standing for 24 hours, becomes lighter in colour, without evolving any gas. On now adding water a quantity of light yellow shining crystals is deposited. These, when filtered off, washed, and dried, resemble anthraflavic acid. They are, however, totally different in their properties, and consist, there can be no doubt, of a so-called nitro-acid, in which one or more atoms of hydrogen are replaced by NO_2. When heated they deflagrate, and they give a potassium salt crystallising in yellow needles, very little soluble in water, and resembling picrate of potassium. Want of material has prevented my examining this product more fully.

Though anthraflavic acid yields intensely yellow compounds with bases, it seems to possess no dyeing properties. The freshly precipitated acid suspended in water communicates not the least tinge of colour to alumina and iron mordants on calico, however long the liquid may be boiled. Its presence in artificial alizarine is therefore of no consequence as regards the dyeing qualities of the latter.

The composition of anthraflavic acid is expressed by the formula $C_{15}H_{10}O_4$. That this is the true formula was proved by an examination of the silver and barium salts. The formula of the first is $C_{15}H_8Ag_2O_4$; that of the second $C_{15}H_8BaO_4+H_2O$. The additional molecule of water attached to the barium salt is not driven off by heating to a temperature of 120° C. The acid is therefore bibasic. Hence it appears that this substance and alizarine stand in a very simple relation to one another. They are homologous bodies. Anthraflavic acid may be viewed as alizarine in which an atom of hydrogen is replaced by methyl. Though the great difference in properties, and especially the far greater stability of the acid, might lead to the inference that it is only as regards their composition that the two substances approach one another, a very simple experiment is sufficient to prove that they are in fact very closely related. If pure anthraflavic acid be dissolved in an excess of caustic potash, and the solution be boiled down to dryness, a yellow residue is left, which, after being carefully heated almost to fusion, dissolves in water with a red colour. This solution contains alizarine, as it shows the absorption-bands in the spectrum peculiar to the latter, though not very clearly on account of undecomposed anthraflavic acid still present. Pure alizarine may, however, be obtained from it, by simply adding an excess of acid, filtering off the flocculent precipitate, dissolving the latter in alcohol, and adding to the solution acetate of lead, when a purple precipitate falls, which contains the whole of the alizarine, the excess of anthraflavic acid remaining in solution. From the lead precipitate alizarine may be obtained having all the properties of that substance. It is certain, therefore, that by the action of caustic potash, anthraflavic acid is converted into alizarine, the process being doubtless one of oxidation, though it should be stated that the conversion is never complete, probably because the action, if carried far enough to convert the whole of the acid, leads to the decomposition of the alizarine already formed. I am at present occupied with some experiments for the purpose of substituting an atom of hydrogen in alizarine by methyl, and thus forming anthraflavic acid synthetically. It is evident that the acid cannot be considered as a methylic ether of alizarine, since both substances combine with two atoms of base to form neutral compounds. If the substitution by methyl be possible, it must therefore take place in the radical of alizarine. The possibility of such substitutions is allowed by Gräbe and Liebermann, who consider chrysammic acid, for instance, as anthraquinone in which 4H are replaced by $4NO_2$. Should the synthesis just mentioned succeed, it will, I imagine, throw some light on the constitution of the so-called yellow colouring matters of madder, such as rubiacine and rubiadine, which certainly contain 16 atoms of carbon, and may possibly turn out to be substitution products of alizarine.

In what manner anthraflavic acid, with its 15 atoms of carbon, is formed from anthracene, which contains only 14, is not very clear. I imagined it to be just possible that the anthracene employed for preparing the alizarine supplied to me might have contained a higher hydrocarbon, say $C_{15}H_{12}$ or methylanthracene, which, by oxidation, would yield methylanthraquinone, and at the end of the process methylalizarine. On requesting Mr. Perkin to favour me with his opinion on this point, he informed me, however, that my supposition was improbable, because the alizarine which he sent me was prepared from nearly perfectly pure anthraquinone, which had been distilled and crystallised from benzol.

Another point remains to be considered in connection with this subject. It is well known that the beautiful discovery of the mode of forming alizarine was the direct result of a previous one, viz., that of the reduction of the natural product by means of metallic zinc to anthracene. The question therefore naturally suggested itself. What is the nature of the hydrocarbon formed by the same process fron anthraflavic acid? Is it anthracene or something else? In order to decide this question I took a quantity of the acid and heated it with 50 times its weight of zinc powder, in the manner described by Gräbe and Liebermann. I obtained a quantity of a brownish crystalline sublimate, amounting to about 10 per cent of the acid employed, which was purified by sublimation and washing with ether. It still retained the yellowish tinge which, according to Gräbe and Liebermann, adheres so pertinaciously to anthracene, but it did not differ in other respects from the pure substance. It melted at the same temperature as anthracene, and began to sublime before

fusing; it dissolved in boiling alcohol, but more readily in benzole, and was deposited from these solutions in lustrous crystals of a very regular form; and it gave, like anthracene with picric acid, a compound crystallising in long red needles. I wish to speak with some reserve on this point, as the quantity of material at my disposal was not sufficient for an analysis, but should it turn out that my product is identical with anthracene, this fact would throw doubt on some of the reasoning of Gräbe and Liebermann, who assume that if an organic substance yields a definite hydrocarbon by the action of metallic zinc, the latter contains the same number of atoms of carbon as the original substance.

ON THE

CHANGE EXPERIENCED BY METALLIC IRON AND BY IRON OXIDES WHICH HAVE SERVED TO DISSOCIATE CO.*

By I. LOWTHIAN BELL.

(Continued from p. 147).

Expt. 347.—After 4½ hours' exposure to a current of CO at a temperature of melting zinc, about 420° C. (788° F.), its composition was:—

Fe	49·83
Oxygen by difference	0·07
Earths	48·67
Carbon	1·43 = 2·87 per 100 Fe.
	100·00

Expt. 348.—Three hours' exposure to CO at a red heat gave—

Fe	49·88
Oxygen by difference	0·20
Earths	48·68
Carbon	1·24 = 2·48 per 100 Fe.
	100·00

In experiments 245, *et seq.*, it was shown that when the proportion of CO_2 exceeded 50 volumes to 100 of CO, all carbon deposition was suspended when oxide of iron, in various ores, was exposed at different temperatures to such a mixture. The same law holds good when the iron contained in such ores has been previously reduced to the metallic condition by hydrogen, oxidation of the Fe taking place. This circumstance appears to prevent carbon impregnation.

Subject to all the precautions of excluding air, and avoiding other circumstances which could interfere with the results, the following trials were made with perfectly reduced Cleveland ore:—

	Mixture of gas. CO. vols.	CO_2. vols.	Time of exposure. hr.	Temp.	Carbon deposited.	Oxygen absorbed per 100 of Fe.
Expt. 349.	100	616	¾	melting zinc	*nil*	18·46
,, 350.	,,	98	½	,,	,,	—
,, 351.	,,	,,	½	bright red	,,	5·91
,, 352.	,,	61	13½	,,	,,	3·48
,, 353.	,,	57	7	,,	,,	5·14
,, 354.	,,	38	½	melting zinc	,,	1·31

The deposition of carbon from CO, as it is effected by metallic iron is, it has been proved, always accompanied by the simultaneous oxidation of a minute portion of the Fe; it remains to be considered, whether, when the metal is taken in its oxidised state, any carbon impregnation takes place before a minute quantity of iron is brought down to the condition of Fe, a state of things deemed indispensable, I believe, by previous writers. It is obvious, that if a mere absorption of O is required, a lower state of oxidation, passing into one of a higher description, may suffice, and thus iron, in its metallic form, may not be required.

To ascertain whether there was any metallic iron present, the iodine test, examined in experiment 51, *et seq.*, was employed, in which, it will be remembered that, although it was impracticable to distinguish whether iron, as hydrated FeO or Fe, is present in the substance in question, it was easy to say whether the metal in neither of these conditions existed; moreover, anhydrous protoxide was almost unaffected by iodine and cold water. Under these circumstances, then, we may speak confidently, that when iodine and cold water dissolves no iron, neither hydrous FeO nor Fe are present, and the anhydrous protoxide, if present, must be so in very small quantities:—

The following trials illustrate the change experienced by different oxides of iron, exposed simultaneously for seven hours in the lead bath, to the action of CO, zinc melting, but antimony not:—

Substance Used.	*Expt.* 355. Fe_2O_3 from $FeSO_4$.	*Expt.* 356. Precipitated Fe_2O_3.	*Expt.* 357. FeO from Nitrate.	*Expt.* 358. Pumice and Fe_2O_3.	*Expt.* 359 Calcd. Clev. ore.
Carbon	31·9	45·5	56·3	1·69	0·11
Fe, soluble in iodine ..	11·4	2·5	1·4	*nil.*	*nil.*
Fe, insoluble in iodine ..	47·1	45·2	37·7	11·34	40·60
O by difference	9·6	6·8	4·6	3·71	15·75
Earthy matter	—	—	—	83·26	43·54
	100	100	100	100	100

Calculated upon 100 parts of metallic iron present, the above numbers yield:—

	Fe_2O_3 from Fe_2SO_4.	Precipitated Fe_2O_3.	Fe_2O_3 from Nitrate.	Fe_2O_3 Pumice stone.	Calcd. Clev. ore.
Fe soluble in iodine ..	19·5	5·3	3·7	—	—
Carbon	54·5	95·4	144·0	14·9	0·3
Oxygen	16·4	14·3	11·7	32·7	38·8
Per cent original O lost	61·7	66·6	72·4	23·6	3·2

These numbers show how materially the molecular state affects deoxidation and carbon deposition, and, at the same time, they prove that the extent of the latter is firstly independent of the presence of any metallic iron (*vide* experiment 358), and, secondly, that it bears, as usual, no ratio to the extent to which the oxygen has been removed, although in each of these cases where the loss of oxygen has been the largest, the carbon deposited is the highest.

The view of the deposition of carbon being effected without the necessary formation of any metallic iron, was also borne out by the following experiment:—

Expt. 360.—Well calcined Cleveland ironstone was exposed for 3½ hours to the action of CO, freed from any traces of free oxygen, and from carbonic acid by passing through strong alkaline pyrogallate solutions. The temperature was close on that of melting zinc, and the CO_2 produced, and C deposited, were estimated.

The ore lost .. .. .. 2·55 per cent of its weight.
CO_2 collected, equal to .. 13·00 of weight of ore.

Residual carbon, estimated by solution in HCl, collected in asbestos filter, and combustion with lead chromate = 0·90 per cent, or 2·3 per 100 of metallic iron present:

Hence, loss of weight	= 2·55 per cent.
Carbon deposited	= 0·90 ,,
Oxygen removed	= 3·45 ,,

* From the "Chemical Phenomena of Iron Smelting."

Again:—

Oxgyen, corresponding to 13·0 CO_2 collected	= 4·72
Ditto associated with 0·9 C deposited	= 1·20
Difference = oxygen removed ..	= 3·52

The agreement between these determinations shows the experiment to be trustworthy. On solution of the residual substance in HCl, and titration with permanganate, a quantity of ferrous compound was found equivalent to 23·2 per cent of Fe, or 29·8 of FeO.

If the reduction had been from Fe_2O_3 to 2FeO, this amount of protoxide would have generated, by the removal of—

$$\frac{16}{144} \times 29{\cdot}8 = 3{\cdot}31 \text{ per cent of oxygen,}$$

whilst, had metallic iron been formed to this extent, an amount of oxygen must have been removed equal to

$$\frac{3 \times 16}{144} \times 29{\cdot}8, \text{ or } 9{\cdot}93 \text{ per cent of O.}$$

The actual amount removed being close upon the former number, it appears that no appreciable quantity of metallic iron could have been produced.

In no instance, let it be remembered, was it found practicable to remove the whole of the oxygen contained in the iron oxide employed by pure CO at temperatures short of a dull red heat, after 12 or 14 hours' exposure, even when the carbon greatly exceeded the weight of iron present. The curious fact, however, was noticed, that when the greater part of the oxygen was removed, the amount of iron insoluble in iodine and cold water, after some hours' digestion, was considerably more than that corresponding to the formula FeO, thus apparently indicating the formation of some sub-oxide, in the way already described. The three varieties of pure Fe_2O_3 previously employed yielded the following results, the temperature being about that of melting zinc, or a little higher:—

	Expt. 361.	*Expt.* 362.	*Expt.* 363.	*Expt.* 364.	*Expt.* 365.	*Expt.* 366.	*Expt.* 367.
Substance used:—	Precipitated Fe_2O_3. anhydrous.			Fe_2O_3 from $FeSO_4$.		Fe_2O_3 from nitrate.	
Time of exposure,	7½ hrs.	7 hrs.	10 hrs.	15 hrs.	7 hrs.	7 hrs.	7½ hrs.
Iron insol. in I. ..	33·1	45·2	51·20	28·0	47·1	37·7	45·9
Do. sol. ,, ..	*nil.*	2·5	0·10	0·6	11·4	1·4	*nil.*
Carbon	58·7	45·5	35·85	64·4	31·9	56·3	46·1
Oxygen	8·2	6·8	12·85	7·0	9·6	4·6	8·0
	100·0	100·0	100·00	100·0	100·0	100·0	100·0
Oxygen remaining with 100 parts iron insoluble in iodine	24·8	15·0	25·1	25·0	20·4	12·2	17·4
Carbon per 100 of Fe present..	177·3	95·3	69·5	225·1	54·5	144·0	100·4

Theoretical oxygen for 100 of iron being for Fe_2O.. 14·3
FeO.. 28·6
Fe_2O_3.. 42·8

Similar results were obtained on exposing various ores of iron to the action of CO in the lead bath apparatus. In all cases, the ores were previously heated in a muffle, to secure complete dryness and peroxidation.

	Expt. 368.	*Expt.* 369.
Substance used—	Clev. calcd. ore.	Hæmatite Lancashire.
Time exposed—	7 hours.	7½ hours.
Temperature—	Melting zinc in both cases.	
Composition after exposure, as determined by analysis .. .. Fe sol. in I,	*nil.*	0·20
Fe insol. ,,	40·60	33·35
Oxygen ..	15·75	9·05
Carbon ..	0·11	22·10
Earths ..	43·54	35·30
	100·00	100·00
Oxygen remaining with— 100 parts Fe insoluble in I	38·8	27·1

ALTERATIONS IN PHARMACOPŒIAL NOMENCLATURE NECESSITATED BY THE ADVANCEMENT OF CHEMISTRY.*

By Professor ATTFIELD.

In speaking of the present position of the chemical names of the *Pharmacopœia*, the author said — The system of nomenclature hitherto accepted from chemists by pharmacists, practitioners in medicine, and the public, that which is employed in European and American Pharmacopœias, was mainly devised by Lavoisier, eighty-four years ago. The fundamental principle on which it was founded was, that the name of a salt should express its composition. The many animal and vegetable substances discovered since that time (notably alkaloids and neutral crystalline principles) are designated, perhaps fortunately, by unsystematic names, names which, at all events, are not liable to change. The great majority of chemical substances employed in pharmacy are such mineral salts as were known to Lavoisier, and their names were given by him on the assumption that they contained, on the one hand, an undecomposable body, generally a metal, common to a great many such salts (the compounds of copper, for example), and, on the other, a body, or a group of elements, also common to a number of salts (sulphates, for example). Soda, potash, lime, baryta, magnesia, and alumina were then considered to be elements; hence, as I shall further show presently, such names as carbonate of soda, nitrate of potash, and sulphate of baryta were perfectly consistent with those of carbonate of iron, nitrate of mercury, sulphate of copper. During the twenty years succeeding 1787, Lavoisier's views of the constitution of salts and the language or nomenclature in which those views found expression, were generally accepted throughout Europe; up to 1807 no necessity arose for interfering with his nomenclature; but in that and the following year Davy made his brilliant researches on the alkalies and alkaline earths, discovered that potash, soda, baryta, strontia, and lime were not elements, as had been previously supposed, but that the true basylous radicals of the so-called compound of potash, soda, baryta, strontia, and lime were metals—to which were given the names potassium, sodium, barium, strontium, and calcium. Thenceforward the old names potash, soda, baryta, strontia, lime, were used to designate the oxides of the new metals. Then there at once arose a dilemma in regard to nomenclature. The names of all the salts of Davy's metals were no longer consistent with the names of the salts of all other metals. Davy, supported afterwards by Dulong, Clark, Graham, Liebig, and Daniell, suggested that *all* metallic salts were composed of metal alone on the basylous side, and a distinct radical on the acidulous side. Unfortunately, however, accurate knowledge of constitution was not included in this idea; even definite names being proposed for the said acidulous radicals. Thus blue vitriol was termed oxysulphionide of copper (Daniell), sulphatoxide of copper (Graham), and sulphanide of copper (Otto). Many other objections to the theory arose, and hence salts came to be regarded as compounds of oxides of metals with certain acidulous radicals (now known as anhydrides). But the followers of applied chemistry never took kindly to the nomenclature.

Hence, when a very few years ago chemists were led by irresistible arguments and stubborn facts to double many of the old atomic weights, an opportunity of abandoning the old constitutional theories then presented itself, and was by common consent accepted. The exertions of Dumas, Laurent, and Gerhardt bore fruit. The dualistic idea of salts being formed of an acidulous radical with the oxide of a metal, and the not less binary notion of their being composed of a distinct acidulous radical united with a metal, were abandoned, and hypothesis altogether

* Abstract of a paper read before the Pharmaceutical Society, Apri 5th, 1871.

rejected, or, at all events restricted to the idea of *oneness*. These views were, of course, accompanied by a commensurate alteration in chemical notation and nomenclature. Blue vitriol no longer being considered to be the sulphate of the oxide of copper, as shown in the formula CuO,SO_3, nor even to have the binary constitution implied in the formula Cu,SO_4, but to be a structure *per se*, or at least, one whose detail of constitution was unknown,—it became necessary to devise for it and all such salts a notation and nomenclature which should be consistent with the unitary idea. Strictly speaking, this was impossible. The relationship—nay, the absolute identity—of the constituent radicals in whole classes of salts demanded fair representation in notation and nomenclature, a result fatal to pure unitary ideas. Thus, the unquestioned relationship of the cupreous compounds to each other demanded the employment of the word "copper" in their names and the symbol Cu in their formulæ; while the unquestioned relationship of salts containing the elements which occur in the non-cupreous portion of blue vitrol demanded the employment of the word "sulphate" in their names and the symbols SO_4 in their formulæ, and with the employment of such names and such formulæ the binary idea is difficult to repress. At the same time all are agreed that the unqualified assumption of knowledge of chemical constitution involved in the old binary theories is wrong; hence professedly binary systems of notation and nomenclature must be abandoned; the names sulphate of oxide of copper, with its formula CuO,SO_3, and sulphate of oxide of magnesium (or sulphate of magnesia), with its formula MgO,SO_3, must be given up for sulphate of copper, $CuSO_4$ (or copper sulphate or cupric sulphate), and sulphate of magnesium, $MgSO_4$ (or magnesium sulphate). Such names and formulæ sufficiently exhibit unquestioned relationships, while they include the least possible amount of theory.

Chemical Notation.—All teachers of chemistry, including the authors of nearly every modern manual, with remarkable unanimity have relinquished the old system of notation, and have, to a greater or less extent, adopted the new. It is to be expected that the next British Pharmacopœia will employthe usual chemical symbols as expressive of the new atomic weights (O=16) to the exclusion of the old (O=8), and will altogether discard the hypothesis of the constitution of salts involved in such formulæ as KO,NO_5, or (accepting the new atomic weights) K_2O,N_2O_5, using only the less theoretical formulæ (*e.g.*, KNO_3) which are now employed by the majority of teachers.

The old system is given up by chemists; the new is already officially recognised by the Council, under whose authority the Pharmacopœia is issued, and by the various examining Boards, and is adopted in educational works on chemistry.

These are sufficient reasons for justifying us in the expectation of seeing the new notation, if any, alone employed in the third British Pharmacopœia. Notation and nomenclature should obviously harmonise, seeing that they are simply different methods of expressing the thoughts and wants of everybody respecting chemical substances. Formulæ are more comprehensive than names, and convey to the mind far more information, but they are intelligible only to the educated chemist. Names comprise less knowledge, but are more or less understood by everybody and suffice for general purposes. To formulæ, however, we look to ascertain the views of chemists concerning the constitution of chemical compounds, and it is on these views that nomenclature is founded.

Properties of Names.—The names of pharmacopœial chemicals should fulfil certain functions or possess definite qualities, positive or negative, namely,—

1. The name should, as far as possible and practicable, indicate composition. This Lavoisierian principle is, as I have already shown, one of necessity as well as expediency.

2. One name should be associated with only one substance; but the converse I would by no means urge, namely, that one substance should be known by only one name, synonyms being useful both from a theoretical and a practical point of view.

3. A name, even if fallen out of use, should not be transferred to a substance having properties different from the original substance.

4. The names of an official chemical substance, that is, a name officially recognised in national pharmacopœias, should possess the minimum of instability.

The free employment of Latin and Greek numerals in a chemical name, though highly useful in general chemical literature for indicating details of a composition, is too dependent on hypothesis respecting atomic values and weights, and too susceptible of disturbance caused by new discoveries, to possess the element of permanence; hence they must be avoided in pharmaceutical chemistry.

5. A pharmacopœial name should admit of being either easily spoken or written, both in the full and in the contracted form.

6. When close resemblance between two salts is indicated by identity in all but one of the syllables of their names, that syllable should be at the commencement of the names and not at the end, where it would be liable to be omitted. So green iodide of mercury and red iodide of mercury are better than mercurous iodide and mercuric iodide, or green sulphate of iron and persulphate of iron to ferrous sulphate and ferric sulphate; any greater precision that may be desired being given by chemical formulæ.

7. A name should not be changed for mere purpose of euphony, real or fancied; thus, chlorhydric for hydrochloric.

8. Names of pharmacopœial chemicals should be consistent with each other.

9. The chemical names employed in pharmacy should be consistent with those used in other branches of applied chemistry, and with the language of scientific chemistry and general chemical literature.

The Proposed Names.—The advantages claimed for the proposed names are that they are more consistent with each other than the old; they are formed on one uniform system instead of two; they include less of theory, and therefore have greater elements of stability than the old; and they are harmonious, whilst the old is absolutely inconsistent with both modern scientific nomenclature and the only chemical notation now employed. Their newness, so far as they are new, is their only disadvantage, and even this disadvantage is, in practice, reduced to insignificant proportions.

Resumé.—The chief alterations in pharmacopœial nomenclature now proposed amount to this, that the compounds of the alkali-metals and alkaline-earth-metals instead of being named as hitherto on two distinct systems, should follow but one; that instead of salts of potassium and of potash we should have salts of potassium only; and instead of sodium and soda compounds, sodium only; and so with preparations of ammonium, lithium, calcium, magnesium, and aluminium. This is a step in the direction of simplicity and permanency, and away from that of theory.

Synonyms.—Modern scientific chemical names and the old dualistic names should, I think, be included as synonyms of the leading name in all Pharmacopœias.

Exceptional Alterations.—Constitutional objections to the name *arsenicum acidum* would be obviated by the old name *arsenicum album*. In view of the peculiar composition of bichromate of potassium, the first word of its name is most unsuitable, and would be advantageously replaced by red chromate, a name which would usefully distinguish the salt from yellow chromate of potassium. The names of the bismuth powders are not at present consistent with each other; if the one be termed subnitrate the other should be subcarbonate, not "carbonate." But these preparations and the similar compounds of copper and lead are normal rather than "sub" salts, containing

oxygen in the place of an exactly equivalent quantity of the acidulous radical of the neutral salts, and might well be termed respectively oxycarbonate of bismuth, oxynitrate of bismuth, oxyacetate of copper, oxyacetate of lead; at all events the latter names would do good service as synonyms. Similar remarks apply to the peroxhydrates of iron. The prefix "sub" is most usefully, and, indeed, indispensably, applied in the case of calomel: it would be well if the meaning of the syllable could be always thus restricted to its etymological signification, and never again used in its old conventional sense. The names tartarated antimony, tartarated iron, tartarated sodium, I do not like at all. The sister terms sulphurated antimony and sulphurated potash are most happy, their utter vagueness fairly representing the nondescript character of the preparations. But tartrate of antimony and potassium, tartrate of iron and potassium, and tartrate of sodium and potassium, are at least as definite in composition as the citric trio which are properly honoured with the definite names citrate of bismuth and ammonium, citrate of iron and ammonium, and citrate of iron and quinia (or, rather, with the old forms of those names).

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Anniversary Meeting, March 30th, 1871.

Professor WILLIAMSON, F.R.S., President, in the Chair.

THE PRESIDENT delivered the following address:—

Gentlemen,—I feel much pleasure in congratulating you on the rapidly increasing prosperity of our Society, and the enlargement which has taken place in its sphere of usefulness; for, on the one hand, the number of our Fellows continues to show a most satisfactory increase; and, on the other hand, your Council has made arrangements for carrying out the system of monthly reports, which has been for some time in contemplation. It was hoped that the Chemical Society of Paris might, from the first, co-operate with us in the preparation of these monthly reports, but circumstances beyond their control have prevented the sister Society from joining us in the beginning of this year. Deeming it undesirable to delay the commencement of the reports, your Council still look forward to the future co-operation of the Paris Society in their preparation.

You are aware that the present available income of the Society was not considered to be sufficient to defray the additional expense of writing and printing these reports; and I have the pleasure of informing you that contributions to the extent of £1175 have been promised by members of your body towards supplying the deficit during the first five years of the appearance of the reports. The British Association has, moreover, granted us the sum of £100 for this year in aid of the undertaking. We hope that in five years the funds of the Society may have sufficiently increased to enable us to pay the whole expense of the reports, and that their publication will be valued by the Members of our Society, and promote the advancement of our science wherever the English language is read. The next number of our journal, which I hope to see in a few days, will be the first to contain the monthly reports in addition to the original papers contributed to the Society.

The following papers have been read at our meetings since the last anniversary:—

"Analysis of Deep Sea Water." By John Hunter.
"On the Refraction Equivalents of the Aromatic Hydrocarbons and their Derivatives." By J. H. Gladstone.
"Note on Bromopicrin." By T. Bolas and C. E. Groves.
"On an Acid Feed-Water from the Coal-Field at Stellarton, N.S., and the Results of its Use." By Professor How.
"On Tetrabromide of Carbon." By T. Bolas and C. E. Groves.
"Chemical Researches on New or Rare Cornish Minerals." By A. H. Church.
"On the Combinations of Carbonic Anhydride with Ammonia and Water." By E. Divers.
"On a New Gas Furnace for Chemical Operations at a White Heat without the aid of a Blowing Machine." By C. Griffin.
"On Vapour Densities." By James T. Brown.
"Researches on Vanadium." Part II. By H. E. Roscoe.
"On the Precipitation of the Solutions of Ammonium Carbonate, Sodium Carbonate, by Calcium Chloride." By E. Divers.
"On the Manipulation of Assays of Gold and Silver Bullion." By C. Tookey.
"On some New Bromine Derivatives of Coumarin." By W. H. Perkin.
"On Organic Matter in Water." By C. Heisch.
"On the Methods for the Determination of Carbon in Steel." By W. D. Herman.
"On Fungi and Fermentation." By James Bell.
"On Acetic and Formic Acid obtained from Human Urine during the Chemical Decomposition of Urochrome." By J. L. W. Thudichum.
"On the Production of the Sulphates of the Alcohol Radicals from the Nitrites by the Action of Sulphurous Acid." By E. T. Chapman.
"On the Composition of the Hyposulphites." By E. A. Letts.
"Mineralogical Notices." By N. Story-Maskelyne and Walter Flight.
"On some Derivatives of Anthracene." By W. H. Perkin.
"On some New Derivatives of Coumarin." By W. H. Perkin.
"On the Action of Sulphuric Acid on the Natural Alkaloids." By Henry E. Armstrong.
"On an Alkaloid from Cinchona Bark, hitherto undescribed." By David Howard.
"On the Origin of Nitrates in Potable Waters." By Charles Elkin, Bath.
"On the Development of Fungi in Potable Waters." By E. Frankland.
"On the Effects of Pressure on the Absorption of Gases by Charcoal." By John Hunter.
"On the Solubility of Bone-Ash in Carbonic Water." By Robert Warington.
"On the Distillation and Boiling-Point of Glycerine." By Thomas Bolas.

The following lectures have also been delivered:—

"On Vanadium." By Dr. Roscoe.
"On the Platinum Ammonias." By Dr. Odling.
"On the Distillation of Wood." By Mr. Chapman.

At the last anniversary meeting, we numbered 551 ordinary Members and 36 foreign Members, and six of the former have withdrawn from the Society. On the other hand, 42 new Members have been elected into the Society. We have lost five ordinary Members by death—viz., Mr. George Jolley, Dr. W. A. Miller, Dr. Augustus Matthiessen, Dr. J. S. Muspratt, and Mr. W. W. Routh; and it is also my painful duty to record the death of two of our foreign Members—viz., Professor Gustav Magnus and Professor Weltzien.

William Allen Miller was born at Ipswich, in Suffolk, on the 17th of December, 1817. His early education was conducted by his mother, a lady of remarkable sagacity, good sense, and religious feeling. Those who had the privilege of her friendship could trace in her son many points of mind and manner in which he closely resembled her. Miller spent one year at Merchant Taylor's School, London, and two years at a school at Ackworth, in Yorkshire, belonging to the Society of Friends. Lectures on

elementary chemistry were given at this school, and hence he seems to have imbibed his taste for a science which was to form the business of his life. He also took great pleasure in studying the stars through a telescope which one of the masters used to exhibit to the pupils. The influence of these early studies may be traced through his life in his devotion to chemistry, and in his application of his knowledge to the chemistry of the stars. His mental training at Ackworth was valuable, and his manner became still more marked by that quiet simplicity which was a characteristic of his mother, as it is also of the members of the Society of Friends.

At the age of fifteen he was apprenticed to his uncle, who was surgeon to the General Hospital at Birmingham. Five years later, he became a student in the medical department of King's College, London. Here his chemical knowledge attracted the notice of Professor Daniell, who availed himself of his assistance in the laboratory during the illness of the appointed assistant. This was the turning-point in Miller's career, and gave a decided bias to his studies. In 1839, he gained the Warneford prize, for the encouragement of theological studies among medical students. In 1840, he visited Germany, and passed some time in Liebig's laboratory at Giessen. In the same year he was appointed to the post of Demonstrator in the laboratory of King's College. In 1841 he became Assistant Lecturer to Professor Daniell, and also took his degree of M.D. in the University of London. He likewise aided in various scientific inquiries, and conducted the experiments on the electrolysis of saline compounds, which Daniell was then carrying on. Their joint names appear in a paper on this subject published in the *Philosophical Transactions* for 1844.

In 1845, Miller was elected a Fellow of the Royal Society, and in the same year he was appointed to the chair of Chemistry in King's College, which had become vacant by the sudden death of Professor Daniell. In the same year, he laid before the British Association an account of some experiments on the action of certain gases on the solar spectrum. This paper appeared in the *Philosophical Magazine* (3rd series, vol. xxvii., p. 81.)

For some years after his appointment, Miller continued to use the work of his predecessor as a text-book for the chemical classes at King's College. His own work on chemistry was undertaken after considerable hesitation lest he should injure the reputation of his master, Professor Daniel. "You are not aware," he said to the writer of this notice, "how much of Daniell I have in my notes." For some time, his idea was to revise Daniell's "Introduction to the Study of Chemical Philosophy," and to make such additions as the progress of science required, and to maintain it in its old position as the text-book of the chemical classes. But, on looking over the work with this view, he found that so many additions and alterations would be required as greatly to supersede the author's peculiar touches. He therefore decided to produce a new work, and to leave "untouched that of his late master as the true exponent of his views upon some of those branches of science which his researches had contributed to advance and adorn." These words are from the preface to the first edition of Miller's "Chemical Physics," dated March, 1855. This volume, as well as the two subsequent ones on "Inorganic" and "Organic Chemistry," which appeared, the one in 1856, and the other in 1857, were written from Miller's lecture-notes, so that when the books were introduced, the students of King's College, far from having to conform to any new method, seemed to recognise in the new text-books the very lectures they had heard. The three volumes of Miller's "Elements of Chemistry," passed through several editions in the course of a few years. Being successful, they were reprinted, as a matter of course, in the United States of America, but we are informed that for the later editions some arrangement was made with an American publisher, whereby the author and his publisher derived some benefit from the sale of the work in the United States. In these later editions, Miller adopted the new method. His conservative principles led him to resist this change as long as it was possible to do so; but having been, during so many years, accustomed to the old notation, he never took kindly to the new. Indeed, it was part of Miller's character to grasp a new idea with a certain amount of mental slowness; but when once fairly appreciated, it was held tenaciously, and not given up without a severe struggle.

Miller was an excellent chemist, thorough in his work, and thorough in laboratory practice and teaching. His views, without being remarkable for originality, always fairly represented the science of the day, and were expressed with great clearness, in simple language. As a lecturer, he was more successful in style and expression than as a writer. With the pen in his hand, his style often became involved, and not fully expressive of his meaning. Feeling this, he was accustomed to submit his writings to an intimate friend on whose judgment and long experience in such matters he could rely.

Miller took a great interest in the progress of spectrum analysis, and in 1861 lectured on that subject at one of the evening meetings of the British Association for the advancement of science, at Manchester. It was expected that this lecture would consist, for the most part, of an account of Kirchhoff's researches; but, to the surprise of many, a large diagram was exhibited, arranged something like a genealogical tree, containing the names of the various discoverers and the points discovered up to the date of Kirchhoff's researches. This lecture was illustrated by a number of experiments, at that time as remarkable for their novelty as for their brilliancy. Few persons who heard that striking lecture delivered in that crowded music-hall were aware that the lecturer had been confined during the day by a severe bilious attack to his bed, from which he arose to deliver the lecture, and to which he returned immediately after its close.

At this meeting, he presided over the chemical section, and read a short paper on "Photographic Spectra of the Electric Light." The full account of his researches on this subject, based upon a very large number of experiments, was laid before the Royal Society, and is published in the *Transactions* for 1862, p. 861. These researches seem hardly to have received the attention they deserve.

"On the 15th of January, 1862, Miller repeated his lecture on "Spectrum Analysis" before the Pharmaceutical Society of London. This lecture was attended by his friend and neighbour, Mr. Huggins; and while returning home together Mr. Huggins proposed that Miller should join him in some experiments on the spectra of the heavenly bodies. The results of this joint inquiry were given to the Royal Society, and were rewarded by gold medals from the Astronomical Society. Miller also gave a course of four lectures "On the Spectra of the Heavenly Bodies" at the Royal Institution in Albemarle Street, in 1867.

He likewise delivered a lecture on the same subject to the working men of Exeter, at the meeting of the British Association in that city in 1869.

During many years Dr. Miller was one of the Assayers of the Mint and of the Bank of England. He also paid great attention to the subject of water analysis. It will be remembered by many present that in 1865 he gave us a lecture "On Some Points in the Analysis of Potable Waters." His latest work was the preparation of a thermometer adapted to deep sea soundings.

We all remember the tone of melancholy that was given to the last meeting of the British Association by the sudden illness of our friend, which soon terminated in his death. Although removed at the early age of fifty-three, he had passed a life which, measured by its results, cannot be called a short one. If we were asked to name, in one word, the most marked feature in Miller's character, we should say it was *sagacity*. Well acquainted with the scientific tendencies of the day, cautious in forming as well as expressing opinions, just in his dealings with others—

his advice and assistance were sought by persons in various stations, from a cabinet minister down to a humble inventor and patentee. His services were of great value to the Royal Society. He frequently served on the Council and on various Committees appointed by the society in its own interests and those of science, if the two can be separated. He became Treasurer of the Royal Society in 1861, and, as such, Vice-President. He was one of the original founders of the Chemical Society, and has frequently occupied this Chair as well as a place at our Council Board. He has, on several occasions, contributed to our Journal, but he was not often heard in debate. Indeed, he had a dislike to impromptu utterances from a feeling that, in the heat of debate, rash things are often said, and the cause of truth and fair dealing endangered. The caution of his character is here again conspicuous, together with his love of truth. These were based on a deep religious belief which formed the mainspring of this whole life.

Professor Miller received the degree of M.D. at the University of Edinburgh ; that of D.C.L. at the University of Oxford in June, 1868 ; and that of LL.D, at the University of Cambridge in May, 1869, after giving the Reade Lecture. He was buried on the 5th of October last, in Norwood Cemetery, by the side of his wife, whom he survived one year.

I have to observe that the preceeding lines are from Mr. Tomlinson's pen.

Augustus Matthiessen, born January 2nd, 1831, was the son of a London merchant. While a child of two or three years old he had a paralytic seizure, from which he never entirely recovered. As a boy, his fondness for chemical experiments was strongly marked, and his wish was immediately on leaving school to devote himself to the study of chemistry ; this, however, he was unable to do. While very young he lost both his parents, and his guardians did not consider themselves justified in sanctioning his devoting himself to physical science, or to his becoming a merchant, believing that the paralytic affection from which he suffered totally incapacitated him for both pursuits, and, believing that the only occupation open to him was farming, they sent him on leaving school to serve an apprenticeship to a large farmer in Dorsetshire. Although thus frustrated in his desire at once of devoting himself to the study of chemistry, it in no way altered his determination. During the three years which he passed at the farm, he devoted much of the time and money at his disposal to chemical experiments and, immediately on coming of age, he quitted the farm and went to Giessen as being the school where he could best carry out his desire of studying chemistry. Under the direction of Professor Will and of Professor Buff, he obtained a thorough knowledge of the elements of chemistry and physics, and, during his stay in Giessen, he took his degree of Doctor of Philosophy. From Giessen he went to Heidelberg, and, under Bunsen's direction, at once began the preparation of the metals of the alkalies and alkaline earths, and the study of their physical properties. Matthiessen's first paper was published in our journal for 1855.

He was the first to obtain the metals strontium and calcium, decomposing the chlorides by an electric current, and, further, he clearly showed that both potassium and sodium were incapable of effecting this decomposition. In the laboratory of Professor Kirchhoff, he carried on experiments on the physical properties of the metals, and, in 1857, published an account of his experiments on the electrical conductivity of sodium, magnesium, calcium, potassium, lithium, and strontium. In the following year he extended this series of researches, estimating the electrical conductivity of many other metals, metalloids, and some alloys. In 1857, Matthiessen returned to England, and, for a short time worked at the College of Chemistry, then under the direction of Dr. Hofmann. While there, he published a short account of the action of nitrous acid on aniline. He afterwards had a laboratory at his residence in Torrington Place, and worked there most assiduously for about four years. During this time he published several papers on the specific gravity of different metals and alloys, and also on the electrical conductivity of some alloys and metals, specially gold and copper. In conjunction with Professor G. C. Foster, he published his first paper on the organic alkaloids, establishing the composition of narcotine, and gaining considerable insight into its rational constitution. In the spring of 1861, he was elected a fellow of the Royal Society, and, in the following year became Lecturer on Chemistry at St. Mary's Hospital Medical School, a post which he held for six years. He was afterwards chosen Lecturer on Chemistry, at first in conjunction with Dr. Odling, at St. Bartholomew's Hospital, and afterwards, on Dr. Odling removing to the Royal Institution, Matthiessen became the sole lecturer on chemistry. While holding these chairs of chemistry, his interest in the science never flagged, and his indomitable love of work enabled him to accomplish much. His later published papers relate entirely to the organic bases—narcotine, morphia, codeia—they all yielded to him most interesting and important results, and he seemed on the eve of still more important discoveries. At St. Bartholomew's, a new laboratory had been built under his supervision, and he looked forward with pleasure to the increased facilities it would give him for carrying on his scientific reseaches ; but this was not to be. The laboratory had been completed but a few days before his death. Interested in his work to the last, planning new researches, completing old ones, unfailing in his official duties towards his large class of students, carrying on a large consulting practice—all was managed with precision and regularity to the last. Under the strain which, without relaxation, had been put upon his brain for years, it apparently gave way, and on the 6th of October last he died by his own hand.

A man of singular acuteness, and wonderful intensity of purpose, what he undertook to do he accomplished, and did it well. The personal defects he had to contend against were what few could have overcome, but he was able with complete success to lecture to large audiences, and as a scientific chemist, he had reached the foremost rank. In 1869, the Royal Society awarded to him a Royal Medal, and, in 1870, he was chosen one of the examiners of chemistry at the University of London.

Karl Weltzien, the son of a Russian merchant, was born in St. Petersburg, but was removed, at the age of ten, to South Germany, on account of his delicate health. The family settled in Karlsruhe, and in 1831, Karl entered the University of Heidelberg, as a student of medicine, taking his degree of Doctor of Medicine in 1835. He was, however, led to this course of study rather by the love of natural science than by a desire to enter upon the practice of medicine ; and accordingly he devoted himself especially to the study of chemistry, which he afterwards pursued further under Mitscherlich's direction in Berlin. On his return to Karlsruhe, he was made Extraordinary Professor of Chemistry at the Polytechnic Institute, and ten years later, on the reorganisation of the Polytechnic Institute, he was appointed Professor of Chemistry and President of the Chemical School. A new laboratory was at the same time fitted up under his direction. Here he worked happily as teacher and investigator for sixteen years, training many able students, who made important investigations under his direction ; writing seven original works on chemical subjects, and performing a large number of original researches which were published in Liebig's *Annalens*. He was always ready to apply his scientific knowledge and skill to the general good, with which view he gave instruction in various technical applications of chemistry, and lectured for many years to the officers of the Artillery on those departments of the science which relate especially to their profession. He also gave great attention to the sanitary condition of the town, and analysed a large number of spring and well waters, with a view of obtaining an improved water supply. This last investigation led him to the discovery of a new method of esti-

mating nitric acid. In 1868, he resigned his Professorship in consequence of failing health, and the remainder of his life was a constant struggle with severe suffering, which he bore with great fortitude. He died on the 14th of November, 1870.

After the reading of the address, and the obituary notes, the Treasurer presented his account of the Society's finances, which shows a balance of £1333 1s. 3d. at the banker's. Of this sum, however, £220 8s. are subscriptions to the Special Fund for the monthly Reports.

The elections of the President, the Officers, and the ordinary Members of Council for the ensuing year then took place, and the following are the names of the gentlemen elected:—

President—E. Frankland, D.C.L., F.R.S.

Vice-Presidents, who have filled the office of President—Sir B. C. Brodie, F.R.S.; Warren De la Rue, Ph.D., F.R.S.; A. W. Hofmann, D.C.L., F.R.S.; Lyon Playfair, Ph. D., C.B., F.R.S.; A. W. Williamson, Ph.D., F.R.S.; Col. P. Yorke, F.R.S.

Vice-Presidents—H. Debus, Ph.D., F.R.S.: J. H. Gilbert, Ph.D., F.R.S.; H. M. Noad, Ph.D., F.R.S.; W. Odling, M.B., F.R.S.; T. Redwood, Ph.D.; J. Stenhouse, Ph. D., F.R.S.

Secretaries—A. Vernon Harcourt, M.A., F.R.S.; W. H. Perkin, F.R.S.

Foreign-Secretary—H. Müller, Ph.D., F.R.S.

Treasurer—F. A. Abel, F.R.S.

Members of Council—E. Atkinson, Ph.D.; H. Bassett; C. L. Bloxam; A. Dupré, Ph.D.; F. Field, F.R.S.; M. Holzmann, Ph.D.; H. McLeod; E. J. Mills, D.Sc.; H. E. Roscoe, Ph.D., F.R.S.; W. J. Russell, Ph.D.; R. Angus Smith, Ph.D., F.R.S.; A. Voelcker, Ph.D., F.R.S.

The Scrutators were Messrs. Spiller and Armstrong.

The customary votes of thanks to the President, the Secretaries, the Treasurer, &c., brought the anniversary proceedings to a close.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

March 21st, 1871.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. John Hopkinson, D.Sc., was elected an Ordinary Member of the Society.

"*On the Mechanical Equivalence of Heat*," by the Rev. H. HIGHTON, M.A.

The following is an abstract of the arguments as given in the paper and brought out in the subsequent discussion.

(1). The author apologised for having mentioned other names in connection with great discoveries which were undoubtedly due primarily to Dr. Joule, and spoke of the very great value of Dr. Joule's experiments, even when he did not agree with the deductions drawn from them.

(2). The subject is of extreme importance both for the interpretation of physical phenomena and for determining what limits are assigned by the stern laws of Nature to the exercise of man's mechanical and scientific skill.

(3). No doubt Dr. Joule has ascertained the heat ordinarily derived from the destruction of energy, by means of friction with various substances, but it has been assumed, *in defiance of facts*, that the numerical relations which connect heat and energy in the case of friction hold good when energy and heat produce or destroy each other by any other means.

(4). In the case of friction itself, energy is not transformed simply into heat, but partly into heat and partly into another kind of energy, which is involved in the expansion of the solids or liquids acted on.

(5). No doubt the coincidence between the mechanica equivalent of heat, found by Dr. Joule from friction, and that by M. Favre from working a magnetic engine, seems very striking; but

(*a*). The value of Favre's experiment disappears on examination. It was but a single experiment, either never repeated, or never repeated with the same results; in a very delicate experiment there was only the difference of 300 units out of 18,000; and even the permanent enlargement which always takes place in magnets which are in use might account for these; and

(*b*). Numerous and long-continued experiments by M. Soret show results entirely discordant with the single one of M. Favre.

(6). It seems incredible, that with the imperfectly constructed engine used by Joule and Scoresby, they should at the very first trial have succeeded in utilising two-thirds of the magnetism evolved, or capable of being evolved, by their battery; and Dr. Joule now tells us that according to his latest calculations of the mechanical equivalence of heat, they utilised six-sevenths of the power of the battery. The only conclusion we can arrive at is, that the real power of the battery, and therefore of a grain of zinc, must have been much greater than he calculated.

(7). For consider the disadvantages under which the engine acted.

(*a*). The temporary and permanent magnets were never nearer than quarter of an inch apart. Though Dr. Joule assures us this does not affect the *power* of the engine, it certainly produces a waste of zinc, as the near approach of the magnets creates counter-currents which check materially the consumption of zinc.

(*b*). The copper wire was not tested for conductivity; a subject little thought of at that time, and it is found that a very small impurity in copper wire will very, very largely diminish the power of an electro-magnet.

(*c*). The iron was not tested for specific capacity for magnetism; yet this is a most important point which is even now but little appreciated. It is found practically that, if two electro-magnets be made from the very same piece of iron, most carefully prepared, with the very same length of the same wire, without the slightest assignable cause, one will sometimes have three times the power of the other. Hence I conclude that the maximum energy capable of being evolved by a grain of zinc must be very much greater than that assigned to it by Dr. Joule.

(7). Dr. Hopkinson's argument, in his paper lately read to this Society, virtually amounted to this—that a well-constructed magnetic engine will get no more duty from a grain of zinc than an ill-constructed one; and consequently, I presume, that magnets might be weakened to any extent, and removed to ever so great a distance from one another, without necessarily affecting the efficiency of the engine.

(8). Dr. Hopkinson has in his criticism strangely substituted $(a-b)$ for (b). In Joule and Scoresby's paper, the consumption of zinc is expressed not by $(a—b)$ but by (b); and consequently the duty of a grain of zinc not by

$$\frac{W}{a-b} \text{ but by } \frac{W}{b}$$

and when the magnets are stronger and approach nearer to each other, even if W be not increased, (b) is diminished.

(9). My argument was this, that since the accepted theory of the mechanical equivalence of heat is *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, then the doctrine of the mechanical equivalence of heat cannot be true; we might with equal justice call it a mechanical equivalence of cold. It is no reply to say that such facts are simple deductions from the laws of thermodynamics. This would only show that the laws of thermodynamics are inconsistent with the doctrine of the mechanical equivalence of heat.

(10). The argument from the fire syringe I withdraw, as inconclusive. But I think my case was sufficiently established without it.

(11). Joule and Scoresby in their paper incorrectly assume that if the quantities of electricity in the current at different times be represented by (a) and (b), the heat varies as a^2 to b^2. This is only true where the resistance is the same. In the cases before us the working of the engine introduces a fresh element in resistance.

(12). Again, by assuming that (a—b) represents diminution of quantity of the current, *and* the diminution in the zinc consumed, *and* the heat converted into useful work, they involve the supposition either that less zinc produced equal heat, or that heat was changed into useful work which was never produced at all, and, therefore, could not be absorbed. In fact, there was no *proof* that any heat was absorbed at all.

(13). It is said that in electro-plating, electro-magnetic engines, worked by steam, are found more economical than batteries. This is in cases where a battery of many cells would be required, which is always wasteful, as a large number of equivalents of zinc must be consumed to deposit one equivalent of silver or other metal.

(14). Besides, there is a far greater advantage in changing work into electricity than electricity into work. In the former case all, or nearly all, the work is effective; in the latter, a very small portion of the electricity has hitherto been utilised.

Dr. Hopkinson said that most of Mr. Highton's objections to the mechanical equivalent of heat appear to arise from a mistake as to what is meant by the term. The nature of this mistake may be best seen in the case of a perfect heat engine, of which t_1 and t_0 are the absolute temperatures of the source and refrigerator. Then from every unit of heat leaving the source we obtain

$$\frac{t_1 - t_0}{t_1} J$$

units of work. Now this a quantity variable with t_1 and t_0; it would be similar to most of Mr. Highton's arguments to infer that from a given quantity of heat a variable quantity of work could be obtained. But, of course, the case really is, that, of the unit of heat leaving the source $\frac{t_0}{t_1}$ is lost in the refrigerator, whilst $\frac{t_1 - t_0}{t_1}$ disappears *as heat* and is converted into the work done, and the principle of the equivalence of heat and work asserts that J is constant. It will be seen that this is the mistake Mr. Highton makes in his paper in the *Quarterly Journal of Science* (end of article 6). He seems there to imagine it stated, that the work done is equivalent to the whole heat thrown into the gas, and he fails to perceive that a certain portion is used to raised the temperature of the air or turpentine.

This will make my criticism of his subject in the Chemical News clearer. Mr. Highton argued against the mechanical equivalent, and what I pointed out was, that the chemical energy, which was converted into mechanical effect and *not* used to heat the wire, was proportional to $a-b$, that, therefore, in order to prove that there was no mechanical equivalent, Mr. Highton must show $\frac{W}{a-b}$ is variable. I do not assert that a badly constructed engine will get as much heat from fuel as a good one, but merely that the work done and the heat, which has disappeared as heat and been converted into work, are in a constant ratio.

Now as regards Mr. Highton's argument from the case of elastic wires—that the wire will be cooled when stretched—follows from the two laws of thermodynamics; a proof may be seen in Tait's "Thermodynamics," p. 105. Mr. Highton replies, "Quite true; but this only shows that one of the laws of thermodynamics is inconsistent with the doctrine of the mechanical equivalence of heat." Now the first law of thermodynamics asserts nothing else than that there is a mechanical equivalent, constant in all cases; whilst the second law as usually stated involves the first law, and involves nothing else but Carnot's axiom and the principle that in conduction heat flows from the hot to the cold body, both of which no one will doubt. Mr. Highton's reply is very similar to stating that one of Kepler's laws is inconsistent with the planets moving in ellipses. What Mr. Highton proposes as a paradox is then a necessary consequence of the principle he attacks.

Though the doctrine of the mechanical equivalent of heat finds its firmest basis in the immortal experiments of Dr. Joule, the fact, that assuming it we can explain many phenomena, is a valuable supplementary proof.

"*Examples of the Performance of the Electro-Magnetic Engine*," by J. P. Joule, D.C.L., F.R.S., &c. (Next week).

NOTICES OF BOOKS.

First Annual Report of the Deputy Master of the Mint, 1870. Presented to both Houses of Parliament by command of Her Majesty. London: Eyre and Spottiswoode, 1871.

This volume contains, in about 130 pages, a large amount of interesting and valuable information. The report is divided into the following three parts:—Historical Account of the Coinage and the Mint; Consideration of the Principles of Minting and Regulations of the Mint; Proceedings of the Mint during the year 1870, and Changes of Establishment, &c., recently made.

To the report is added an appendix, from which we abstract the following portions of the memoranda and reports from the able hand of Mr. Roberts, Chemist to the Mint:—Most of our readers are aware that gold, even when mixed with very minute quantities of either lead, antimony, arsenic, or bismuth, becomes what is technically called "brittle," that is to say, it breaks so readily, under even a slight blow, as to become unfit for undergoing the various mechanical operations of converting it into coin. It appears from a long correspondence between the authorities of the Bank of England and bullion importers, as well as between the former and the Mint officers, that brittle gold is of far more common occurrence than even scientific men would be at first sight led to believe. While various modes of remedying this defect have been now and then proposed, it appears that the process, first suggested and successfully applied by Mr. Miller, of the branch Mint at Sydney, has, after a series of trials and experiments at the hands of Mr. Roberts, proved the best remedy for this evil. The loss of gold while in molten state, under the action of chlorine gas, is very trifling indeed, while the result of the operation (lasting only for four minutes, with a quantity of over 300 ozs. troy), even upon gold containing 0·05 per cent of antimony and 0·05 of arsenic, was perfectly successful; as was also an experiment upon a bar of gold containing 1·5 per cent of base metals, viz.—antimony, 0·3; lead, 0·2; zinc, 0·2; iron, 0·2; tin, 0·2; arsenic, 0·3; bismuth, 0·1. The gold, treated with chlorine, was found to be perfectly tough, and in every respect suited for undergoing the necessary operations of coining.

Mr. Roberts's memorandum on Assaying, illustrated by engravings, is an excellent monograph on this subject. It is divided into the following sections:—Assaying as an operation of Minting; Gold Assaying; Silver Assaying; Method of Reporting the results of Assays; Degree of Accuracy attainable; Assaying as a means of Account. From this section we quote the following interesting particulars:—Analysis of the assay reports (composition) on coins taken from half-a-million of gold pieces issued by the Mint in 1870.—The mean composition was, per mille = 916·617 gold, and 83·383 copper. That this result is in the highest degree satisfactory, and a proof of the high perfection of workmanship—aided by sound science—attained at the Mint, our readers may judge

from the fact that gold coins would be within the remedy of fineness prescribed by law, if the amount of precious metal they contained varied per mille from 914·6 to 918·6; while the coins issued by the Mint exhibit scarcely any divergence from the standard 916·6. The importance of maintaining the true standard composition of the coins is clearly shown by the fact that a variation of *one-tenth of a millième* above or below the standard represents a gain or loss to the department of nearly £100 on every million sterling issued.

The monograph above-named further contains a chapter on trial plates, and on the adjustment of blanks. The whole report and appendix bear strong evidence that on the part of the officers of the Mint, everything is done to bring that establishment to the highest possible efficiency. Combined with proper economy, and aided by scientific as well as engineering and mechanical skill, the Royal Mint may justly take rank as the first among these establishments in Europe.

CORRESPONDENCE.

CONCENTRATION OF SULPHURIC ACID.

To the Editor of the Chemical News.

Sir,—I have not observed any process by which sulphuric acid may be evaporated at a lower temperature than the boiling point of the liquid, and I wish to draw the attention of manufacturers of this acid to the following process which I have successfully tried.

The acid is heated in a lead pan in the usual way, and when it has reached a temperature of 300° F., a current of atmospheric air is blown through it, keeping it up to this temperature by the fire under the lead vessel.

By this means brown vitriol of 1·700 can easily be made without raising the temperature much above 300° F., and concentrated vitriol may also be made in a leaden pan by the same means at a temperature of 500° or so F.—I am, &c.,

James Stoddart.

Uphall Mineral Oil Works, N.B.
March 27, 1871.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus des Séances de l'Académie des Sciences, November 28, 1870.

Use to be made of Osseine as Food.—E. Fremy.—This very lengthy and exhaustive memoir contains the following sections:—On the difference existing between gelatine and osseine, and on the nutritive value and properties of the latter; preparation of osseine from bones; boiling of osseine, and the modifications it thereby undergoes; aromatisation of the osseine so as to render it more palatable and digestible. This memoir gave rise to a very lengthy discussion, here reproduced verbatim, in which M. J. Dumas, Dr. Liouville, and the author of the paper just mentioned took part. The gist of this discussion is, whether the osseine is really different from gelatine as food.

New Odoriferous Acid, the Product of the Fermentation of several Nitrogenous Matters, and particularly so of Tendons.—E. Chevreul.—The author says that, as far back as the year 1820, he had observed that the fermentation of some animal substances, especially the tendons, and parts used for making glue, produces a peculiar acid, which predominates over the ammonia, also formed by the fermentation (putrid) here alluded to. The acid in question is analagous to that which the author discovered in suint, and termed paraphocenic acid, both belonging to the class of volatile fatty acids. The bad smell given off by glue and size is due to this acid, which is also formed in the water wherein, in anatomical laboratories and dissecting rooms, parts of corpses are macerated.

Gas to be Used Instead of Coal-Gas for Filling Air-Balloons. H. Hureau de Villeneuve.—The author proposes the use of gas from wood, but, as observed by M. J. Dumas, this gas always contains a large proportion of oxide of carbon, which, as instanced by the trials made with wood-gas, caused asphyxia to the aëronaut, Dupuis-Delcourt. Moreover, even under the extraordinarily exceptional conditions Paris was placed in, there was no lack of coals for producing the gas necessary for filling air balloons.

New Arrangement of Galvanic Batteries, applicable also to the kind known as Bunsen's.—J. C. d'Almeida.—The advantages of the arrangement here alluded to, and described at great length, are stated to consist in—(1) The rapidity of getting the cells in working order, so that sixty cells are put in activity in the same time it now takes to arrange one in order; (2) economy in the use of acids; (3) transportability of the battery without any risk of breakage.

December 5, 1870.

Nutritive Properties of the Organic Substances met with in Bones, and on the Composition of the Alimentary Rations capable of Sustaining the Human Body in a Proper State of Health.—Dr. Milne Edwards.—This lengthy memoir contains, *en resumé*, a review of the labours of D'Arcet, Magendie, Dupuytren, and others on the subject mentioned.

Observations on some portions of M. Fremy's Paper "On the Use of Osseine as Food."—E. Chevreul.

Experimental Researches on the Nutritive Properties of Coca Leaves.—Ch. Gazeau.—A lengthy memoir detailing some experiments made with the leaves of the *Erythroxylon coca*, a well-known shrub, native of South America, the leaves being chewed like tobacco.

Specific Heat of Gases under Constant Volume.—J. Moutier.—An algebraico-physical essay.

Preparation of Osseine and Gelatine.—A. Riche.

Refining and Purifying of the Crude Tallow of Commerce.—J. Castelhaz.—The author proposes the use of weak alkaline solutions instead of the method and means now employed for this purpose. From the discussions to which the reading of this paper gave rise, it appears that this plan was adopted long ago, and is described in M. Payen's well-known work "Chemie Industrielle," vol. ii., p. 771.

December 12, 1870.

This number contains the following original papers and memoirs bearing upon chemistry and allied sciences:—

Hippophagy: Fat, Alimentary Oils, and Gelatinous Substances Obtainable from the Tissues and Bones of Horses and Cattle.—A. Payen.

Decemdiurnal or Tridodecuple Period of the Atmospherical Phenomena, and the Influence thereof on Health and Disease.—Ch. Sainte-Claire Deville.—This very lengthy essay on applied meteorology is illustrated by several diagrams.

Algebraical Formula for the Velocity of Sound.—J. Moutier.

Introduction of the Swine as Domesticated Animals by the Ancient Egyptians.—F. Lenormant.

December 19, 1870.

This number opens with the first portion of an historical *resumé* and essay on—

Gelatine, and the Investigations and Labours this Substance has been the Object of.—E. Chevreul.

This number further contains the following papers relating to chemico-physical and collateral sciences:—

Process Employed by the Tribe of Flat-Headed Indians to Extract Oil from the Long Bones of Animals.—Dr. Roulin.

Physical Geography: The Seine; Studies on the Quantity of Rain; on the Sources and Feeders of that River, and Application of that Knowledge to Engineering and Agriculture.—J. Belgrand.—This very lengthy memoir is, as indicated by the heading, chiefly of local interest.

Thermo-Dynamics: Force Exerted by Explosive Substances.—A. Cazin.—An algebraico-physical essay.

Study on the Volcanic Gases of Santorin.—Dr. Fouque.—From the contents of this lengthy memoir, it appears that the gases emitted by volcanoes vary very greatly in composition; that the pasty lava carries along with it combustible and other gases occluded in the pasty mass; that free hydrogen and marsh gas prevail and are more abundant as the temperature at which the lava escapes is higher; lastly, that hydrogen and oxygen co-exist in these gas mixtures without combining together, a result due in all probability to the extremely high temperature, being sufficient to keep the vapour of water dissociated.

Process of Panification whereby the Whole Grain of Wheat is Employed along with Flour.—M. Dubrunfaut.—The author proposes to steep the wheat in water to soften it; next, to crush it, by means of machinery akin to the well-known linseed crushers; and then to use the material along with flour in making bread.

December 26, 1870.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

Continuation of the Memoir on Gelatine, &c.—E. Chevreul.

Researches on the Solid State of Bodies.—J. Moutier.—An essay on the state of aggregation.

Eclipse of the Sun of December 22nd, 1870: Measurement of the Variation of Light.—C. Flammarion.

Various Methods of Preserving Meat without the Use of Salt.—L. Soubciran.

Mode of Solidification of Our Globe.—S. Meunier.—From researches made on meteorites, the author comes to the conclusion that the solidification of our globe has proceeded from the surface to the centre.

Bulletin de l'Académie Royale des Sciences, des Lettres et des Beaux Arts de Belgique, No. 1, 1871.

This number opens with the programme of the questions proposed by the Academy for essays on the following subjects relating to chemistry and collateral sciences. The complete study and investigation of an organic alkaloid containing oxygen and nitrogen, preferably quinine; in order to elucidate the intimate constitution of that body, and the place to be assigned to it in a seriary classification. The complete discussion is demanded on the question of the temperature of space (of the universe), to be based upon experiments and calculation, and with explanation on the motives, in reference to the different temperatures attributed to space. The composition of the plutonic rocks, or such as are so viewed, which are met with in Belgium and French Ardenne. The value of the medal to be given as prize for approved answers to the first and second questions here quoted will be £40, and for the other £24. Answers, written in Latin, French, or Flemish, should be forwarded, carriage paid, to M. A. Quetelet, perpetual secretary of the Academy at Brussels, before June 1, 1871.

This number further contains two short notices on the—

Aurora Borealis observed in Belgium from September to December, 1870—And on the—

Eclipses of the Sun, December 22nd, 1870, and of the Moon, January 6th, 1871, as observed at Brussels—By M. A. Quetelet.

Fossil Reptiles found in Belgium.—P. J. van Beneden.—A catalogical review of this subject.

NOTES AND QUERIES.

Distillation of Wood.—In the CHEMICAL NEWS, vol. xxiii., p. 91, was commenced a very interesting account of the distillation of wood, by Mr. E. T. Chapman. I believe that a great many of your readers would be highly interested to read the further particulars of this paper. —W. HOFMANN.

Welding Steel and Iron.—(Reply to W. A. Murray.)—No such process can be given, as the white heat required for welding iron to iron would "burn" or decarbonise the steel; for instance, the making of the so-called cold chisels from cast-steel requires great care in regulating the heat of the forge, so as to prevent the steel breaking off while being hammered out on the anvil.—A.

Terra Alba.—In November, 1869, I had occasion to make some enquiries about terra alba, which were answered by a party signing his name as "Nemo" in the CHEMICAL NEWS (vol. xx., p. 252), and in which he stated that, if "Selenite" would send his address to him, he would send him a pamphlet on the subject. I would be glad to have the address of "Nemo" also, and I may be able to communicate some valuable information to him in relation to the subject of terra alba. —JOHN MANNING, Windsor, Nova Scotia, Dominion of Canada.

MEETINGS FOR THE WEEK.

TUESDAY, 11th.—Photographic, 8.
—— Civil Engineers, 8.
WEDNESDAY, 12th.—Society of Arts, 8.
FRIDAY, 14th.—Astronomical, 8.
—— Quekett Microscopical Club, 8.
SATURDAY, 15th.—Quekett Club. Excursion to Barnes; to meet at Waterloo Station (Richmond Line) at 2 p.m.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 594.

ON AMMONIUM AND THE SOLUBILITY OF METALS WITHOUT CHEMICAL ACTION.*

By CHARLES A. SEELY.

In April and May last it came in my way to make a pretty careful study of the constitution of the so-called ammonium amalgam, and on May 9th, I reported to the Lyceum of Natural History of New York what I had done in the matter up to that time. Subsequently, I continued the study, and was led to the discovery of the solubility of the alkali metals in anhydrous ammonia, an account of which formed a paper read at the Troy meeting of the American Association for the Advancement of Science. Neither of these communications have been printed; the first was oral, and the latter was lost. The present paper is intended to be a concise and authoritative statement of the contents of the two papers and as a substitute for them.

In the Lyceum communication I proved that the so-called ammonium amalgam is in fact no amalgam at all, but a metallic froth of which the liquid part is mercury and the gaseous part a mixture of hydrogen and ammonia. This conception of the nature of the substance was at that time not altogether novel, but the considerations through which the conclusion was reached were believed to be new and of such moment that, taken together, they have the effect of a demonstration. The considerations were these:—

1st. In the formation of ammonium amalgam the mercury is increased in bulk tenfold or more, while in weight it is increased only one or two thousandths. This fact would be easily predicted by the froth-theory, while otherwise it is anomalous and utterly inexplicable.

2nd. Ammonium amalgam has less of the mirror surface than mercury; it approaches in appearance the whiteness and lack of lustre of matt silver. This fact also finds a satisfactory explanation in the froth theory.

3rd. Ammonium amalgam is readily compressible; when confined in a fire syringe it steadily contracts on forcing down the piston till the original bulk of the mercury is almost reached, and on relieving the pressure the original volume and appearance are resumed. It is only a froth which behaves in this manner.

Either (1) or (3) will be conclusive to many minds; (3) is put forward as a fair *experimentum crucis*. The condensation by pressure is easy to execute, and an elegant experiment for the lecture-room. The condensation is somewhat greater than in the case of atmospheric air, for the reason that some of the ammonia gas goes into the liquid state.

Having concluded that the ammonium amalgam is a mercuric froth, a very important question presents itself: Why and how do metal and gases become mingled? A froth is a mechanical mixture, and in all ordinary cases the manner of mixing and the forces engaged are quite evident. In the formation of the mercuric froth, the mercury covered by a watery solution rises up spontaneously into the comparatively light water, and the still lighter gases descend into the mercury and mingle with it until they become diffused through its whole mass. What are the forces which thus overpower the action of gravity? Moreover, a froth implies a certain viscosity or plasticity of the liquid which seems not to be possessed by the mercury in its normal condition. Do hydrogen and ammonia in their nascent state have the power to modify the physical properties of mercury? The production of mercuric froth as a fact of physics simply may seem anomalous and paradoxical. Is it necessary after all to admit the existence of an ammonium amalgam, not of course as a constituent of the mercuric froth, but as a condition precedent of the formation of the latter,—in other words, of another ammonium amalgam to satisfy a new thory?

I was making satisfactory progress in the investigation of the theory of mercuric froth when, early in May, my attention was diverted to the researches of Weyl on ammonium and compound ammoniums as reported in Watts's "Dictionary" (article, Sodium), and in the last edition of Fownes's "Chemistry;" subsequently, I compared Weyl's original memoirs in Poggendorff's *Annalen*, of 1864. Weyl's statements and conclusions were quite at variance with my own views; his alleged facts were inconsistent with my own experience. Weyl, it appeared, had actually isolated ammonium; if he represented the facts correctly all my work needed revision. I therefore at once devoted my whole care to a verification of his results. I repeated his experiments, some of them many times, and with such precautions, modifications, and additions as the case seemed to require. The result of all is that I find that Weyl has had only an imperfect apprehension of the fundamental facts, and that thus building on a weak foundation the superstructure of reasoning and conclusion cannot stand. For the purpose of presenting in a clearer light what I suppose to be of more interest to the chemical world, I dismiss, for the time, the consideration of Weyl's views, and I proceed to give what in my judgment are the proved physical and chemical facts.

This discussion involves the relation of ammonia to the alkali metals, and the results which grow out of that relation. Now, the key to the whole subject is the fact that liquid anhydrous ammonia is a solvent, without definite chemical action, of the alkali metals. I mean that these metals dissolve in the ammonia as salt dissolves in water,—the solid disappears in the liquid, and on evaporating the liquid, the solid reappears in its original form and character. There is no definite atomic action in any such cases; the components of the solution are not changed in their chemical relations to other substances.

I began my experiments by bringing together ammonia and the metals in sealed inverted U-shaped glass tubes. Chloride of silver saturated with ammonia was placed in one leg of the tube, and the metal under test in the other; the chloride of silver leg being immersed in boiling water, and the other in ice-cold water, the ammonia was liberated and passing over was gradually condensed on the metal. Snch an arrangement, although sufficient to demonstrate the fact of the solution of the metal, proved inconvenient in several respects; the tube is liable to burst, the experiment can be made only on a pretty small scale, the chloride of silver does not absorb a desirable amount of ammonia, and by reason of its fusing or agglomerating, the rapidity of its absorption varies greatly. I therefore constructed an ammonia generator or retort of iron, to the horizontal neck of which turned downward at its end, I attached by means of an iron screw coupling the stout glass tube containing the metal or other substance to be subjected to the ammonia. In the place of the chloride of silver I substituted anhydrous chloride of calcium saturated at 32° F. with anhydrous ammonium. With this arrangement the experiments were continued with comparative ease and expedition.

When sodium is subjected in this apparatus to the condensing ammonia, before any ammonia is visibly condensed to the liquid state, it gradually loses its lustre, becomes of a dark hue, and increases in bulk. The solid then appears to become pasty, and at last we have only a homogeneous mobile liquid. During the liquefaction, and for a little time after, the mass is of a lustrous, copper-red hue; the condensation of the ammonia and its mingling with the

* From advance-sheets of the *Journal of the Franklin Institute*. Communicated by the Editors.

liquid steadily goes on, the liquid is progressively diluted, and passing through a variety of tints by reflected light at last it becomes plainly transparent and of a lively blue as well by reflected as by transmitted light; the liquid now closely resembles a solution of aniline blue or other pure blue dye-stuff. On reversing the process by cooling the ammonia generator, the ammonia gradually evaporates out of the liquid, and the changes observed during the condensation reappear in the reverse order, till at last the sodium is restored to its original bright metallic state. If the evaporation be conducted slowly and quietly the sodium is left in crystals of the forms seen in snow. The formation of the transparent blue liquid, and the restoration of the sodium are steadily progressive, and the repeated and closest scrutiny of the process has failed to reveal the slightest break or irregularity in its continuity. The inevitable conclusion from such facts is that the blue liquid is a simple solution of sodium in ammonia, not at all complicated or modified by any definite chemical action.

If crystallised sodium amalgam be made to replace the sodium in the experiment, it remains unchanged, although it is probable enough that at a higher temperature or with a larger excess of sodium, some of the sodium would be removed. This interesting fact is consistent with the simple solubility of sodium in ammonia, and indeed is some confirmation of it. The force, whatever it be, which keeps together sodium and mercury is certainly weaker than that exhibited between sodium and ammonia. If sodium amalgam is not a definite chemical compound, then surely the blue liquid cannot be. I have not yet made the experiment to determine whether mercury will remove sodium from its ammoniacal solution, but it seems extremely probable that it would so act.

The brilliant and varied colours exhibited in the experiment may seem anomalous to some, when, in fact, a closer scrutiny of the case will show that they might have been predicted. Sodium appears white to the eye, but with the white light reflected from its surface to the eye, there are always mingled red rays. If most of the incident white light were normally decomposed, sodium would appear as a brilliant red metal. Ammonia favours such a decomposition probably by reducing the density and opacity of the surface, and thus the concentrated solution of sodium is lustrous copper-red by reflected light. What should be the colour by transmitted light? Not red, for the red rays do not penetrate the substance; the colour must be looked for in that which is complementary to red; it must be blue or yellow, or combinations of these. A continuance of the argument will bring the conclusion that the colour by transmitted light will be blue. Intense blue tinctorial substance like aniline blue, indigo, and Prussian blue, all illustrate the phenomena of colour of the sodium solution; they are metallic red when concentrated, and if the solvent be applied in vapour as in the sodium dissolving experiment, there will be the same modification of colour exhibited. Sodium has a remarkable tinctorial power which seems not to be surpassed by that of any of the aniline colours.*

If a salt of a metal electrically negative to sodium be subjected with the sodium to the ammonia, in the apparatus, the sodium will replace the negative metal, and the latter will be reduced to the simple state. This reduction seems always to be accompanied with the evolution of heat, but does not commence till a visible quantity of the sodium solution is formed. It is possible that there may be exceptions to these generalisations. I need only say that they seem extremely reasonable, and are confirmed by a good many experiments. The reaction furnishes a very simple and elegant process for the reduction of rare metals. I anticipate that the process will prove of great value.

* For a further elucidation of the matter of this paragraph, I would refer the reader to my paper on the "Colours of Metals," printed in the proceedings of the Lyceum of Natural History of New York, October, 1870.

I have heretofore spoken of the solution of sodium only, for the reason that it is a typical case, and because demonstrations can be more satisfactorily made with it. Potassium behaves towards ammonia in almost precisely the same way. It is readily dissolved, the strong solution being copper-red and the diluted solution blue. Lithium gives also a blue solution, but is not nearly so soluble. The only other alkali metal I have tried is rubidium, and although the experiment was prematurely ended, it had progressed far enough to make it almost certain that rubidium is soluble like potassium, and I intend, as soon as I can procure material, to repeat the rubidium experiment.

Besides the metals named, I have subjected many others to the ammonia, and have found none outside of the alkali metals which are in the least affected. I have directly tried aluminium, magnesium, thallium, indium, mercury and copper.

Chemical Laboratory, 26, Pine Street, N. Y.

ON THE CORONA SEEN IN TOTAL ECLIPSES OF THE SUN.

By Professor W A. NORTON.

In a communication to the *American Journal of Science and Arts*, of September, I alluded briefly to the auroral theory of the solar corona, and referred to publications in which I had advocated to it. I propose now to give a brief discussion of the theory.

The grounds upon which I have maintained the auroral origin of the corona in different publications are the following:—

(1). the corona cannot be the permanent atmosphere of the sun, shining by reflected light, since its outline is neither circular nor oval, but exceedingly irregular, and it extends out from the sun many times farther in some directions than in others. The utmost that can reasonably be maintained is that for a small portion of its outward extent, for which the gradation of light is nearly uniform, it may possibly be a solar atmosphere.

(2). The natural indications of the aspect of the corona are that it is chiefly composed of separate masses of luminous matter, of unequal brightness and length, radiating out from different points of the sun's limb. The general radiated structure of the corona, and the great comparative outward extent, of the luminous radiations in certain directions, have attracted the attention of the observers of all modern eclipses. Some streamers have been seen to extend more than 1,000,000 miles from the sun, while others did not extend to one quarter of this distance.

(3). Reasoning analogically from the earth to the sun we naturally conceive the body of the sun to be surrounded by a permanent atmosphere. On the same grounds we should infer that the space exterior to this atmosphere is pervaded, either occasionally or permanently, by auroral streamers, similar to those which at times shoot out many hundreds of miles into space, from the upper atmosphere of the earth.

(4). If the luminous radiations of the corona are in fact auroral streamers, we must expect that they will not be permanent in their extent and position. Now it is well known that such is the fact, for the aspect of the corona has been very different in different eclipses (*e.g.* eclipses of 1842, 1851, 1858, and 1860). It has even been maintained by some observers that the rays of the corona had a flickering lustre, and varied in extent and position during the short period of a single eclipse.

(5). Admitting, as we must, the actual radiated structure of the corona, its individual streamers, or luminous radiations, may be conceived either to be permanently connected with the sun, or to be composed of luminous

matter actually streaming away from the sun, to an indefinite distance, into space. If we adopt the former idea, we virtually admit that a permanent vaporous atmosphere of sensible density extends from the body of the sun to a distance greater than to sun's diameter, a position that cannot with any plausibility be maintained.* In support of the other hypothesis we have the well-established fact that some form of luminous matter belonging to cometary bodies, when it comes under a certain degree of influence from the sun, is projected or in some manner detached from the nuclei of these bodies, and repelled from the sun, and under the operation of the solar repulsion urged away from them to an indefinite distance, forming the luminous trains by which they are attended. (See the author's papers on Donati's Comet published in *the American Journal of Science*, January and May, 1860, and July, 1861; and the discussion of the Dynamical Condition of the Head of a Comet in the No. for January, 1859).

To suppose that the rays of the corona are actual radiations of luminous matter, is only to suppose that a portion of the photospheric matter of the sun becomes subject to the operation of the same forces, that we perceive the sun to exert upon a portion of the matter of comets. The luminosity of such radiations may be ascribed either to a reflection of the sun's light or to electric discharges. Upon this question we shall see important evidence was obtained at the total eclipse of August 7, 1869.

(6). If we adopt the auroral theory of the corona, and at the same time admit that the auroral streamers are actual emanations of luminous matter, the following consequences may be expected to follow:—

(*a*). A portion of the auroral matter emitted from the sun should fall upon the earth's atmosphere, and may furnish the substance of terrestrial auroras, for which no terrestrial origin has yet been detected.

(*b*). Upon this view of the possible origin of terrestrial auroras, the close correspondence that has been detected between the periods of the sun's spots and of auroras, should subsist if we allow that the spots are merely the natural result of the supposed discharges of the solar matter, prevailing for a time at certain points of the photosphere; or, indeed, if we grant that they are in any way the result of these discharges visible in the corona.

(*c*). In the wave propagation of the impulsive actions on the ether of space, of the electric discharges to which we may ascribe the material emanations from the photosphere, and in the electric and magnetic phenomena attendant upon the reception and accumulation of the solar auroral matter in our atmosphere, we have a plausible general explanation of the periodic and irregular disturbances of the magnetic condition of the earth, and of their known physical relations to the sun's spots. This theory of the origin of the diverse perturbations of terrestrial magnetism I have elaborated, and followed out into a detailed discussion of the variations of the different magnetic elements in former numbers of the *American Journal of Science and Arts* (viz., for March and July, 1855).

We may add that it derives additional support from the general result arrived at by Professor Chambers, in his discussion of the "Nature of the Sun's Magnetic Action," viz., that "the mode in which forces originating in the sun influence the magnetic condition of the earth, is not analogous to the action of a magnet upon a mass of soft iron placed at a great distance from it, but that these forces proceed from the sun in a form different from that of magnetic force, and are converted into this latter form of force probably by their action upon the matter of the earth or its atmosphere." If this be admitted, then we must conclude that the perturbations of the earth's magnetic condition, as evinced by the variations of the position and directive force of the magnetic needle, must result either from some action direct or indirect on the earth or its atmosphere, of some form of matter emitted from the sun, or from a wave-action propagated from the sun, or from both of these operative causes combined.

(*d*). The streamers of the corona should have at different points of the sun's photospheric surface different directions, parallel to the diverse directions of the magnetic force of the sun at this surface. These directions should be variously inclined, in different heliographic latitudes, to the horizontal lines at the points of the surface, and also to the plane of the sun's equator; like the dipping needle on the earth and the streamers of a terrestrial aurora. In low latitudes the angles of inclination to the plane of the equator should be large, and the streamers proceeding from corresponding points in the two hemispheres should converge and intersect in the plane of the equator. In proportion as these corresponding streamers proceed from points more remote from the sun's equator, they will intersect under a smaller angle, and their point of intersection will be more distant from the sun's surface; until at the heliographic latitude of 30° to 35°, they will become parallel to the plane of the equator. Those emanating from still higher latitudes will diverge from the plane of the equator and from each other.*

If these facts be attentively considered it will be seen that the result should be the formation of a luminous appearance extending indefinitely outward from the sun into space, and elongated in the plane of his equator; and that to observers on the earth it would have an apparent form more or less triangular.

The same fundamental conception which accounts for the solar corona, and the physical relations known to subsist between the sun's spots and terrestrial auroras, as well as between these spots and the varied magnetic disturbances occurring on the earth, furnishes then an adequate explanation of the extent, form, and position of the *Zodiacal light*. In fact, we see that the zodiacal light is but the indefinite extension of the corona.

We have here tacitly supposed that the solar emanations consist of magnetic matter projected with great velocity into space, in the directions of the prolongations of the auroral columns, and proceeding on indefinitely in these directions; but if, like the cometic matter, they are exposed to a continual repulsion from the sun, the paths described by the receding particles would be hyperbolas convex toward the sun. The point of intersection of any two streamers proceeding from corresponding low latitudes in the two hemispheres, would in consequence be thrown to a greater distance from the sun, but the general result as to the form and position of the luminous appearance produced (the zodiacal light) would be the same.

In support of this view of the origin of the zodiacal light, we may state that Cassini drew from his observations on the sun's spots and the zodiacal light, made during the interval from 1665 to 1688, the conclusion that a physical connection subsisted between these two phenomena, and that the substance of the zodiacal light was, in fact, some emanation from the sun's spots. Again, according to Arago, it appears from the entire series of observations at Paris and Geneva, that the zodiacal light varies considerably from one year to another, and that the observed variations cannot result entirely from changes in the transparency of the atmosphere. We shall soon see that the form of the corona, as seen in the eclipse of 1869, and previous eclipses, presented certain prominent features that accord with the theoretical conclusion that the zodiacal light is but the indefinite extension of the corona.

It is proper to state here, that in what precedes we have really been contemplating but different sides of one comprehensive theory, which embraces a connected series of solar phenomena, of which the corona is but one term.

* According to the recent spectroscopic determinations of Lockyer and Frankland, the solar atmosphere, must be of exceeding tenuity in the region of the rose-coloured protuberances just above the general surface of the chromosphere.

* It is here assumed that the magnetic equator of the sun is coincident with his heliographic equator.

The outline of this theory is given in the author's "Treatise on Astronomy," revised edition (1867). It is that a portion of the matter of the sun's photosphere is in the habitual condition of auroral magnetic columns; that by electric discharges along these columns, their substance becomes dispersed and in part projected into space; and that this process, wherever occurring, may by a continued dissipation of a portion of the photospheric matter at that locality, eventuate in the formation of a visible spot on the disc; that the photospheric matter thus discharged into space is in that peculiar condition recognised in cometic matter in which it becomes subject to a repulsive action from the sun (or else to a diminished attractive action, as occurred to a certain extent in the case of Donati's comet, and in that of 1861), and in the act of flowing away is visible in solar eclipses as the streamers of the corona, and at more remote distances as the zodiacal light; that these solar emanations furnish the matter of terrestrial auroras, and when descending in copious showers into the earth's atmosphere, and developing electric currents and disturbing the magnetic condition of the earth, are the determining cause of all the phenomena of "magnetic storms." The apparent structure, and variability of the corona, and, as we shall soon see, the most characteristic features of its form, the form, position, and variability of the zodiacal light, the coincidence of the periods of the sun's spots with the periods of terrestrial auroras, and with those of the perturbations of the magnetic needle, all accord with this general theory. I have also endeavoured to show in my papers on the variations of the magnetic elements, that these variations are such as should naturally result from the electric currents in the upper atmosphere (or what may be called the photosphere of the earth), that would ensue from the reception of the supposed impulsive waves, and material emanations proceeding from the sun. (See *Am. Journ. Sci.*, II., vol. xix., March and July, 1855).

If it indeed be true, that from the fundamental conception of material emanations from the sun similar to those which we know to take place from the head of a comet under the influence of the sun, a connected series of phenomena may be theoretically deduced which have their actual counterparts in Nature, it must be conceded that there is a high probability that this conception is founded in truth, and furnishes the true explanation of the varied phenomena observed.—*American Journal of Science.*

EXAMPLES OF THE PERFORMANCE OF THE ELECTRO-MAGNETIC ENGINE.*

By J. P. JOULE, D.C.L., F.R.S.

SOME experiments and conclusions I arrived at a quarter of a century ago having been recently criticised, I have thought it might be useful to place the subject of work in connection with electro-magnetism in a different and I hope clearer form than that in which I have hitherto placed it. The numbers given below are derived from recent experiments.

Suppose an electro-magnetic engine to be furnished with fixed permanent steel magnets, and a bar of iron made to revolve between the poles of the steel magnets by reversing the current in its coil of wire. Such an arrangement is, perhaps, the most efficient, as it is the most simple, form of the apparatus. In considering it, we will first suppose the battery to consist of five large Daniell's cells in series, so large that their resistance may be neglected. We will also suppose that the coil of wire on the revolving bar is made of a copper wire 389 feet long, and 1-16th of an inch diameter, or offering a resistance equal to one BA unit. Then, on connecting the terminals of this wire with the battery, and keeping the engine still, the current through the wire will be such as, with a horizontal force of earth's magnetism 3·678, would be able to deflect the small needle of a galvanometer furnished with a single circle of one foot diameter, to the angle of 54° 23′. Also this current going through the above wire for one hour will evolve heat that could raise 110·66 lbs. of water 1°, a quantity equal to 85,430 ft.-lbs. of work. In the meantime the zinc consumed in the battery will be 535·25 grains. Hence the work due to each grain of zinc is 159·6 ft.-lbs., and heat 0·20674 of a unit.

I. In the condition of the engine being kept still we have therefore, current being 1·396, as shown by a deflection of 54° 23′—

(1). Heat evolved per hour by the wire, 110·66 units.
(2). Consumption of zinc per hour, 535·25 grains.
(3). Heat due to 535·25 grains, 110·66 units.
(4). Therefore the work per hour will be $(110{\cdot}66 - 110{\cdot}66)\ 772 = 0$.
(5). And the work per grain of zinc will be $\frac{0}{535{\cdot}25} = 0$.

II. If the engine be now started and kept by a proper load to a velocity which reduces the current to $\frac{6}{7}$, or 0·9307, indicated by deflection 42° 57′, we shall have—

(1). Heat evolved per hour by the wire, $110{\cdot}66 \times \{\frac{6}{7}\}^2 = 49{\cdot}18$ units.
(2). Consumption of zinc per hour, $535{\cdot}25 \times \frac{6}{7} = 356{\cdot}83$ grains.
(3). Heat due to 356·83 grains, $110{\cdot}66 \times \frac{6}{7} = 73{\cdot}77$ units.
(4). Therefore the work per hour will be $(73{\cdot}77 - 49{\cdot}18)\ 772 = 18983$ ft.-lbs.
(5). And the work per grain of zinc will be $\frac{18983}{356{\cdot}83} = 53{\cdot}2$, or $\frac{6}{7}$ of the maximum.

III. If the load be lessened until the current is reduced to $\frac{1}{2}$ of the original amount, or to 0·698, we shall have—

(1). Heat evolved per hour by the wire $110{\cdot}66 \times (\frac{1}{2})^2 = 27{\cdot}665$ units.
(2). Consumption of zinc per hour, $535{\cdot}25 \times \frac{1}{2} = 267{\cdot}62$ grains.
(3). Heat due to 267·62 grains $110{\cdot}66 \times \frac{1}{2} = 55{\cdot}33$.
(4). Therefore the work per hour will be $(55{\cdot}33 - 27{\cdot}665)\ 772 = 21357$.
(5). And the work per grain of zinc will be $\frac{21357}{267{\cdot}62} = 79{\cdot}8$, or $\frac{1}{2}$ of the maximum duty.

IV. If the load be still further reduced and velocity increased so as to bring down the current to $\frac{1}{3}$ of what it was when the engine was still, or to 0·4653, shown by a deflection of the galvanometer of 24° 57′, we shall have—

(1). Heat evolved per hour by the wire $110{\cdot}66 \times (\frac{1}{3})^2 = 12{\cdot}294$ units.
(2). Consumption of zinc per hour, $535{\cdot}25 \times \frac{1}{3} = 178{\cdot}42$ grains.
(3). Heat due to 178·42 grains, $110{\cdot}66 \times \frac{1}{3} = 36{\cdot}89$ units.
(4). Therefore the work per hour will be $(36{\cdot}89 - 12{\cdot}294)\ 772 = 18988$ ft.-lbs.
(5). And the work per grain of zinc will be $\frac{18988}{178{\cdot}42} = 106{\cdot}4$, or $\frac{1}{3}$ of the maximum duty.

V. Remove the load still further until the velocity increases so much that the current is brought down to $\frac{1}{100}$th of its quantity when the engine is still. Then we shall have—

(1). Heat evolved per hour by the wire, $110{\cdot}66 \times (\frac{1}{100})^2 = 0{\cdot}011066$ of a unit.
(2). Consumption of zinc per hour, $535{\cdot}25 \times \frac{1}{100} = 5{\cdot}3525$ grains.
(3). Heat due to 5·3525 grains of zinc, $110{\cdot}66 \times \frac{1}{100} = 1{\cdot}1066$ units.
(4). Therefore the work per hour will be $(1{\cdot}1066 - 0{\cdot}011066)\ 772 = 845{\cdot}73$ ft.-lbs.

* Read before the Manchester Literary and Philosophical Society, March 21st, 1871.

(5). And the work per grain of zinc will be $\frac{845{\cdot}73}{5{\cdot}352} = 815$, or $\frac{99}{100}$ of the maximum duty.

When the velocity increases so that the current vanishes the duty = 159·6.

I. Let us now improve the engine by giving it a coil of four times the conductivity, which will be done by using a copper wire 389 feet long and $\frac{1}{8}$th of an inch diameter, the same battery being used as before. Then when the engine is kept still we shall have a current 1·396 × 4 = 5·584, shown by a deflection of 79° 51′. Then we shall have—

(1). Heat evolved per hour by the wire, 110·66 × $\frac{4}{4}^2$ = 442·64 units.
(2). Consumption of zinc per hour 535·25 × 4 = 2141 grains.
(3). Heat due to 2141 grains, 442·64 units.
(4). Therefore the work per hour will be (442·64 − 442·64) 772 = 0.
(5). And the work per grain of zinc will be $\frac{0}{2141} = 0$.

II. Start the engine with such a load as shall reduce the current to $\frac{3}{4}$, or to 3·7227 (74° 58′), then we shall have—

(1). Heat evolved per hour by the wire, 442·64 × $(\frac{3}{4})^2$ = 196·73 units.
(2). Consumption of zinc per hour, 2141 × $\frac{3}{4}$ = 1427·3 grains.
(3). Heat due to 1427·3 grains, 442·64 × $\frac{3}{4}$ = 295·09 units.
(4). Therefore the work per hour will be (295·09 − 196·73) 772 = 75934.
(5). And the work per grain of zinc will be $\frac{75934}{1427{\cdot}3} = 53{\cdot}2$ or $\frac{1}{4}$ of the maximum duty.

III. Lesson the load so that the velocity of the engine is increased until the current is reduced to one-half its original amount, or 2·792 shown on the galvanometer by a deflection of 70° 18′. Then we shall have—

(1). Heat evolved per hour by the wire, 442·64 × $(\frac{1}{2})^2$ = 110·66 units.
(2). Consumption of zinc per hour, 2141 × $\frac{1}{2}$ = 1070·5 grains.
(3). Heat due to 1070·5 grains, 442·64 × $\frac{1}{2}$ = 221·32 units.
(4). Therefore the work per hour will be (221·32 − 110·66) 772 = 85430 ft.-lbs.
(5). And the work per grain of zinc will be $\frac{85429}{1070{\cdot}5} = 79{\cdot}8$, or $\frac{1}{2}$ the maximum duty.

IV. Let the load be further reduced until the velocity reduces the current to $\frac{1}{8}$, or to 1·8613 shown by a deflection of 61° 45′. Then we shall have—

(1). Heat evolved per hour by the wire, 442·64 × $(\frac{1}{8})^2$ = 49·182 units.
(2). Consumption of zinc per hour, 2141 × $\frac{1}{8}$ = 713·66 grains.
(3). Heat due to 713·66 grains of zinc, 442·64 × $\frac{1}{8}$ = 147·55 units.
(4). Therefore the work per hour will be (147·55 = 49·182) 772 = 75940 ft.-lbs.
(5). And the work per grain of zinc will be $\frac{75940}{713{\cdot}66} = 106{\cdot}4$, or $\frac{3}{8}$ of the maximum.

V. Let the load be still further reduced until, with the increased velocity, the current becomes reduced to $\frac{1}{100}$, or to 0·05584 showing a deflection of 3° 12′. Then we shall have—

(1). Heat evolved per hour by the wire, 442·64 × $(\frac{1}{100})^2$ = 0·044264 of a unit.
(2). Consumption of zinc per hour, 2141 × $\frac{1}{100}$ = 21·41 grains.
(3). Heat due to 21·41 grains of zinc, 442·64 × $\frac{1}{100}$ = 4·4264 units.
(4). Therefore the work per hour will be (4·4264 − 0·04426) 772 = 3383 ft.-lbs.
(5). And the work per grain of zinc will be $\frac{3383}{21{\cdot}41} = 158$, or $\frac{99}{100}$ of the maximum duty.

Now suppose that we still further improve our engine by making the stationary magnets twice as powerful. In this case all the figures will remain exactly the same as before, the only difference being that the engine will only require to go at half the velocity in order to reduce the current to the same fraction of its first quantity. The attraction will be doubled, but the velocity being halved no change will take place in the amount of work given out.

In all cases the maximum amount of work per hour is obtained when the engine is going at such a velocity as reduces the current to one-half of its amount when the engine is held stationary; and in this case the duty per grain of zinc is one-half of the theoretical maximum.

The same principles apply equally well when, instead of employing the machine as an engine evolving work, we do work on it by forcibly reversing the direction of its motion. Suppose, for instance, we urge it with this reverse velocity until the quantity of current is quadrupled or becomes 22·336 indicated by a deflection of 87° 26′. Then we shall have—

(1). Heat evolved per hour by the wire, 442·64 × 4^2 = 7082·2 units.
(2). Consumption of zinc per hour, 2141 × 4 = 8564 grains.
(3). Heat due to 8564 grains of zinc, 442·64 × 4 = 1770·56 units.
(4). Therefore the work per hour will be (1770·56 − 7082·2) 772 = − 4100432 ft.-lbs.
(5). And the work per grain of zinc will be $\frac{-4100432}{8564} = -478{\cdot}8$ or − three times the maximum working duty.

The principal reason why there has been greater scope for the improvement of the steam engine than for the electro-magnetic engine arises from the circumstance that in the formula $\frac{a-b}{a}$, applied to the steam engine by Thompson, in which a and b are the highest and lowest temperatures, these values are limited by practical difficulties. For a cannot be easily be taken above 459° + 374° = 833 from absolute zero, since that temperature gives 12·425 atmospheres of pressure, nor can b be readily taken at less than the atmospheric temperature or 459° + 60° = 519°. Also there is much difficulty in preventing the escape of heat; whereas the insulation of electricity presents no difficulty.

I had arrived at the theory of the electro-magnetic engine in 1840, in which year I published a paper in the fourth volume of "Sturgeon's Annals," demonstrating that there is "no variation in economy, whatever the arrangement of the conducting metal, or whatever the size of the battery." The experiments of that paper indicate 36 foot-lbs. as the maximum duty for a grain of zinc in a Wollaston battery. Multiplying this by four to bring it to the intensity of a Daniell's battery we obtain 144 foot-lbs. Here, as in the experiments in the paper on "Mechanical Powers of Electro-Magnetism, Steam, and Horses," the actual duty is less than the theoretic; which is owing partly to the pulsatory nature of the current, and partly also to induced currents giving out heat in the substance of the iron cores of the electro-magnets; although these last were obviated as far as possible by using annealed tubes with slits down their sides.

SPECTRUM ANALYSIS
IN ITS APPLICATION TO THE BESSEMER PROCESS FOR THE MANUFACTURE OF STEEL.*

By Professor ROSCOE, F.R.S.

IT not unfrequently happens that scientific discoveries, the results of which are apparently far removed from our everyday wants, and are apparently of purely theoretical interest, become all at once of practical value and importance. We chemists, who are occupied with investigating the properties of bodies, and preparing new ones, if we are not satisfied, as indeed we ought to be, with the study of science for its own sake, may say, at any rate, that we have another field of interest, inasmuch as a body which may be discovered to-day, and which for a time remains a chemical rarity, useful and seen only in the collections of the scientific chemists, may become an object of great pecuniary value as applied to the arts and manufactures, or may become, as in the case of chloroform, an alleviator of the suffering of mankind. I have this evening to bring before you an application of this kind, which, if it is not so important as many, yet I think we shall all admit is one of the greatest interest. It is the application of the newly-discovered principles of spectrum analysis to the steel manufacture. It would be presumptuous in me on this occasion, and before such an audience as I have the honour of addressing, to enter into details on the subject of the Bessemer process. Still less would it become me to extol, as I might otherwise do, the genius and the persevering energy of the discoverer of this process. I think, however, that you will all agree with me in this,—that whether we look at it from an economical point of view, or from a chemical point of view, or from a mechanical point of view, the Bessemer process is one which has not its equal, either in modern or in ancient metallurgy, and that it has called forth a new era which I think is now about to be accomplished in the manufacture of our most important metals, iron and steel.

It is, then, to the application of the scientific principles of spectrum analysis to the manufacture of steel by the Bessemer process that I have to draw your attention; and I will begin by pointing out to you, as shortly as I may be able, the chief principles which regulate this most interesting branch of science, and I will afterwards point out how these principles affect the particular process under examination.

In the first place, then, so long ago as the year 1675, Sir Isaac Newton discovered that white light is the compound, whereas coloured light is the simple, phenomenon. He took a triangular piece of glass called a prism, and allowed the light shining through a hole in a shutter to pass through it, and to be received on a screen, and he found that the white sun-light was split up into a many-coloured band, which we now know so well as the solar spectrum. He came to the conclusion that the light of the sun consists of rays differently refrangible. He then performed the reverse operation; the coloured band which he thus obtained by passing the sun-light through the prism he looked at by means of another prism placed in the same direction. The coloured rays were then brought again to a focus, through the prism, in his eye; all the differently coloured light was united again, and produced in his eye the impression of white light; and he saw, as it were, a round hole of white light at that point. Kirchhoff, to whom we are indebted for an immense amount of knowledge, and for the discovery of the composition of the sun's atmosphere, instead of using one prism, as Newton did, uses four prisms, and, instead of looking at the spectrum with the naked eye, he views it through a telescope; but the most important alteration since Newton's time was that, instead of allowing the light to pass through a round hole, as Newton did, and so forming a round beam, Kirchhoff, and many others before him, especially Fraünhofer and Wollaston, took a narrow line of light. They opened a very narrow slit, and allowed the light to fall through it, so that the line of white light fell upon the first prism, and was bent out of its course as it were, and thus resolved into its ultimate parts—resolved into all the differently coloured rays, beginning with red, and ending with violet.

Although I cannot show you the solar spectrum, I can show you what will answer our purpose. I will show you the spectrum of the white carbon points of the electric lamp, for we have an arrangement by which we can throw the beam of light through a set of prisms, and instead of receiving it as a white light upon the screen, we receive a beautifully coloured band of light, in which we pass from red through all the gradations of orange, yellow, green, and blue, into the most refrangible rays, violet. But when we come to examine sun-light, we find that the sun-light possesses certain characters which the light from these white-hot carbon points does not possess, and to that peculiarity of sun-light I would, for a moment, draw your attention. What I wish to call your attention to, is the presence of these dark lines which intersect the coloured riband. These dark lines are always found in the sun-light, and they have received the name of Fraünhofer's lines, because they were first properly mapped by the German optician, Fraünhofer.

These lines have, since Kirchhoff's discovery about ten years ago, assumed a very great amount of interest, inasmuch as that discovery depended on mapping carefully and observing the positions of these dark lines which occur in all sun-light, whether we take direct sun-light, or reflected sun-light, or planet light (which is, in fact, reflected sun-light), or moon-light. These lines become of the greatest importance, because it is by their presence that we are able to tell anything whatever about the composition of the solar atmosphere.

It is owing to the position of these dark lines in the sun's spectrum coinciding with that of certain bright lines produced by the metals, that we are able not only to discover the composition of the solar atmosphere, but, in a similar way, the composition of the atmosphere of the fixed stars which possess lines of a different character altogether.

Now let us examine in another way the beam of coloured light, and note the character and properties of the light in the different portions of the solar spectrum. Imagine that you have before you the solar spectrum intersected as it would be by fine dark lines. We shall find that the different portions of this coloured band possess very different properties. Those rays which produce the effect which we term heat are situated almost altogether at the red end, and even beyond the red portion of the spectrum, so that the maximum of heating effect is situated there. The maximum of luminous effect is more in the centre, and the heating rays gradually diminish until they sink to an almost inappreciable quantity at the blue end. But as we pass along from red, through the orange, green, and blue, we find that the rays begin to assume a different character; that is to say, they are capable of producing different kinds of action—different kinds of work. When we come into the blue rays we find that we have light which is capable of producing chemical action. This fact has been known for many years; hence it has been customary to divide rays of light into the heating, the luminous, and the chemically active rays; and it was at one time believed that there was a total distinction between these three different kinds of light, and that in a particular portion of the spectrum we could separate the heating rays from the light-giving rays, and the light-giving rays from the chemically active rays. Such, however, is not really the case. We have no more power of distinguishing between the chemically active rays and the luminous rays than we have of distinguishing between the yellow light

* A Lecture delivered before the Iron and Steel Institute, on Wednesday evening, March 29th, 1871.

and the green light. They simply differ in wave length and in power of refrangibility. But although we cannot separate out, from any given portion, the light-giving rays from the heating rays, or the chemically active rays from the light-giving rays, yet I can show you by a singularly beautiful experiment that the maximum heating effect is at the red end. If I cut away the whole of this light by means of a screen, and allow only rays which are less refrangible to pass through—that is to say, rays which to the ordinary human eye are invisible—then we shall have the maximum of heating effect. This I can show you by a simple experiment which I have arranged here. I have in this dark box an electric lamp connected with the battery with which we have been working; and in front of this box I have what Dr. Tyndall—(for this experiment is his)—very appropriately calls a "ray-filter." It is a solution which is so dark that it will let no rays of light through; but it lets through the rays of heat, so that I can actually in the focus of these dark rays inflame a piece of black paper in perfectly dark space. There; you see I have got the paper burning in the focus of the dark rays. I could in this way light a cigar if I happened to have one. You see, too, that I can set fire to some gunpowder by the same means. This, then, shows us that beyond the red portion of the spectrum the rays of heat predominate.

Now allow me to prove to you what we have at the other end of the spectrum, where we have, as I said before, the chemically active rays present. I have in my hand a little glass bulb which is filled with a mixture of chlorine and hydrogen gases. These gases have this very remarkable property—that when they are exposed to the chemically active rays they combine. If I were to throw this little bulb out into the daylight, it would explode before it fell to the ground in consequence of the hydrogen and chlorine combining together under the action of light, or under the action, rather, of a particular portion of the solar rays. The gases, combining, will form hydrochloric acid gas. I have here a little lantern which contains differently coloured glasses: one is blue, another is red, and another is yellow; and the glass of one side is colourless. I am going to expose this little bulb inside my lantern, first to light passing through the red glass, and then to light passing through the yellow glass. The faint light of the ordinary gas of the room does not explode my bulb. I must cover up that portion of the lantern which consists of white glass, otherwise I shall get an explosion of the bulb before I want it. At present I leave only the red side visible. Now I am going to produce a bright flash of light which will be violet light, and contain a large quantity of the chemically active rays; but these chemically active rays will be filtered off by the red glass, and will not produce any explosion of the bulb. [The experiment was performed.] Now I am going to do the very same thing again; but I will turn the blue side of the lantern to the light and produce another bright flash, and then you will hear an explosion produced by the combination of the gases in the bulb. [The experiment was performed accordingly.] There, you see, the explosion was sufficient to indicate what I want, and we have our bulb smashed into fine powder.

So much, then, for the general side of the question—the general arrangement of the rays of light in the solar spectrum, and for their general distribution.

Now I will show you what kind of material it is which gives us this bright coloured band, varying from red to violet. We shall throw on the screen here an image of the carbon poles which are giving us this light, and you will see that these are white-hot incandescent solid substances; and, if I had made the poles of any other solid body which I could heat white-hot without that substance giving off any vapour, I should find that I should produce the same effect, namely, the effect of a continuous spectrum. You see that we can separate these poles for a little distance, but I cannot separate them for any great distance. We have now focussed them, and you see the beautiful image of them. We can separate them to a little way without the electricity ceasing to pass. They are white-hot and ignited by the passage of the current. Now, if you observe them, you will see that there is an arc of light between the two. This arc of light consists of glowing gas. Glowing solids and glowing liquids all give off a continuous spectrum, whilst glowing gases give off a discontinuous spectrum; that is to say, glowing gases have the power of what we term *selective radiation* they have the power of giving off a certain kind of light. We will now place on these poles a small quantity of a metal which has recently been discovered—the metal thallium—and you will see that we shall be able to volatilise the thallium; and I will ask you to observe that when it is placed on the poles we shall have a very brilliant arc—an arc of ignited thallium vapour. Now here we have the thallium. Look at this glorious stream of green light! It is due to incandescent thallium in the form of gas, and I can separate the poles to a very great distance, you observe, and still get this bright green band following, as it were, and carrying the electricity; and in that act of carrying the electricity the thallium is converted into vapour and giving off this particular green light. Now, if we come to examine this light by means of a prism, as we will do in a moment, you will see that thallium gives a totally different spectrum from that of the glowing carbon poles. [The prism was applied.] Now, observe the green band. We observe here the green arc and the green band at the same moment. If we narrow the slit a little, we shall get a still more beautiful scene. Now the whole of this spectrum analysis depends upon the fact that glowing gases have this power of selective radiation—the power of giving off bright bands instead of continuous spectra. No other substance that we know of gives this same green light. I am obliged to throw this light on the screen by means of a rough sort of apparatus, as it is necessary to get an intense light to enable you to see it properly; but, if instead of that, I looked at the thallium spectrum with one of those beautiful spectroscopes which Mr. Browning has placed on the table, I should see this green line reduced to the finest dimensions—as fine as the finest spider's web. We know that no other body gives this green line in exactly the same position.

Now, in the same way, we may go through a large list of metals. The next spectrum which we will throw on the screen is that of the metal copper. We have the power here of volatilising the copper, and we shall observe the kind of light which the copper gives off. Here you see the bright green bands due to copper. No other substance that we know of gives these bright green lines, and these other violet lines which are less distinct, though not less characteristic of the metal copper. You will readily imagine what a wide field spectrum analysis covers, inasmuch as if, in any distant star or source of light, we observe these particular green bands, we infer with truth that that light is due to the vapour of glowing copper. Thus, if we were to see these bright lines in the sunlight, or in the light of any other star, we should come to the conclusion that that far distant luminary contained copper. I will next show you, just as an example, the spectrum of silver; and in this case I will use a silver pole. We shall find that the silver pole becomes molten at the top, and that then the silver boils and is converted into vapour, and we get the peculiar light which the silver emits; and you will again notice, even in this very rough way, that the light is different from the light of the copper. In fact, in this instance, we see the copper lines too, for there is a little copper left at the top of the poles. If we keep the poles far apart, we get scarcely any continuous spectrum at all. These are the lines due to the vapour of metallic silver. Now we will use brass, and here we shall have the lines both of copper and zinc visible. Here are the splendid lines due to those metals, for in an alloy such as brass we get indications of the presence of both metals. You will recognise the copper lines, and you will also see the splendid red lines due to zinc. You see that in this

way we can detect the presence in brass of both copper and zinc.

The delicacy of this reaction is something almost beyond belief. I have a table here which will give you an idea of the enormous delicacy of this method :—

DELICACY OF THE SPECTRUM ANALYTICAL METHOD.

1. *Soda.*—1-3,000,000th part of a milligrm., 1-180,000,000th part of a grain, of soda, can be easily detected. Soda is always present in the air. All bodies exposed to air show the yellow soda line. If a book be dusted near the flame the soda reaction will be seen.
2. *Lithia.*—1-100,000th part of a milligrm. of lithia, or 1-6,000,000th part of a grain, can be easily detected. Lithium was only known to occur in four minerals. It is now found, by spectrum analysis, to be one of the most widely distributed elements. It exists in almost all rocks, in (3 cubic inches) sea and river (Thames) water, in the ashes of most plants, in milk, human blood, and muscular tissue.
3. *Strontia.*—6-100,000th part of a milligramme, or 1-1,000,000th part of a grain, of strontia is easily detected, and has been shown to exist in very many limestones of various geological ages.
4. *Lime.*—6-100,000ths of a milligrm., or 1-1,000,000th of a grain, of lime can be easily detected.
5. *Cæsium.*—This new alkaline metal was discovered by Bunsen in the mineral waters of Baden, Durkheim. Its spectrum consists of two bright blue lines. Thirty tons of mineral water yielded 100 grains of cæsium.

We detect the 1-180,000,000th part of a grain of soda, for instance, and the 1-6,000,000th part of a grain of lithium. Some substances which were supposed to be very rare are now found almost everywhere, and by this process no fewer than four elementary bodies have been discovered—viz., rubidium and cæsium, the new alkaline metals discovered by Bunsen; thallium, which was discovered by Mr. Crookes; and indium, which has also lately been discovered by some German chemists.

If we next take a mixture of the various alkaline salts, or of the alkaline earths, we shall get similar coloured bands. It is, however, by means especially of the electric spark that we can investigate these matters. I have here a means of producing a very bright spark, which is due to the passage of electricity between two brass points; and if I were now to look at this bright spark with a delicate apparatus such as is on the table, I should see exactly the lines which I have just very roughly shown to you on the screen; but, instead of seeing only a few lines, I should see many hundreds, the apparatus being more delicate, and the light being thrown directly into the eye. It thus becomes possible to map, with great accuracy, the lines of the different metals; and this was Kirchhoff's great business. He not only mapped the dark lines in the sun, but he also mapped the bright lines of the metals; and it was in this way that he found that every one of the bright lines of certain metals coincided with certain dark lines in the sun. This coincidence was found in the case of iron to the extent of many hundred lines. This observation showed that there was some connection between the bright iron lines produced by the electric spark and some of the dark lines which exist in every form of sunlight. How the lines which are due to iron become black in the sun, instead of being bright, I have not now time to explain. I will now throw on the screen the spectrum of a mixture of the salts of the alkalies and the alkaline earths, to give you an idea of the splendid parti-coloured riband of light which we thus obtain. Here are the spectra of potassium, sodium, lithium, barium, strontium, and calcium.

But it is not merely with the metals that we have to do in spectrum analysis: we have to deal with every form of matter, and therefore we have to deal with gaseous materials. You perceive, therefore, that we have to investigate the question as to what is the nature of the light which gases emit; I mean bodies which are gaseous at the ordinary temperature. I will show you how we can make use of electricity for the purpose of igniting the gases. I have here, for instance, a series of gases—hydrogen, carbonic acid, nitrogen, chlorine, and iodine—and these are contained in very minute quantities sealed up in a glass tube, so that the pressure within the interior of the tube is very nearly that of a vacuum, and yet the tube contains a trace of each of these materials. If we pass a current through the tube, we shall see that each gas glows with a peculiar light of its own. Here the hydrogen has a red tint, the iodine a green, and the chlorine a light blue colour; and when we come to examine these substances also by the spectroscope, we shall find that they emit a light of a perfectly peculiar character, and that just as well can we recognise hydrogen by its spectrum, when we get it in the state of ignition by means of the electric current, as we can recognise the spectra, as I have shown you, of the metals proper. I might show you, with tubes of other gases, the same principle—viz., that bodies of a gaseous character emit the light, or give off rays of the light which it is their peculiar property to emit. In this other tube you have nitrogen, with a most extraordinary striated mass of ignited rays; and beneath this you have the beautiful phenomenon of fluorescence caused by the presence of uranium glass in this tube. We can investigate the whole range of chemical elementary bodies in this way.

Here I will bring small quantities of substances into the flame. This body thallium has the power of tinging the flame. The splendid band now visible is due to the volatilisation of the metal of one of the alkaline earths. We can examine these bands not merely with the electric light, but, if we want to make the examination on a small scale, we make beads of the metals, and bring them into a flame. For instance, here I have a thallium salt, which you see tinges the flame green, giving off the peculiar green tint which is characteristic of thallium. Salts of the new metal, rubidium, have the power of volatilising and of tinging the flame of a splendid purple colour. This purple tint is characteristic only of the metal rubidium —that is to say, the lines which it gives off are peculiar to that metal. Here we have the violet light which is produced by potassium contained in the well-known substance, potash; and here, again, we have strontium, which gives a beautiful red light. We can readily obtain the spectra of most of these coloured flames.

(To be continued.)

ABSOLUTE TEMPERATURE.

By the Rev. H. HIGHTON, M.A.

1. THE doctrine of the Mechanical Equivalence of Heat has involved the necessity of imagining an absolute zero of temperature—that is, a point below which cold cannot go, and at which there is an absolute deprivation of all heat.

2. This absolute zero is arrived at in the following way:—It is said that air for every degree of temperature loses $\frac{1}{274}$th part of the volume it had at the ordinary freezing point of water; and, therefore, that at the temperature $-274°$, it will cease to have any volume at all, or, as it is more euphoniously expressed, in order to avoid the obvious absurdity, it would cease to have any elastic force. Hence, it is gravely assumed in our fashionable modern books of science, that $-274°$ is the deprivation of all heat, and that the amount of absolute heat in a body is measured by its temperature above $-274°$. Thus it is proved that a pound of water at ordinary temperatures contains within itself heat capable of raising it at least 80 miles. Well may Tait, in his "Thermodynamics" (p. 21), call this one of the most extraordinary results of physical science!

3. But, unfortunately, if we take water as our standard, absolute zero would be totally different. Applying the same arguments to water, we might, if we reasoned from certain temperatures above the point of maximum density, make absolute zero of temperature about −2800°; and from others below the maximum density, the zero of cold beyond which heat could not be raised would be +6450°! Iron, mercury, and other bodies, would each give different zeros. The fact is, it is assumed that the same, or nearly the same, rule which applies to gases at some temperatures applies to them at all, and that they could never be liquefied.

4. To suppose an absolute zero of temperature appears to me as utterly absurd as to suppose that negative electricity could go on increasing in intensity till no positive electricity at all was left and there was an absolute zero of electricity. Heat, like electricity and motion, is relative, not absolute. It is positive, when it bears a certain relation to the state of things around it; negative, when it bears an opposite relation. Of absolute electricity we know nothing; but we assume, as our starting point, a state of equilibrium in neighbouring matter. So of motion; till we can find a point of absolute rest, we know only nothing of absolute, but only of relative motion. So in heat; our true starting point of zero can only be the average temperature of the universe, if ever that can be ascertained. Heat, motion, and electricity are all dual, not single forces; and it is from overlooking this duality of heat that all the modern errors on the subject have sprung. It is not so many years since, that when Mayer first propounded the theory of the Mechanical Equivalence of Heat, the opposition of the physicists of that day, and the difficulty he found in getting any scientific publication to print his views, actually drove him out of his senses. I can plainly see that the doctrine will be far more easily overthrown than it was established. I observe that your contemporary, the *Popular Science Review*, gives a verdict that my articles in the Chemical News have proved my point; and that the April number of the *Scientific Review* supports the case by an experiment of the simplest and most conclusive kind.

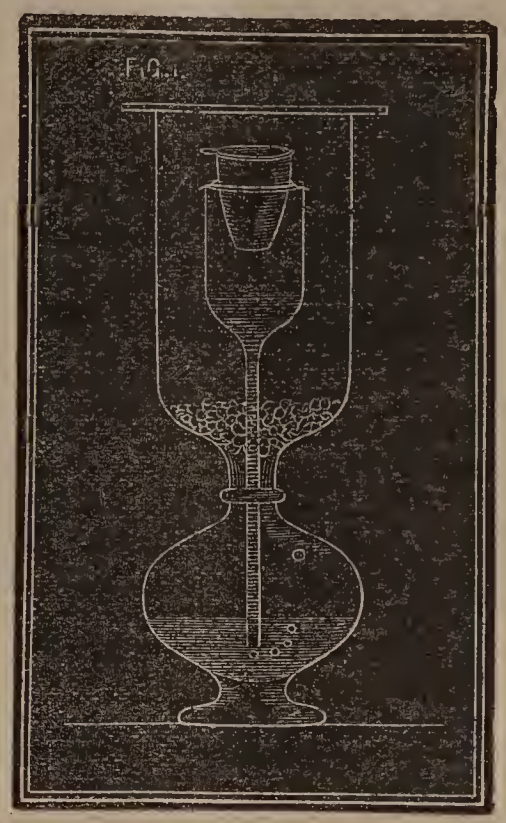
LABORATORY APPLIANCES.

By Professor LEEDS.*

New Form of Desiccator.—The desiccator in most common use, as is well known, consists of a low broad glass bottle, the outside of which is fitted by a ground glass joint to a glass cover. The inside is partly filled with fused calcic chloride, and the platinum crucible is supported either upon a glass tripod or upon a platinum triangle. It has the objection that the air in the interior expands when the hot crucible is introduced, and lifting the lid makes it escape. On cooling, a partial vacuum is sometimes produced which renders it difficult to remove the lid. To obviate this difficulty, a hole is frequently drilled in the cover. But in doing so it makes the ground glass joint of little benefit, and the apparatus thus arranged is little if any better than a bell-jar set upon a ground glass plate.

Schrötter's desiccator provides for the free egress of air in contact with the hot crucible. It consists of a bell glass placed upon a glass plate and containing the crucible. Through a hole in a cork fitted to the tubulure of the bell-jar a fine glass tube is passed to allow an escape of the heated air. This first tube is enclosed in a second tube, somewhat wider, closed at its upper end, and with holes drilled in the lower end, through which the air passes and bubbles up through sulphuric acid contained in the bottom of a third tube that includes the other two. The top of this outer tube terminates in a bulb opening upward, which is filled with calcic chloride. When a partial vacuum is produced in the interior, the air is made to pass through the sulphuric acid before re-entering and thus dried.

A more compact and convenient desiccator is represented in the woodcut. It consists of the base of an alcohol lamp, which is ground into the tubulure of a small bell-jar. The base is partly filled with sulphuric acid, and has a hole drilled in the side near the top to permit the egress of air. The bottom of the bell-jar is covered with fused calcic chloride and the top by a ground glass plate. One of the two discs which are used to exhibit the cohesion between polished glass surfaces answers nicely. Through the tubulure a funnel with a very large bulb is passed, its end dipping somewhat beneath the



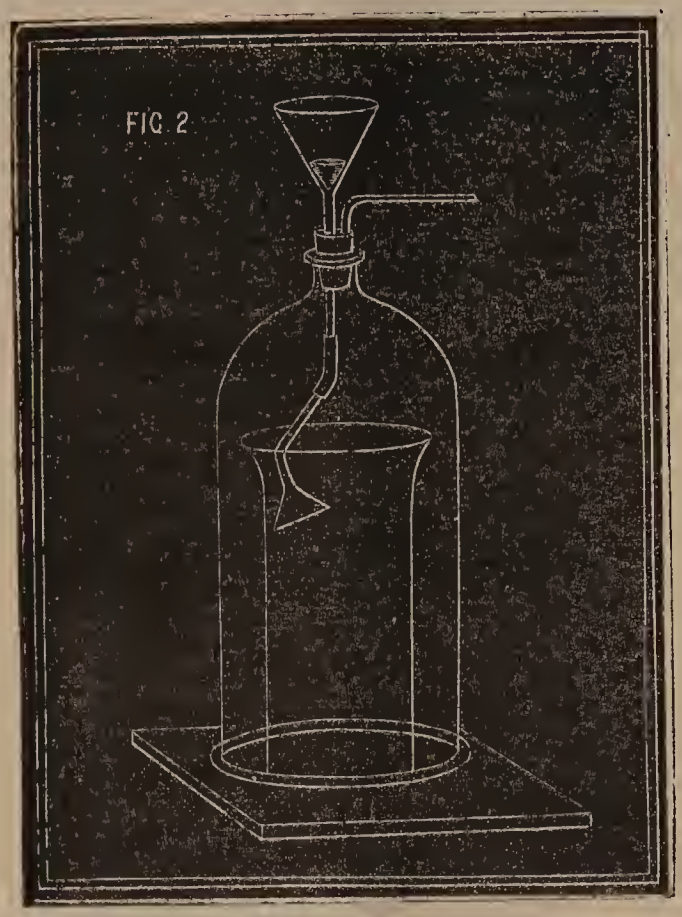
surface of sulphuric acid. The platinum crucible is supported in the mouth of the funnel by a triangle. When the air is heated by the introduction of the crucible it bubbles up through the acid and makes its escape by the opening at the side. On cooling, the air forces the acid through the tube and partly fills the funnel, but as soon as the surface of the acid sinks below the end of the tube, air bubbles up through the long column of sulphuric acid until equilibrium is restored. The calcic chloride insures complete drying. It is hardly needful to say that the tube of the funnel is supported in the tubulure by an air-tight joint.

Water Air-Pump.—When the water air-pump is used in connection with a bell-jar and beaker, such as I have before described in the *Journal of the Franklin Institute*, a difficulty is encountered arising from the bursting of air-bubbles contained in the water, so soon as the latter finds itself in a vacuum. In order to obviate this difficulty a number of expedients were resorted to, the one figured in the woodcut proving most satisfactory. To the end of the funnel tube, which is passed through the tubulure of the bell-jar, a very small funnel is connected by a short piece of india-rubber tubing. The ends of the funnel tubes are ground so as to bring them as near as may be in contact. They may be fused, but this not only requires careful workmanship, but was found to be an arrangement very liable to accidents from the rigidity of the parts. The tube of the little funnel is bent in such a way as to bring one of its sides nearly vertical and parallel to the inside of the beaker against which it rests. When now the bubble expands it finds itself hemmed in by the walls of the funnel, and finally breaks quietly and trickles down the vertical side of the funnel until it comes into contact with the beaker.

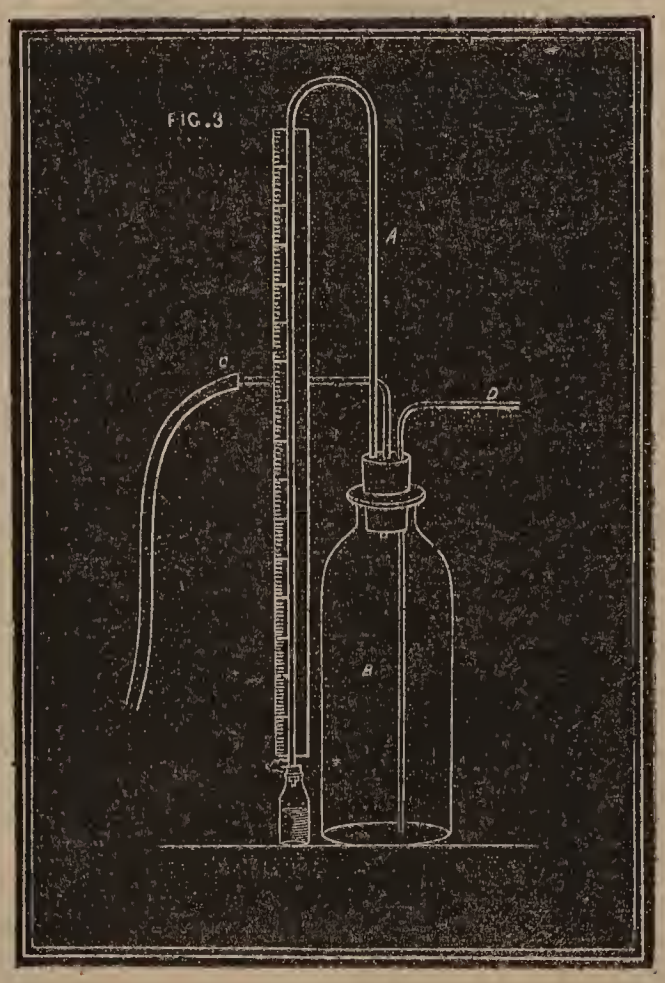
The exhaust of the water air-pump with which I am working at present is somewhat more than 36 feet in length.

*From advance-sheets of the *Journal of the Franklin Institute.* mmunicated by the Editors.

It is not unusual to have the barometric columns in the gauge rising to the height of 78 c.m., and to filter under a pressure of 65 c.m. When the vacuum is allowed to stand, the water supply being shut off, the water remaining in the exhaust tube is frequently lifted and runs back. To prevent it from entering the bell-jar the contrivance re-



presented in the above cut was resorted to. The exhaust pipe, represented by D, enters through the india-rubber cork of a wide-mouthed quart bottle, and passes down almost to its bottom. The tube connecting with the bell-jar to



be exhausted, c, terminates on the inside of the bottle just below the cork, and so likewise does the tube forming the mercury gauge. When the water regurgitates it fills the bottle nearly to the top, but not a drop ever passes over into the bell-jar. On starting a fall of water again through the long exhaust pipe, the water in the bottle is entirely and at once removed by means of the syphon, and the exhaustion of the air follows immediately.

CORRESPONDENCE.

JOULE AND SCORESBY ON THE MECHANICAL VALUE OF ELECTRO-MAGNETISM, STEAM, AND HORSES.

To the Editor of the Chemical News.

Sir,—You will perceive by the *Proceedings of the Manchester, Literary, and Philosophical Society* that, in consequence of the criticism in the Chemical News, Dr. Joule has withdrawn the details of his theory, and substituted others.

I have now tested, by facts and experiments, his new theory, and find it as untenable as his old one.

I hope shortly to show this in detail, and I feel more and more convinced that facts are inconsistent with the theory of the invariable mechanical equivalence of heat.—I am, &c.,

H. Highton.

Putney, April 10, 1871.

COMMERCIAL ANALYSIS.

To the Editor of the Chemical News.

Sir,—Having for some time past been struck with the conflicting results obtained by chemists for commercial analysis, I made the following experiment, and thinking that it may interest some of your readers, I beg to forward you the particulars and results.

A sample of ordinary superphosphate of lime was taken and reduced to a fine state of division, and to ensure greater uniformity, was passed several times through a fine sieve. Seven tins were then filled from the sifted sample, sealed up in the presence of a disinterested witness, and sent to seven different chemists. In due course the following results were obtained:—

Amount of soluble phosphate of lime contained in a sample of superphosphate of lime received from—

I.	II.	III.	IV.	V.	VI.	VII.
25·46	19·48	24·43	20·33	22·74	19·81	24·52

From the above you will see there is a difference of nearly 6 per cent soluble phosphate in the seven different analyses, representing a money value of at least 18s. per ton.

The chemists were all well-known men; three of them respectively Chemists to the Agricultural Societies of England, Scotland, and Ireland. Three of the others men whose analysis is always taken in commercial transactions. In the face of this, how is it possible for manufacturers to guarantee the quality of their goods, and how can the public be expected to have any faith in chemical analysis?

The sample was so carefully prepared, that I have no hesitation in stating that there ought not to have been a difference of more than 1 per cent in any of the analyses.

I am unwilling to suppose carelessness in either case, so can only conclude that different chemists have different methods of determining soluble phosphate of lime, and that all the methods do not give the same result.—I am, &c.,

Edw. Purser, jun.

London, March 30, 1871.

MISCELLANEOUS.

Successor to the Late Dr. P. Bolley, at the Federal Swiss Polytechnic School, at Zürich,—From a communication addressed to us, from Turin, dated 21st March last, we learn with great pleasure that the Council of the School just named has appointed Dr. Emile Kopp as Professor of Technology in the room of the late lamented Dr. Bolley. Professor Kopp's merits, during the time he

has held the Professorship at the Institute Superiore, at Turin, have been gracefully acknowledged by the Italian Government, it having pleased H.M. King Victor Emmanuel to grant to Dr. Kopp the dignity of Commandeur de l'Ordre Equestre de la Couronne d'Italie. We sincerely congratulate Dr. Kopp, and no less so the Government of the Helvetian Republic, for having secured the services of a man so eminently well suited as Dr. Kopp to the important Professorship vacant by the demise of Dr. Bolley.

Natural Science at the Universities.—Of seven open scholarships at Christ College, Cambridge, two for Natural Science have been awarded to Mr. Elwes, of University College, London, and Mr. Jude, of King's College School, London. The scientific teaching in the London colleges is beginning to reap its fruits at the elder universities. In the case of Mr. Jude, his success may be looked upon as the result of throwing open to the pupils of the school the chemical and physical laboratory teaching previously confined to the students of the College, thus solidifying and bringing to a practical test the knowledge acquired in lectures. Should the University of Cambridge carry out the proposal, as there seems every probability of its doing, to admit modern languages in lieu of Greek, the modern departments of schools will soon be rivalling the classical sides in the universities as well as in the general business of life.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 3, 1871.

This number opens with a communication from Dr. A. W. Hofmann, announcing the establishment of an Italian Chemical Society, under the auspices of Dr. Canizzaro. This Society will publish a periodical under the superintendence of MM. Schiff, Tassinari, Körner, Paterno, and Gabba, as joint editors. The above-quoted number contains the following original papers and memoirs:—

Analysis of a Fossil Thigh-Bone of a Child (aged apparently from 5 to 7 years) taken from an Ancient Tumulus near Ohlsdorf (Hamburg).—F. Wibel.—The bone here alluded to belongs to the so-called "bronze period." The results of the analysis are, in 100 parts—Organic matter, 12·52; calcic phosphate, with traces of magnesic phosphate and fluoride of calcium, 74·99; carbonate of lime, 5·48; salts and mineral impurities, 7·01; nitrogen, 1·82.

Formation of Native Azurite (Blue Carbonate of Copper).—F. Wibel and E. Tüngel.—The authors have tested large quantities (25 grms.) of the Siberian mineral for ammonia, in order to see whether the intense blue colour exhibited by azurite may be due to the presence of an ammoniacal compound of copper. The result of repeated experiments in this direction were negative; but, when a solution of sulphate of copper is heated along with lumps of white marble to 200° in a sealed tube, and the green carbonate of copper thus formed left in the tube for several months, the authors observed the formation of an intensely blue-coloured carbonate; while, at the same time, all the water in the tube (although yet sealed) had disappeared, crystals of gypsum being simultaneously deposited. The difficulty of properly separating from each other the two carbonates (green and blue) prevented the making of an analysis, but the surmise is that azurite is formed from malachite by absorption of carbonic acid and loss of water.

Gold from Vancouver Island.—F. Wibel.—From a well-crystallised sample weighing 26¼ grms., a portion was taken for analysis; sp. gr. at 22°, 18·5. The sample contains, in percentages—Gold, 91·86; silver 6·63; copper, 1·0; iron, 0·51; no mercury, lead, nor other metals.

Presence of Sarco-Lactic (Flesh-Lactic) Acid in the Urine of a Patient Suffering from Trichinosis.—Th. Simon and F. Wibel.—This brief notice records the fact that the testing of the urine of a patient alluded to contained that peculiar form of lactic acid which is met with in meat. The authors obtained the zinc salt of the body alluded to, $Zn_2(C_3H_5O_3)_2H_2O$. The presence of this acid in the excreta of a patient suffering from this disease is rather interesting, as indicating the increased activity of oxidation and rapid decomposition going on in the human system, the acid alluded to being the product of the oxidation of fats and albuminous compounds.

Lecture Experiment to Illustrate the Action of Dilute Sulphuric Acid upon Starch.—A. Vogel.—The author says that, since nearly all kinds of writing paper are nowadays so very largely sized with starch that they are coloured blue by a dilute solution of iodine, the following experiment may be made to illustrate visibly the change which starch undergoes when acted upon by dilute sulphuric acid, for which purpose writing or other figures are traced on the paper with very dilute sulphuric acid, and a gentle heat applied, care being taken not to char the paper. When next a dilute solution of iodine is painted over this paper, the portion acted upon by the dilute acid remains white, while the rest becomes blue; but since, in course of time, this blue colour disappears altogether, the same piece of paper may be repeatedly used for this experiment.

Contribution to our Knowledge on the Reaction between Urea and Nitrous Acid in Aqueous Solution.—A. Claus.—This lengthy paper contains an account of the author's researches, bearing upon the true acceptation of the formulæ according to which the reaction alluded to takes place. The paper is, notwithstanding its value for theoretical chemistry, not well suited for useful abstraction.

Decomposition of the so-called Sulpho Ureas by Nitrous Acid.—A. Claus.—This essay is an appendix to the paper just alluded to by the same author. The following sections are contained in this paper:—Action of nitrous acid upon substituted ureas; diphenyl-sulpho urea; sulpho urea; sulphocyanide of ammonium.

Some of the Derivatives from Aceto-Phenon.—A. Emmerling and C. Engler.—This essay is divided into the following chapters:—Secondary ethyl-benzol-alcohol—

$$C_6H_5C\left\{\begin{matrix}H\\OH\end{matrix}\right\}CH_3;$$

chlorethyl-benzol, $C_6H_5.CHCl.CH_3$; synthesis of styrol; bromated aceto-phenons; aceto-bromphenon, $C_6H_4Br.CO.CH_3$; bromaceto-phenon, $C_6H_5.CO.CH_2Br$.

Relation existing between the Chemical Properties of the Gases and their Refractive Power for Light.—F. Mohr.—This valuable physico-chemical treatise is not well suited for any useful abstraction. This observation equally applies to the next treatise by the same author.

Optical Proof of the Nature of Water of Hydration.

Behaviour of Chlorinated Phenols and Nitric Acid.—A. Faust.—This brief notice only refers to a question of priority of discovery made by the author, and published, but ignored by M. P. Weselsky, who has taken up the same subject three years later.

Preliminary Notice.—F. Sajohelyi and M. Ballo.—After briefly referring to the experiments on solidification (really hydration and freezing of that hydrate) by the last-named of the authors, they now state that they have obtained what they call chloroform snow and iodethyl snow, by directing a rapid and moist current of air upon these fluids contained in shallow basins. As regards the real nature of these solid substances, their experiments are not yet quite finished.

Oxypicric Acid (Styphnic Acid).—J. Schreder.—The acid named is identical with trinitro-resorcic acid; it is reduced by the action of tin and hydrochloric adid, yielding readily the crystalline salt—

$$C_6\left\{\begin{matrix}H\\OH\\OH\\NH_2.HCl\\NH_2.HCl\\NH_2.HCl\end{matrix}\right\}+SnCl_2.$$

This salt, treated with sulphuretted hydrogen, yields the hydrochlorate of the corresponding triamido compound, which, in its turn, yields, by the action of oxidising substances, a compound analogous to the azo-triamido-phenol. Several other reactions and formulæ are recorded. The extract of sapan wood, the best material for obtaining oxypicric acid, yields, by fusion with solid caustic potassa, a large quantity of resorcine.

Contribution to the History of Guanidine.—A. Bannow.—This paper treats at length on the preparation of guanidine by means of the action of iodide of cyanogen upon alcoholic solution of ammonia in a sealed tube, and heating in a water-bath.

Preparation of Pure Benzol.—Dr. A. W. Hofmann.—Illustrated with a woodcut, and, moreover, mainly the reproduction in German of Mr. Mansfield's "Researches on Coal-Tar" (*Chemical Society's Quarterly Journal*, vol. i., p. 244).

Decomposition of Ferricyanide of Potassium by Sunlight.—A. Vogel.—This paper bears chiefly on the fact that the decomposition alluded to has been already long known. Among other papers published on this subject, and here quoted, we mention Dr. Schönbein's "Observations on the Sensitiveness of the Ferro- and Ferri-Cyanides of Potassium (Poggendorff's *Annalen*, Bd. 67, 1846).

Peculiar and Improved System of Sewers.—Captain Liernur.—This lengthy essay is, however interesting the subject may be, not well suited for abstraction. In practice, as far as experience can decide, the author's system is a decided improvement, since it saves the use of large quantities of water for carrying off sewage and fœcal matters, and yields a valuable and concentrated manure.

No. 4, 1871.

This number contains the following original papers and memoirs:—

A Base Homologous with Cyanethine.—A. G. Baeyer.—This essay is divided into the following sections:—Chlorocyanmethine,

$C_6H_8ClN_3$; bromocyanmethine, $C_6H_8BrN_3 + 3H_2O$; periodides of these bases; cyanmethine-biniodide, $C_6H_9N_3.I_2$; constitutional formulæ of these bodies.

Observations on Ballo's Alleged Hydrate of Sulphide of Carbon.—V. Wartha.—This paper contains a refutation of the statement made by M. Ballo, that the congealed sulphide of carbon is a solidified hydrate of that substance. The author, after referring to Dr. Thilorier's discovery of solidified carbonic acid (1835), points distinctly out that M. Ballo is wrong, while he proves the correctness of his own experiments.

Law of Avogadro.—J. Thomsen.

Contribution to our Knowledge of the Sulpho-Nitrogen Acids (Sulphoxyazo-Acids).—A. Claus.—This lengthy memoir contains the following sections:—Disulpho-hydroxyazotate (*azosaures*) of potassa, $(NOH)2(SO_3K)+2H_2O$; sulphazonate of potassa, $(NH.NO_2K).4(SO_3K)=K_5N_2HS_4O_{14}$; oxysulpho-zotinate of potassa, $(N_2O_2).4SO_3K=K_4N_2S_4O_{14}$; trisulphoxy-azotate of potassa—

$$(NO).3(SO_3K)+H_2O=K_3NS_3O_{10}+H_2O.$$

The paper contains the exhaustive description of the preparation, reactions, and properties of these salts, and the products of their decomposition.

Isomeric Modification of Sulphocyanide of Potassium.—A. Fleischer.—This essay contains the account of a series of experiments made by the author in order to obtain from persulphocyanic acid, by treating it with alcoholic solution of potassa, the modification of sulpho-cyanhydrogen and sulphocyanide of potassium isomeric with essential oil of mustard (*senföl*) containing sulphur. The salt obtained by the author (isosulphocyanide of potassium, $2KCSN+H_2O$), is at once recognised by its behaviour with per-salts of iron; because, since the ordinary sulphocyanide of potassium yields, with the per-salts of iron, the generally well-known very characteristic blood-red colour, the isosulphocyanide of potassium obtained by the author yields, with a single drop of a neutral solution of the iron salt, a brown colouration, which disappears upon the addition of more of the iron solution, or also by strongly agitating the liquid. The reactions of the ordinary sulphocyanide and isosulphocyanide are described at length, and prove that the iso-salt is really a very different modification of the ordinary salt.

Action of Sulphuric Acid on Opianic Acid.—C. Liebermann and C. Chojnacki.—This exhaustive essay does not admit of any useful abstraction, notwithstanding its intrinsic scientific merits.

Law of Avogadro.—Dr. R. A. Mees.—A learned algebraico-physical essay.

Heat Set Free by the Action of Oxygen upon Hydrochloric Acid.—F. Hurter.—This paper is written with the view of proving that M. Thomsen's views expressed on the researches of Mr. H. Deacon, concerning the heat set free by the action of oxygen upon hydrochloric acid, are erroneous, and due to the fact that M. Thomsen did not read Mr. H. Deacon's original memoir, but a badly-made and translated abstract thereof (CHEMICAL NEWS, vol. xxiii., p. 99).

Lecture Experiments.—Dr. A. W. Hofmann.—Reserved for full translation.

Direct Substitution of the Alcohol Radicals for the Hydrogen of Phosphuretted Hydrogen.—Dr. A. W. Hofmann.—This lengthy and learned essay does not admit of any useful abstraction, taking into consideration the great merits of the author's work.

Sensitiveness for Light of the Haloid Salts of Silver, and on the Connection Existing between Optical and Chemical Absorption of Light.—C. Schultz-Sellack.—The following chief results may be drawn from the author's researches:—All rays of coloured light which are optically perceptibly absorbed by the haloid salts of silver in films of some few millimetres' thickness cause a chemical decomposition. The absorption of light is in these substances always connected with chemical action. All the haloid compounds of silver are chemically affected and altered by all those rays of light which the salts absorb in perceptible quantity.

Les Mondes, March 9, 1871.

This number contains only one original paper relating to chemistry, viz.:—

Preservation of Meat by the Aid of a Low Temperature and by Drying in Vacuum.—C. Tellier.—As regards the application of cold, the author uses dry cold air at 0° or −1°, but the mode of producing it is not mentioned. The drying in vacuum takes place while there is simultaneously present a substance which readily and rapidly absorbs water—as, for instance, sulphuric acid or chloride of calcium—with which, of course, the meat does not come into contact.

Discovery of a Layer of Phosphate of Lime near St. Petersburg.—Rev. F. Moigno.—The author states that, while some workmen were engaged in making excavations for putting in drain-pipes, they found, at 1·8 metres below the surface, a deposit of native phosphate of lime, similar in texture and properties to that recently found near Charleston, South Carolina, U.S.

The American Journal of Science and Arts, March, 1871.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

Discovery of Actual Glaciers on the Mountain Slopes of the Pacific Slope, U.S.—C. King.—This exhaustive essay is a valuable contribution to the physical geography and geology of the western part of the great Transatlantic Republic.

Some Rocks and other Dredgings from the Gulf Stream.—S. P. Sharples.—This paper contains the results of some chemical analyses of bone, rock, and ooze (soft mud or slime) from the bed of the Gulf Stream between Florida and Cuba, obtained at various depths while dredgings were being made for the coast survey of the United States. The bone (a portion of a rib of a manatee, an herbivorous cetacean) was dark-coloured, and almost destitute of organic matter, but still showed distinct traces of fibrous structure; its density (sp. gr. 2·83) was much greater than that of recent bone (sp. gr. 2·07). The piece was found at 106 fathoms' depth, and contained, in 100 parts—$Ca_3P_2O_8$, 62·40; $Fe_2(P_2O_8)$, 7·60; $CaCO_3$, 26·47; SiO_2, 0·34; H_2O + organic matter, 2·67. The following is the analysis of a piece of hard dark-coloured rock taken at 100 fathoms' depth off Sand-Key, Florida (sp. gr. 2·81); 100 parts contained—$CaCO_3$, 36·50; $Ca_3P_2O_8$, 35·54; SiO_2, 0·49; Fe_2O_3, 14·77; $MgCO_3$, 10·56; H_2O + organic matter, 1·43. Ooze, in 100 parts—$CaCO_3$, 85·62; $Ca_3P_2O_8$, 0·18; SiO_2, 1·52; Fe_2O_3, 0·31; $MgCO_3$, 4·26; H_2O + organic matter, 8·15. This paper contains, moreover, a series of analyses of various corals, and of two more specimens of rock very similar to that here mentioned.

Porcelain Rock of China.—Baron von Richtenhofen.—Reserved for full reproduction.

Notes on Granitic Rocks.—Dr. T. Sterry Hunt.—The continuation of this excellent and exhaustive essay; this portion containing the following sections:—Granitic veins of Maine, Brunswick, Topsham, Paris (U.S.), Westbrook, and Lewiston; crystalline limestones; Danville, Ketchum; denuded granitic masses; banded veins; Biddeford, Sherbrooke; veins at various New England localities; mineral species of these veins; veins in erupted granites; geodes in granites; veins distinguished from dykes; Volger and Fournet on the origin of veins; certain fissures and geodes distinguished from veins opening to the surface; temperatures of crystallisation of granitic minerals.

Geology of the Eastern Uintah Mountains.—Professor O. C. Marsh.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin, January, 1871.

This very small number does not contain any papers relating to chemistry and collateral sciences.

NOTES AND QUERIES.

Sugar Refining.—Can any of your readers give me a description of the Seyfert process (sulphuric acid) for sugar refining, as used at New York and elsewhere?—J. T. A.

Ammonia Sulphate from Gas Liquor.—Can you inform me where or how to obtain *practical* information on the above manufacture as adapted to a small gas-works, the sulphate being required in the crude state only?—AMMONIA.

Ammonium Salts.—I should be glad if any correspondent of your valuable journal would kindly inform me whether sulphocyanide of ammonium (rough impure, as found now in the market) can be directly used for the production of ordinary ammonium salts, as sesquicarbonate, strong solution of ammonia, &c.—H. M.

Hyposulphite of Sodium and Silver.—Will you kindly inform me through the CHEMICAL NEWS whether the hyposulphite of soda and silver, formed by dissolving a salt of Ag in hyposulphite of soda, is *poisonous*; and, if so, what quantity of it would constitute a dangerous dose? I have not been able to find any statements or records of experiments on the question in books; can you refer me to any? In Watts's "Dictionary," in articles on hyposulphites, I find several references to "Herschel;" can you tell me how to find the memoir referred to?—SAFETY.

MEETINGS FOR THE WEEK.

MONDAY, 17th.—Medical, 8.
—— London Institution, 4.
TUESDAY, 18th.—Zoological, 9.
—— Civil Engineers, 8.
—— Royal Institution, 3. William Pengelly, Esq., F.R.S., F.G.S., "On the Geology of Devonshire, especially of the New Red Sandstone."
WEDNESDAY, 19th.—Society of Arts, 8.
—— Meteorological, 7.
THURSDAY, 20th.—Chemical, 8.
—— Royal Society Club, 6.
—— Royal, 8.30.
—— Royal Institution, 3. Prof. Tyndall, "On Sound."
FRIDAY, 21st.—Royal Institution, 9. Prof. Blackie, F.R.S.E., "On the Pre-Socratic Philosophy."
SATURDAY, 22nd.—Royal Institution, 3. Mr. Lockyer, "On Astronomy."

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THE CHEMICAL NEWS.

VOL. XXIII. No. 595.

ON THE FORCE EXERTED BY DETONATING GASEOUS MIXTURES.

By Dr. BERTHELOT.

As an appendix to, and completion of, my investigations on the force exerted by the explosion of powder and other explosive substances, I have considered it proper to collect together some analogous facts bearing upon the chief detonating gaseous mixtures:—

Nature of the explosive mixture.	Quantity of heat disengaged by 1 kilo (1).	Volume of 1 kilo. of the mixture (2).		Pressure exerted at the moment of combustion at constant bulk (3), in atmospheres.
	Caloric.	Initial M.C.	Final M.C.	
H_2+O_2	3,280,000	1·86	1·24	20
$C_2O_2+O_2$	1,570,000	0·75	0·50	24½
$C_2H_4+4O_2$	2,375,000	0·84	0·84	33
$C_4H_4+6O_2$	2,530,000	0·72	0·72	41
$C_4H_2+5O_2$	2,800,000	0·74	0·63	44½
$C_4H_6+7O_2$	2,300,000	0·70	0·78	39
$C_8H_8+12O_2$	2,450,000	0·63	0·72	46
$C_8H_{10}O_2+12O_2$ (vapour of ether)	2,400,000	0·59	0·75	49
$C_{12}H_6+15O_2$ (vapour of benzine)	2,300,000	0·60	0·63	45
$C_4N_2+4O_2$	2,300,000	0·58	0·58	49½

(1). This quantity has been calculated according to the heat of combustion, as adopted in my memoir in the *Annales de Chimie et de Physique*, 4th series, vol. vi.

(2). This volume is taken at a pressure of 0·760 metre, and at 0°. The water is supposed to be as vapour—that is to say, the formulæ wherein this figure occurs only apply to degrees above 100° generally.

(3). This pressure is calculated according to the laws of Marriotte and Gay-Lussac. The following values for the specific heat by constant volume have been taken:—

$H_2O_2 = 7·2$ for 18 grms.
$C_2O_4 = 7·2$,, 44 ,,
$N_2 = 4·8$,, 28 ,,

It is supposed that the explosive gaseous mixture has been made at atmospheric pressure.

According to this tabulated review, the maximum effect which is obtainable from 1 kilo. of each of these explosive gaseous mixtures varies only from one to two, and is about equal for the various hydrocarbons; but this effect surpasses that of all solid, as well as of all liquid explosive compounds. With hydrogen and oxygen, for instance, it is five times greater than that of powder, and two and a half greater than that of nitroglycerine; with the hydrocarbons, it is four times greater than that of powder, and twice greater than that of nitroglycerine. Notwithstanding the diversity of composition and condensation of the gases above named, the pressure varies only from one to two. When, instead of oxygen, atmospheric air is taken for the mixtures, the maximum effect of a given weight of the combustible gas does not change, but the pressures become less by half, or even below that, in consequence of the necessity of heating the nitrogen. It might have been expected that protoxide of nitrogen could have been substituted, with advantage, for pure oxygen, because the protoxide yields, by its decomposition, a volume of nitrogen, and an additional quantity of caloric; but experiments proved that this advantage is more than outweighed by the necessity of heating the nitrogen. The gaseous mixtures we have experimented with are supposed to be made under atmospheric pressure. The pressure theoretically exerted by them lays between 20 and 49 atmospheres, each at 15 lbs.' pressure to the square inch, and is very widely distant from the pressure exerted by the bulk of the solid and liquid explosive substances we are acquainted with, a result the very opposite to that which was supposed to be the case by most people until now. In order to obtain a pressure more nearly like that exerted by solid and liquid explosive substances, it would be necessary to compress the explosive gas mixtures previous to exploding them; but even then, in order to come up to the force of pressure exerted by solid and liquid explosive substances, it would be required to reduce, by strong pressure, the bulk of the gaseous mixture to the thousandth of its initial volume; that is, in other words, it would be required to make these gaseous bodies to the same density as that of solids and liquids. Leaving out of the question for the moment the difficulty of this operation, the result would be the liquefaction of most of the hydrocarbonated gases; while the oxygen remaining in the state of gas, the result would be the destruction of the homogeneity of the explosive mixture, and, accordingly, also the possibility of obtaining its instantaneous explosion by a spark. The liquefied protoxide of nitrogen would be of some advantage in this emergency, since I have found that, when that liquefied gas is mixed with liquefied hydrocarbons, mixtures are obtained the explosive force of which is comparable to that exerted by nitroglycerine or mixtures of chlorate of potassa with gun-cotton and picrate of potassa; it would, however, be difficult to obtain the instantaneous explosion of mixtures of the liquefied gases alluded to. In order, however, to give some proof of the great explosive power of these liquefied mixtures containing liquid protoxide of nitrogen, I quote the following figures:—

Explosive material.	Quantity of heat disengaged by 1 kilo.	Volume of gas produced. M.C.	Product of these figures.
Liquid protoxide of nitrogen mixed with liquefied olefiant gas, or with isomeric carburets, or with ordinary ether	1,300,000	0·76	990,000
Liquid protoxide of nitrogen and liquefied acetylen	1,400,000	0·72	1,000,000
Liquid protoxide of nitrogen and liquefied cyanogen	1,400,000	0·69	970,000
Nitroglycerine	1,280,000	0·71	910,000

These numbers, it should be stated, are the result of calculation, and only approximative, not rigorously exact. —*Moniteur Scientifique.*

ON THE CONDUCTING POWER OF VARIOUS METALLIC SULPHIDES AND OXIDES FOR ELECTRICITY, AS COMPARED WITH THAT OF ACIDS AND SALINE SOLUTIONS.*

By WILLIAM SKEY,

Analyst to the Geological Survey of New Zealand.

THE author refers to his results, as stated in a former paper ("On the Electro-Motive Power of certain Metallic Sulphides, &c.,") in which paper the conductivity of these sulphides for electricity is shown.

* Abstract of a paper read before the Wellington Philosophical Society, January 29, 1871.

He then refers to the order of this conductivity as given in the latest works on electricity now to hand, and states there is a great discrepancy between his observations and those upon which this order is founded, since a great many sulphides and oxides are far better electric conductors than the best conducting acids or salts which are instanced in these tables, or have been experimented with by the author, the reverse being affirmed of them in such works.

The manner in which these results were obtained is then stated, and a table embodying them is given, from which it appears that the following minerals are better electric conductors than nitric acid (concentrated), which is the best conducting non-metallic liquid yet tested by him:—Mispickel, galena, sub-sulphide of copper, copper pyrites, protosulphide of iron, tin pyrites, nickel pyrites, sulphide of bismuth, magnetic iron ore, graphitic tellurium, binoxide of manganese, iron pyrites, oxide of zinc. Titanic iron and boulangnite were found but feeble conductors.

The following minerals were proved to be non-conductors, or comparative non-conductors:—Sulphide of molybdenum, zinc blende (cryst.), stibnite (cryst.), cinnabar (compact red variety, cryst.), orpiment, bouranite (cryst.), manganese blende (not cryst.), proustite, pyrargite, silver glance, horn silver, carbonate of iron (black variety cryst.), calamine, chrome ore (cryst.), tungstate of iron (cryst.), specular iron ore (cryst.), rutile, bramite, tin ore (cryst.), leverite (cryst.), oxides of copper (sub and proto, cryst.), iserine, oxychloride of copper, &c.

These results, when checked and added to by further experiment, especially by experiments upon larger mineralogical collections, and also upon certain pure sulphides and oxides as chemically prepared, and in allotropic states, will, no doubt, prove useful towards the ready discrimination of minerals in certain cases, and interesting in relation to the science of electricity generally.

As a step towards testing these pure and chemically prepared sulphides, the author finds sulphide of mercury is a good conductor, whether the precipitate is only dried or sublimed, so long as it retains its dark colour. Sulphide of silver, on the other hand, neither conducts when merely dried, nor yet when submitted to a dull red heat.

The author hopes that further inquiry into this behaviour of metallic ores in regard to electric currents will furnish evidence of the particular law which regulates such behaviour.

SPECTRUM ANALYSIS
IN ITS APPLICATION TO THE
BESSEMER PROCESS FOR THE MANUFACTURE OF STEEL.*

By Professor ROSCOE, F.R.S.

(Concluded from p. 176).

LET us now proceed to the other point which we have to reach, namely, the examination of the Bessemer flame.

I have already said that I should not feel myself justified, nor should I be qualified if justified, in entering into a detailed statement of the chemical reactions which go on in this most important and interesting process; for one good reason is that there are many here who know much more about it than I do, and another reason is that I think that even those who know most about it know, perhaps, very little. I think, however, that we may take it that, in the first place, the changes which occur are mainly as follows:—The silicon and carbon which are contained in the iron are oxidised to begin with. I suppose it is a fact that, in the first place, the carbon or graphite which is contained in the pig-iron is converted into combined carbon. We find that, in the second stage of the process, the carbon has already diminished somewhat, but that it has not diminished in such a large proportion as the silicon; and then the carbon gradually diminishes out till we get to the last portion, where it has not altogether gone, but where we have about 1-10th or 2-10ths of carbon left; that the silicon then has diminished down either to something like a trace, or 2-100ths per cent.

Now, with regard to the manganese. This, as you all know, plays a most important part in the process, but we know that the ores of iron used for the Bessemer process differ widely in their amount of manganese. The irons used in Styria differ very widely from those used at the Atlas Works, Sheffield. The Dowlais iron too, and, I believe, almost all our English iron ores contain very much less manganese than the iron which is worked in Styria; and, therefore, since we have here in England to deal with an iron containing less manganese, we shall not expect the manganese, as far as regards the spectroscope, to play a very important part. With regard to sulphur and phosphorus, I have but little to say. The sulphur apparently diminishes somewhat in the iron. We find that in iron containing a small trace of sulphur that trace of sulphur either diminishes slightly, or altogether disappears, or leaves very little trace; whereas the phosphorus is scarcely got rid of at all. The reasons for this I will not attempt to enter into now. I will not attempt to explain them, but there are reasons which I should like to state as to why we should not expect to see in the flame reactions either of silicon, or of phosphorus, or of sulphur.

It is well known to all present that it is very necessary to stop the blast at the exact moment that the carbon is burnt out. The mode of ascertaining this is by the dropping of the flame. Now, it has struck many people that it was very probable that this indication of the dropping of the flame by which the end of the process was ascertained, might be ascertained with greater accuracy if, instead of looking at the flame as a whole, we were to look at the component parts of the flame; that is to say, if, instead of looking at the flame with the naked eye, we were to look at it through a prism, we then should most likely see certain effects which would not be visible, or the effects which are visible would probably become more decided and more striking if observed in this way.

Let us first try to get a clear idea of the general appearance of the Bessemer flame, and then endeavour to explain what we see.

Spectroscopic Appearance of the Bessemer Flame.

Time. Mins., about.	Appearance to Eye.	Appearance of Spectrum.
First Stage:—		
0— 4	No flame seen.	Faint continuous spectrum, due to sparks of burning metal.
4— 6	Small pointed flame.	Brighter, with yellow sodium line flashing.
6— 8	Unsteady flame, and explosions.	Bright, continuous, with sodium line permanent, red lithium line, red and violet potassium line.
Second Stage:—		
8—10	Very bright and dense flame.	Bright carbon lines, in red, green, and blue, with preceding spectrum.
10—14	Flames less dense, but bright.	Bright green carbon lines more distinct.
Third Stage:—		
14—16	Flame diminishing in size and intensity.	Bright green carbon lines become faint.
16—18	Flame drops. Blow ends.	Bright green carbon lines suddenly disappear, leaving continuous spectrum.

* A Lecture delivered before the Iron and Steel Institute, on Wednesday evening, March 29th, 1871.

The bright lines of iron, hydrogen, nitrogen, and manganese have also been observed in the Bessemer spectrum.

We may divide the process into stages; and, to begin with, we notice that the Bessemer flame gives a continuous spectrum. At a certain period, which varies according to the heat, and according to the time which the whole operation takes, certain lines begin to make their appearance; and three lines appear, to begin with, namely, the yellow sodium line, the red lithium line, and the bright potassium line. Now, when the thing has gone on for a certain time, a change comes over the whole appearance of the spectrum. Lines begin to make their appearance in almost every portion of the spectrum, and at last we have a most complicated series of interspaced bright and dark bands; and you must imagine, if you please, that this is covered with a continuous spectrum. Then we get not a continuous spectrum, but one in which there is a whole series of bright bands. This spectrum is a very complicated one. At a certain period of the blow these lines again disappear, and at the last stage of the operation, we see the second spectrum repeated; and this is the point at which the flame drops, and it is the point at which it has been found, by practice, necessary to turn off the blow.

Understanding now what is observed,—understanding the four changes which occur in the different stages of the operation,—let us next try to find out what it really is that we are looking at in these bright bands. The question comes as to what these lines are caused by, and here, of course, we enter, to a certain extent, into the field of conjecture, because the subject is an exceedingly difficult one, and because the spectrum as seen is very complicated. Still, I believe, there is no doubt at all that the bright lines are mainly caused by carbon in some form or other. I will not undertake to say what the exact form of the carbon is. One very interesting fact, with regard to the spectra of bodies such as carbon, is, that we find, under certain circumstances, they vary. Even such a body, for instance, as nitrogen gas can be made to exhibit two distinct spectra. Under certain circumstances, we get one series of bands; under other circumstances we can get a different series of bands. Another variation of the same sort is observed in the case of the calcium spectrum. The calcium spectrum, as seen in the Bunsen gas flame, differs from that which is seen by aid of the intense electric spark. In one case there are broad bands, and in the other fine bright lines. We can explain why there is this difference, because we know that at the low temperature of the Bunsen gas flame the calcium compounds are not decomposed, and that we are in that case really looking at the spectrum of a compound,—probably calcium chloride, or calcium oxide; whereas, when we heat the compound so intensely as we do in the bright electric spark, we get the true spectrum of the metal, and which cannot be changed or varied. But then, when we come to nitrogen gas, we are not able to explain matters so well. In looking at two of the spectra of nitrogen, —one which is seen under low pressure, and one which is seen at atmospheric pressure—we observe that there is a very considerable difference between them. The low pressure spectrum contains a series of bands which are not seen in the high pressure spectrum. What the cause of this is we cannot tell. In this case we enter into the region where we are on the border-land of knowledge and ignorance. We do not pretend to say what is the cause of this: we must be satisfied at present with recognising the fact.

Let us now turn to the spectrum of carbon. We find that, under certain circumstances, carbon can be made to give very different spectra. The carbon spectrum obtained by burning olefiant gas in oxygen gives bright bands. If we pass an electric current through a small quantity of carbonic acid gas in these Geissler tubes we get a second spectrum. There is still another spectrum differing from these two.

In the Bessemer flame, there are lines in the violet which are not identical in position with the known lines of carbon; still I think there is little doubt, seeing, in the first place, that carbon is capable of giving different spectra, as we have shown in three instances, that this Bessemer flame spectrum is probaly a further modification of the carbon spectrum. It is a remarkable fact that it has not been found possible to get, artificially, the peculiar spectrum which is seen in the Bessemer flame, and seen in several other flames—seen, for instance, as is well known, in that beautiful flame which is generally emitted when the spiegel iron is allowed to run in. It is seen also in a hot-blast furnace. It has been seen by a gentleman who has devoted some attention to this subject—Dr. Watts, an old pupil of mine, who has examined this carbon spectrum with some care. He has noticed that the Bessemer spectrum is observable in the hot-blast furnace when they turn the blast down to the bottom of the furnace. In certain cases, where a very high temperature is brought to bear upon burning carbon, this same flame has been exhibited.

I need not say that, if we know little about the chemistry of the process, which goes by the name of the illustrious President of the Iron and Steel Institute, we know still less about the constitution of the spectrum; and it requires still that experiments should be conducted on a much more extended scale than they have been at present, and with an instrument of much higher power than we have yet been able to bring to bear upon the subject, in order that we may separate these lines into their exact positions, or ultimate parts, if I may use that expression, and that we may thus recognise the different substances which are present. This, however, is a matter of very great difficulty. Imagine, if you please, that we have to be provided with such an instrument as you see before you, the beautiful automatic spectroscope of Mr. Browning; imagine that we have to go with this instrument into works fitted up for iron works. The instrument must be so placed on a stand that it can be easily carried about. Then we must carry with us an induction coil and a number of these tubes, because we cannot trust to any scale for measuring these lines; and we must have the light from the Bessemer flame in one part of the eye-piece; and we must have the light from the iron, carbonic acid, and hydrogen, and the various other bodies, in another part of the field, or otherwise we shall never get a good result. These difficulties, you see, are not small in coming to an exact conclusion with respect to the composition of this flame.

Now we must pass on to the last part of our subject, and that is the most difficult one—the practical application of these principles.

Assume, for a moment, that these lines are carbon lines, and of that, as I have said, there can be no doubt. Those who have read the literature on this subject will know that it has been stated, and stated with some degree of confidence, as if it were a well-established and well-recognised fact, that these bright lines are mainly due to manganese. Now this I really cannot believe. I do not know what the exact appearance of the spectrum of a Bessemer flame is in which very large quantities of manganese are used; but this I do know, that, in such works as the Barrow Steel Works, where they employ an iron which we all know contains next to no manganese at all, these lines are as brightly seen as in those places in which a more manganiferous iron ore is employed; and I know, too, that these lines disappear as the carbon disappears; and, therefore, taking these together, although I will not say that there are no lines due to manganese, especially in the spectrum of such ore as is worked on the Continent, still I think we may take it that the manganese lines are a very unimportant portion of the spectrum in the flames which we see in England.

The first question which will naturally suggest itself is, Do these lines disappear at the point at which the blow is turned off? I think the whole evidence on this point is unanimous in showing that the lines disappear at the

time when it has been found necessary, by experience, to turn the blow off. The second question which we may find it necessary to ask ourselves is,—Can this point of decarbonisation be more accurately determined by the disappearance of the lines than by the unassisted eye of the workman? And the third question which we may put is,—Can we, by any application of the spectroscope, or by any determination of the disappearance or appearance of these lines—can we, by any observations made in this way, come to this point, that we shall be able to determine when the blow ought to be stopped, so as to leave in the steel the requisite quantity of carbon? These are the three points which we have to decide.

Now I believe that, as regards the first point, there is no practical difficulty about it. I believe it is a point which can be ascertained. As regards the question whether we can more accurately determine the point by the disappearance of these lines than is done at present by the naked eye, it appears to me that whether the adoption of the spectroscope becomes general depends upon the future—upon how far in future it will be found necessary to prepare steel containing the same degree of tensile strength, and containing, as nearly as possible, the same percentage of carbon. There is no doubt, however, that variations in the percentage of carbon must occur, even were it possible always to hit the point of decarbonisation, owing, among other causes, to variations in the temperature causing a larger or smaller quantity of the carbon in the spiegel to be burnt before the steel can be cast. But that a uniform steel can, as a general rule, be prepared when the charges are kept uniform by working by the unaided eye, is a fact with which we are all well acquainted. Still, under the most favourable conditions of uniform charges and careful working, exceptional and different blows occur; indeed, nothing is more striking than to observe when the conditions of uniformity are most favourable. Great inequalities in the blows, both as regards temperature and length of time, are needed for the decarbonisation, and there are blows which are termed "fiery" blows, or "lively" blows, which differ from the others. So much is this the case, that it is generally said by the workmen, I believe, that no two blows are exactly alike. We may well admit, even in spite of the discoveries which have been made, that we are still in the infancy of the steel manufacture, both as regards its chemistry, and probably, also, its mechanical and metallurgical details. The reference which was made by the President of the Institute, in his opening address, to Sir Joseph Whitworth's application of cast-steel under pressure, proves, I think, perfectly, that we are only just beginning to understand what we or our children will have to do in future. So it may be with the application of spectrum analysis. If we find that it is necessary to produce a steel varying, not to the extent of eight-, or six-, or four-, or two-tenths of carbon, but in which the amount of carbon does not vary beyond one-tenth, I think we shall be obliged to use the spectroscope.

In certain cases, however, the spectroscope has already proved itself of inestimable value in new works, and where different ores are needed to be used. On the Continent especially, it has been most successfully employed. From information which I have just received from Gratz, I learn that the spectroscope is largely used in Styria, Saxony, and Belgium. Indeed, at one place, it appears to have been impossible to produce a uniform steel until the spectrum reaction was introduced in 1869, and from that time the steel has not varied more than 5-100ths per cent of carbon. In England the conditions are somewhat different. We probably use a more uniform ore than the highly manganiferous ores employed on the Continent, and hence the spectroscope may be more valuable on the Continent, for this purpose, than it is or than it may be with us. At any rate, the spectroscope has not been so largely adopted with us as abroad, and this not because our steel-makers distrusted it, or were ignorant of the application, but because they saw no advantage in the employment of a more delicate test than that of the workman's eye.

We next pass to the third point, which is, perhaps, the most interesting and the most important of all, namely, whether it is possible (as I believe is done in Sweden) to stop the blow at a particular point indicated by means of spectrum analysis. Now, on this subject, I am glad to be able to give you some information—I am sure of interest, and I believe of importance. It has just been placed in my hands by a gentleman whose name you are all well acquainted with, and whom I am proud to be able to number amongst my pupils—a gentleman about whom I think we shall hear a great deal more before long. That gentleman is Mr. Snelus, who has been kind enough to help me here. He has put into my hands the result of a series of experiments which go to prove what appears to me to be a very important point, and that is, that not only does he find that the point of decarbonisation can be accurately determined by means of the spectroscope, but he finds also that it is possible, at the beginning or during the course of the blow, to predict how long the blow will be; that is to say, he can, in the middle of the blow, predict that the blow will last, for instance, eighteen minutes, and he finds that it does last eighteen minutes. Then he predicts twenty-four or twenty-five minutes, and he finds that it lasts twenty-three and a half minutes. He then predicted thirty-two minutes, in another blow, and he found that it did take thirty-two minutes; and so on with twenty experiments made both at Dowlais and Abervale. He finds that it is possible, by means of this reaction, to find when the blow will cease. You will all of you see the importance of this observation, because, if it enables him to tell at what point the blow would cease, it would enable him to stop three or four minutes before that time. It only, therefore, remains to know whether the steel, after having partially blown, as is the case in Sweden, is in a fit state to be used,—whether the last portion of the blow (the last two, or three, or even four minutes) does not, in some way, alter the quality of the steel besides getting rid of the carbon. For instance, if in the last portion of the blow a large quantity of silicon is got rid of, which it is necessary or desirable to get rid of, then if we stop three or four minutes before the natural end of the blow, of course we shall have a steel containing large quantities of silicon. The results are quite new, and, of course, they will have to be worked out; but I think you will all agree with me in congratulating Mr. Snelus on this very interesting observation, and in wishing him success in working it out to useful results.

I have now nearly done. I have only to say that I have also to-day received a note from another gentleman whose name is, perhaps, even still more familiar in the ears of all steel-makers—Mr. Bragg, of the Atlas Works,—who tells me, "In reply to your questions—(first), we are daily using the spectroscope in determining the point of the disappearance of carbon in the process; and (second), we are, to a certain extent, satisfied with the results." (He is careful to say "to a certain extent." He can scarcely say that he is fully satisfied.) "We have lately had a new instrument made by Mr. Browning. It has been an improvement upon the other which we had to begin with," and which, I believe, was got for them in the year 1863.

I will now show you the spectrum of iron and the spectrum of steel. I have a new method of showing these, which, I believe, is really better than any we have have had before. It consists of placing a bar of the metal at the bottom pole of the electric lamp. Here you have a beautiful series of bands. These lines, or, at any rate, some of them, have been seen in the Bessemer flame. The greater portion of the iron comes off in the molten state, either as fluid oxide, or fluid metal, and of course gives the spectrum which is due to that fluid state, namely, a continuous spectrum. But in the hottest part of the blast, a portion of the iron is seen in the gaseous state, and by direct comparison with the iron spectrum, I have got a

coincidence of the lines. In the iron spectrum before you, you see what masses of green lines we have; and I scarcely need to tell you that, in a more delicate and accurate instrument, these resolve themselves into whole systems of lines; and each one of these lines which we have in the iron spectrum has its dark representative in the solar spectrum. Hence Kirchhoff comes to the conclusion that iron is contained in the sun. Now, we will put a piece of steel into the apparatus, and a distinct difference will be noticed. Here, now, is the spectrum of steel. There are the steel bands; and you see that there is a large number of bands in the violet. I told you that in the Bessemer flame there were bands in the violet. These are the bands which I have no doubt are due to carbon, and I have no doubt that we are here really burning carbon. You see that the steel spectrum is decidedly different from the spectrum of metallic iron.

Some of our friends are very anxious that the spectroscope should do—I was going to say, *everything*—but, at any rate, more than turns out to be possible. They say, "We are very much obliged to you for coming here, and telling us how to find the point of decarbonisation, but we can do that for ourselves very well; but we shall be much more obliged if you can tell us about the phosphorus and about the silicon." I should be very glad to do so—*if I could*. There are some things which we can do, and other things which we can not do, and it appears to me that that is one of the things which we can not do; and now, if you will allow me, I will endeavour to give my reasons.

From what I have said you will observe that, in order that we should see the spectrum of a substance, it is necessary that it should be present in the state of a glowing gas. If it is present in the form of a liquid or a solid, it does not give us any distinct or characteristic spectrum. But if the body is present in the state of gas we can tell you that we have, for instance, phosphorus, or silicon, present.

Now, first, as regards silicon, the fact is that it is excessively difficult to get silicon in the state of gas. In fact, so difficult is it that all attempts to map with precision the spectrum of silicon have been in vain. The moment silicon boils it forms, as we know, the white oxide, or silica; and this white oxide may be melted by the heat of the oxyhydrogen flame, but it cannot be volatilised. This is, in my opinion, the reason why at the commencement of the blow we have a continuous spectrum, and while during the whole of the blow we have, more or less, a continuous spectrum, masses of silicates being shot out in that form,—little masses which are white-hot, but which do not indicate more than that they are solid bodies. Whether those masses contain silicon, or phosphorus, or sulphur, we cannot tell; and, therefore, though it is neither safe nor wise to predict beforehand, I do not expect to find that in future we shall be able to tell much about the silicon by these means.

Then with regard to the phosphorus. It is quite true that we can get the spectrum of phosphorus. It consists of three bright green lines. "Well, then," you say, "why don't you see the spectrum of phosphorus in the Bessemer flame?" Well, I think that the analysis of the slag will tell you why we do not see the spectrum of phosphorus. It is for a very good reason,—because the phosphorus is not there, or there is a very small quantity of it, and we know very well that it does not come out but remains with the iron.

I think, therefore, that it is rather hard upon us that we should be told, "Well, you cannot do us any good at all, because you cannot tell us anything about the silicon, and you cannot tell us anything about the phosphorus." Still, I think that what I have said, and especially what I have recently been informed of, will show you that spectrum analysis is not wholly without its use, and that we may believe that it will in time really come to be much more largely employed than it is, and that it will be able to tell you what we originally hoped it would be able to tell you —*when to finish your blow*.

I intended to say a few words about the presence of iron in the sun; but I am sure that I have exhausted your patience already, and therefore I have simply to thank you for the very kind way in which you have listened to what it has been my pleasure to bring before you.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

April 6th, 1871.

Professor Frankland, F.R.S., President, in the Chair.

The President, occupying the chair the first time since his election, returned his thanks to the Society for the honour conferred upon him, and expressed his readiness to discharge the duties of his office to the best of his abilities.

The following gentlemen were elected Fellows:—F. Coles, C. E. Groves, E. W. T. Jones, L. T. McEwan, J. L. Shuter.

The following papers were read:—

"*On Burnt Iron and Burnt Steel*," by W. Mattieu Williams.

On page 7 of Dr. Percy's treatise on the "Metallurgy of Iron and Steel," he quotes the following conclusion of Mr. Riley:—"The property of becoming useless after exposure to a welding heat appears from the above experiments to be a special character of fused wrought-iron. The experiments have not been carried far enough to lead to any explanation of this: it may probably be due to the absence of a small quantity of carbon usually present in wrought-iron. In Bessemer's iron (Mr. Riley believes), there is no carbon, yet it certainly welds, though not very well."

Iron which has thus been damaged by re-heating is designated "burnt iron" by the workman, who also gives the same name to iron which has been excessively heated and exposed *after balling* in the puddling furnace. It is worthy of remark that no amount of heat applied to the iron in the blast furnace or in the early stages of the puddling process produces burnt iron.

Burnt iron is brittle, its fracture is short and what is called crystalline; it has lost the fibrous character and silky appearance of good iron.

If steel is raised to a bright-red heat, and suddenly cooled, it is rendered hard and brittle, but this hardness may be modified, and brittleness diminished, by the well-known process of tempering. If, however, the steel be raised to a yellow or white heat, and then be suddenly cooled, it is no longer capable of being tempered by mere re-heating. It remains permanently brittle, and is worthless for the ordinary uses of steel, unless it is again raised to a welding heat, and thoroughly rolled or hammered while hot, and then allowed to cool gradually.

The fracture of burnt steel presents a very "coarse grain." It displays in a marked degree the so-called "crystalline" appearance. A careful examination, however, reveals something beyond a merely crystalline structure; for the facets of the aggregated granules have a more or less conchoidal or rounded form. This has been observed by practical men, and the name of "toad's eyes" has been given to the concavities. As brittleness, and granular or "crystalline" fracture, and even toad's eyes, may be due to other causes, it is sometimes of considerable practical importance to be able to determine whether certain defects of a particular sample of iron or steel are due to original defects of the raw material, or to the carelessness of the workman, who may have burned the iron or steel in subsequent stages of working.

When engaged as chemist to large iron works I was

very anxious to be able to solve this question, but for a long time was unsuccessful. Karsten's theory, attributing the change to the absence of carbon, was shaken by the case of the burnt steel. This was also the case with the obvious explanation that the burning is due to oxidation.

An accident finally removed a portion of the theoretical difficulty, and, at the same time, suggested a practical method of testing for burnt iron.

An important part of my laboratory duty consisted in determining the percentage of carbon in Bessemer steel. Each blow was thus examined daily. Besides determining the carbon of the raw ingots, I frequently had to do the same for samples of rails, plates, tyres, and miscellaneous samples of manufactured steel. As many as thirty or forty of such determinations were frequently made in one day.

On one occasion, a trap was laid to catch the chemist, by an excessively "practical" man, who was displeased by the detective revelations of the laboratory. Two samples of borings, marked A and B, were sent for the carbon of the "*steel*" to be determined. My assistant reported that neither contained more than a trace of carbon,—that neither sample was steel, but both were iron. I then examined them myself, and observed that they behaved very differently on the application of the standard nitric acid used for solution.

When the acid was applied to sample A, the liquid at first became unusually dark, suggesting a very large proportion of carbon. This colour gradually became less marked, and, finally, disappeared altogether, and when the solution was completed I found no indications of carbon at all. Sample B behaved in the usual manner of good iron, and only indicated a small trace of carbon—too small to be quantitatively determined by the colour test.

Suspecting the presence of oxide in sample A, I repeated the experiment more carefully, and then observed that the dark colour of the liquid was due to suspended particles, kept in suspension by the effervescence, and that these particles were finally dissolved.

Further examination proved that they were particles of oxide of iron entangled amidst the metallic iron. On enquiry I learned that the samples were borings taken from the opposite sides of an armour plate, one side of which was burned, and the other side good. Sample A was from the burnt side.

I afterwards obtained samples of iron burnt in different degrees, from such as were just perceptibly damaged, to others that were quite rotten, and I found that, in all cases, there was a small quantity of oxide in the burnt iron, and that I could readily detect it by the very simple method of putting the borings into a nitric acid diluted to the specific gravity of 1·20. The burnt iron always gave a dirty appearance to the acid, which afterwards disappeared. This appearance is quite different from that presented by the acid in which similar borings of good iron have been placed. With a little practice in observing the normal appearance of good iron, and by using similar borings, the difference is easily detected, and thus a valuable practical laboratory test for burnt iron is afforded.

This test is only applicable to borings from samples of clean finished iron, the silicon, graphite, and other impurities of pig-iron produce a similar dirtiness, which might be mistaken for that produced by the oxide. The dark colour produced by combined carbon is so different in its appearance that it is at once distinguishable, besides which this is permanent.

I examined samples of burnt steel in the same manner, but no indications of the presence of intermingled oxide were presented. This, of course, was to be expected, as the carbon of the steel must protect it more or less completely from oxidation by heat. It thus appears that, although the mechanical peculiarities of burnt iron and burnt steel so nearly resemble, the respective chemical changes effected in the so-called burning are quite different.

A number of other facts go to show that the burning of burnt iron is simply a process of partial oxidation and, that therefore the term "burnt iron" is philosophically correct. An armour plate, for example, is made up of a larger number of balls or blooms of puddled iron welded together. It is built up by welding puddled balls with slabs, slabs into moulds, and piling and welding these moulds into plates. These processes include several re-heatings of the iron, and at each re-heating the whole of these great masses of iron must be raised to a full welding heat.

The rolling of a 4-inch plate was formerly declared by all practical men to be an impossibility, until a remarkably enterprising man, then scarcely aware of the difficulties to be overcome, undertook to do it, and succeeded, not only in reaching the four inches, but ultimately progressing to fourteen. One of the greatest difficulties to be overcome was the re-heating of these great masses without burning them, and opportunities of earning large wages were offered to the workmen who should superintend these furnace operations successfully. I know an illiterate black-faced workman, one of the pioneers in this enterprise, who earned as much as £40 per week, by payments received on tonnage of work done under his direction, and have watched with great interest the mode of proceeding of this man and others who have successfully conducted such furnace operations. By various devices, the philosophy of which they do not dream of understanding, they subject the iron to the action of a *reducing* flame only. I have peered into these great reverberatory furnaces or ovens at all stages, scorching my face, and inflaming my eyes continually, and, except at quite the early stage of the re-heating, or the preparatory stages of heating the oven walls, have always found a densely carbonaceous, obviously reducing flame to be the only one permitted to play upon the iron.

One device I may specially mention on account of its simplicity, universality, and effectiveness. The great doors of the reverberatory chamber, through which the iron is introduced and withdrawn, are made to rise and fall by the action of a counterpoised lever. They are frequently opened to examine the state of the pile, and do not fit closely. There is considerable air space at the bottom. If this were left open, a supply of oxygen would find its way to the iron within. Without any idea of the philosophy of the practice, the workman places large lumps of coal just inside the doors in such a position that the entering air must pass them before they can reach the iron. This coal, intensely heated by radiation from the great flame, and the white-hot furnace walls, is prepared to combine with more than all the oxygen that can enter, which thus passes forward as carbonic oxide and aqueous vapour.

I have made similar observations in other processes of iron working; thus, when the puddler has called up his charge, *i.e.*, when it is no longer protected from oxidation by its own combined carbon, he closes his damper, and fills up the air-ways of his furnace *with lumps of coal.* He does this most carefully, just at the time when oxidation would be mischievous. He knows nothing about the theory of this admirable device, and, as far as I have learned, no chemist has hitherto explained the philosophy of this carbon door, and why the puddler, instead of using an ordinary furnace door so easily applicable, always has an iron ledge or shelf fixed in front of his fire hole, on purpose for thus blocking it up with coal. The "stuff hole" is also carefully stuffed with coal at the same time.

Thus, it appears, that when iron is unprotected by combined carbon, it is oxidised not merely on its surface but through its whole substance, if exposed at a sufficiently high temperature, and for a sufficient length of time to the action of atmospheric oxygen. The possibility of such internal oxidation no longer presents a difficulty, the researches of Deville, Troost, and Graham having proved that red-hot iron is permeable by certain gases.

The Bessemer process illustrates the protecting action of combined carbon in a very striking and interesting manner. Mr. Riley mistakes in supposing that weldable

Bessemer iron contains no carbon. I have made repeated analyses of Bessemer metal in which the carbon is reduced to the lowest proportion compatible with coherence, and find that this limit is reached when the carbon is reduced to about 0·2 per cent in Bessemer metal prepared in the usual manner. The general practical limit is 0·25 per cent.

This is well known to all practical manufacturers who are at all acquainted with the chemical composition of their steel.

With less than 0·2 per cent of carbon, the Bessemer iron crumbles under the hammer, and if nearly free from carbon, it behaves like a piece of coarse sandstone. This is the ordinary condition of the iron at the termination of the "blow," and before the speigeleisen is introduced. It is then a burnt iron of very exaggerated type, and is quite useless until it has been unburnt by the carbon of the speigeleisen.

The introduction of the speigeleisen displays in a very beautiful manner the presence of the intermingled oxide. When the fused speigeleisen is poured into the converter containing the fused material, which I shall call the melted burnt iron, a violent ebullition is visible, jets of carbonic oxide are forced through this burnt iron, and a huge blue carbonic oxide flame pours out of the mouth of the converter. The quantity thus poured forth fills completely the mouth of the converter; it is perfectly obvious that no air is entering at the same time, the blowing has ceased, and thus the only possible source of supply of oxygen for the formation of this great volume of carbonic oxide is within this melted burnt iron. If the quantity of speigeleisen thus introduced is insufficient to supply a minimum surplus of 0·20 to 0·25 per cent of carbon, the reduction of the free oxide is not completed, and the resulting product breaks under the hammer from mere want of complete coherence.

In like manner, when the spongy balls of iron from a puddling furnace are first crushed down by the hammer, jets of carbonic oxide (to which the workman applies the universal name of "sulphur") issue forth. I have observed that this takes place most abundantly when the ball has stood for some time waiting its turn for the hammer, and believe that it is caused by the commingling of the superficial burnt iron with the inner iron which remains unburnt, and still retains a small portion of carbon.

The manufacture of pure iron by the ordinary processes is practically impossible, for immediately all the carbon is gone oxidation commences, and iron quite free from carbon will not bear the exposure which is necessary in the processes of forging, still less of fusing. By working in an atmosphere of carbonic acid, pure iron might be obtained.

The amount of carbon necessary to prevent the iron from becoming burnt varies with the severity of the oxidising exposure. The case of the Bessemer iron above cited is that of the maximum severity of exposure. The idea sometimes expressed that chemically pure iron would be unfit for commercial purposes is, I believe, a great fallacy. The nearer it approaches to purity the greater its value for all purposes where toughness, malleability, and ductility are demanded. The great art of producing good armour-plates is that of working exactly up to the point where the carbon is reduced to its uttermost minimum, and where another re-heating, even to bright redness, would produce burnt iron, the iron being also free from other impurities, especially phosphorus.

I have already shown that burnt steel does not contain the free oxide which by its presence destroys the continuity and cohesion of burnt iron. What, then, is the nature of the burning process in this case, and how does it produce the observed mechanical changes? The following facts may assist us to the required explanation.

When engaged at the works of Sir John Brown and Co., at Sheffield, I frequently had to determine the carbon of rails, tyres, plates, &c., made from known samples of Bessemer steel, from ingots of which I had already determined and recorded the percentage of carbon. In every case, where I had reliable opportunities of identifying the raw material, I found that the finished rail, &c., contained less carbon than the ingot from which it was made. The amount of difference, however, was variable—was usually the greatest when the steel was the hardest, *i.e.*, when it contained the largest proportion of carbon. This reduction of the quantity of carbon varied from 0·02 to 0·10 per cent. The extreme of these limits was reached in the case of some hard steel plates, which, on account of their excessive hardness, were objects of special investigation.

Now, the steel of these plates, or the ordinary rails, &c., are only heated to bright redness, then hammered, heated again, and rolled.

Another case. Some very brittle plates were made by rolling an unusually hard sample of Bessemer steel, containing 0·75 per cent of carbon. Being engaged in some experiments on annealing, I secured some pieces of this plate, and heated them for periods varying from six hours to four days in an annealing furnace where the temperature was purposely retained at a dull red heat. I then examined them mechanically and chemically—determined the carbon of their "skin" and of their interior at different depths from the surface. By the skin, I mean the clean metal surface, after the scale was removed. This skin was shaved off for analysis by means of a flat-faced drill.

I found that the skin of the piece of plate that had been heated during four days contained no carbon, or only a trace too small for determination, that this absence of carbon continued to a sensible depth, to about the fiftieth of an inch. (Having no means of accurate measurement, this estimation is but an approximation). Below this, a sensible amount of carbon was detected, and this rapidly increased till, at a depth of about one-eighth of an inch, it amounted to 0·72 per cent, and this without any sensible variation continued to the middle of the plate (which was half-inch thick), and on to within about one-eighth of an inch of the other side.

All the other plates suffered a loss of carbon from their surface, but in lesser degrees, corresponding to the time to which they had been heated.

I made several repetitions of such experiments, and found that, by exposing steel to open fires, the removal of the carbon from its surface and interior was much more rapidly effected. I do not bring forward these experiments as novelties, as I believe that similar experiments have been made by others. They show that the carbon of steel may be oxidised and removed without fusion or *visible* combustion; that a slow combustion of carbon takes place even at a low red heat, and that this combustion, like the opposite process of cementation, is not limited to the surface, but proceeds gradually inwards.

The permeability of red-hot steel by oxygen and carbonic oxide enables us to understand the otherwise mysterious action of the interior oxidation of the carbon. Under favourable circumstances, it is probable that a considerable portion of the carbonic oxide thus produced may remain occluded in the iron.

Let us now suppose that we have a piece of steel at the welding or pasty heat, just at the temperature most favourable for the most rapid endosmosis of oxygen and the exosmosis of carbonic oxide, and that while these actions are in full progress, we suddenly cool the steel, and arrest the possible occlusion of the carbonic oxide. The result would be the production of a certain degree of molecular disintegration, and porosity of the steel, the minute arrested bubbles of carbonic oxide or oxygen producing a similar effect upon the cohesion of the burnt steel, and that of the particles of oxide upon the burnt iron.

The "toad's eyes," or conchoidal facets of the so-called crystals, are in strict accordance with this explanation, as also is the fact that burnt steel may be cured by re-heating and hammering, or rolling at a welding heat, as at this temperature, and subjected to such pressure, the separated faces of the bubble holes would be welded together, and the gases either occluded or driven out.

All steel presents a "grain" or so-called crystalline

structure, and this varies in its degree of coarseness. In ordinary hardened steel the grain is coarser than in the same quality of steel when tempered or softened.

My own conviction is that all steel and all iron is of spongy structure, that such a structure may even be common to all metals, that the so-called crystalline fracture of iron and steel is not due to crystallisation at all, but to a visible degree of porosity, and that the fibrous structure of rolled iron is merely due to a different arrangement, an elongation of these mechanical pores. By pores and porosity I do not mean those imaginary interspaces between imaginary atoms in the midst of which our modern mathematicians so luxuriously revel, but actual, visible, and demonstrable spaces.

A further statement of the grounds upon which this view of the constitution of metals is based would carry me beyond the proper limits of this paper, and, therefore, with the permission of the President and Council, I will bring this subject before the Society on some future evening.

Communications from the Laboratory of the London Institution. No. I. "On the Formation of Sulpho-Acids." By Henry E. Armstrong.

Occupied with an investigation into the constitution of sulphuric acid, the views on which are, as is well known, even at the present moment, extremely conflicting, my attention was soon drawn to the body discovered by Professor Williamson—viz., chlorhydric sulphate (sulphuric chlorhydrate, sulphuric oxychloride), SO_2HOCl, and particularly by a somewhat remarkable reaction noticed by Karl Knapp,* who found that the chief product of its action on benzol was sulpho-benzid, sulpho-benzolic chloride and sulpho-benzolic acid being formed, but in relatively very small quantity.

$$2C_6H_6+SO_2HOCl = \left\{\begin{matrix}C_6H_5\\C_6H_5\end{matrix}\right\}SO_2+OH_2+HCl.$$

$$C_6H_6+SO_2HOCl=SO_2C_6H_5Cl+OH_2.$$

$$C_6H_6+SO_2HOCl=C_6H_5SO_2HO+HCl.$$

Whereas it was presumable that sulpho-benzolic acid would be the main product, the reverse was the case!

This led me to commence a series of experiments to determine, if possible, the conditions under which the one or the other of the above reactions took place, and to arrive at a general expression for the action of chlorhydric sulphate on organic bodies.

Action of SO_2HOCl *on Brombenzol*, C_6H_5Br.—Monobrombenzol, 1 eq., diluted with carbonic disulphide, was mixed with the chloride, 1 eq.; moderate reaction took place, HCl being evolved in quantity. After warming on the water-bath for a short period, the CS_2 was distilled off, and the product mixed with water; a brownish, semi-solid mass separated out, and was removed by filtration; the filtrate was neutralised by baryta-water, and heated to boiling, whilst a current of CO_2 was passed into it to remove excess of baryta. On cooling, a difficultly soluble barium salt crystallised out in magnificent white iridescent plates, which were pressed and dried between bibulous paper and analysed.

1·4032 grms. lost 0·035 grm. on drying at 100°=8·6 per cent OH_2.

0·4494 grm. dry salt gave 0·1723 grm. $BaSO_4$=22·5 per cent Ba.

These numbers correspond well with the formula $(C_6H_4BrSO_3)_2Ba+3aq.$, which requires 8·1 per cent OH_2 and 22·6 per cent Ba.

The portion insoluble in water was washed with cold alcohol, to free it from adhering brombenzol, and then dissolved in hot alcohol; on cooling, it crystallised in fine, long, white, glistening needles, which, after pressing and drying at 100°, were analysed, with the following results:—

0·4271 grm. gave 0·607 grm. CO_2 and 0·0974 grm. OH_2=38·7 per cent C and 2·5 per cent H.

The formula— $\left\{\begin{matrix}C_6H_4Br\\C_6H_4Br\end{matrix}\right\}SO_2$ (dibrom-sulpho-benzid), requires 38·3 per cent C and 2·1 per cent H. When dry, this body forms white silky needles, soluble in hot, difficultly soluble in cold, alcohol; it melts at 168°, and solidifies again at 149°.

The action on brombenzol is, therefore, somewhat different to that on benzol; bromsulpho-benzolic acid is the main product, the yield of dibromsulpho-benzid not exceeding 20 per cent. Bromsulpho-benzolic chloride was formed, if at all, in but very small quantity.

Whether the bromsulpho-benzolic acid thus obtained is identical with that prepared by the action of concentrated sulphuric acid on brombenzol I cannot state, not having compared them directly.

Action of SO_2HOCl *on Nitrobenzol*, $C_6H_5NO_2$.—The two were mixed, as before, in the proportion of 1 eq. of each, but without the addition of CS_2. In this case the action was extremely sluggish, and it was only after long-continued warming that the evolution of HCl ceased. On mixing with water, a small quantity of a black, tarry mass remained, which may possibly contain dinitro-sulpho-benzid. The solution was converted into the barium salt; but, as this was exceedingly impure, it was dissolved, together with a quantity of baric hydrate, and the solution saturated with hydrogen sulphide. After boiling the solution, the barium was carefully precipitated by SO_4H_2, and the filtrate evaporated to crystallisation. By this means, the very impure nitro-sulphobenzolic acid first formed was reduced to the amido-acid, which could more easily be purified.

According to Schmitt,* by the action of concentrated sulphuric acid on nitrobenzol, and reduction of the product to the amido acid, an amido-sulpho-benzolic acid is obtained isomeric with sulphanilic acid, prepared from concentrated sulphuric acid and amido-benzol (aniline). His acid crystallises with 1½ molecules aq., sulphanilic with one molecule only. My product, I believe, is not sulphanilic acid; it is much too soluble in water, and crystallises differently; but, on the other hand, it crystallises with one molecule aq. only:—

1·2555 grms. lost 0·1153 grm.=9·1 per cent OH_2.

$C_6H_4NH_2HSO_3+aq.$ requires 9·4 per cent; so that, until it has been compared directly with Schmitt's acid, it is impossible to say with certainty whether, by the action of SO_4H_2, and of SO_3HCl, on nitrobenzol, the same, or isomeric, sulpho acids are produced.

Action of SO_2HOCl *on Nitrophenol*, $C_6H_4NO_2HO$.—Nitrophenol (the volatile modification, melting-point 45°), 1 eq., mixed with CS_2, was mixed with 1 eq. of the chloride; immediate action took place, and large quantities of HCl were evolved. After distilling off the CS_2, the product was entirely soluble in water.

The excess of sulphuric acid was removed by $PbCO_3$, the excess of lead by SH_2, and the solution then neutralised with potassic carbonate and evaporated to crystallisation. In this way, the very characteristic yellow needles of the di-potassic nitrophenol-sulphate—

$$C_6H_3NO_2OKSO_3K+2aq.$$

were obtained; and from a solution of this salt, on the addition of acetic acid, the tufts of light yellow needles of the potassic nitrophenol-sulphate, $C_6H_3NO_2OHSO_3K$.

Very remarkable is the action of the chloride on the non-volatile modification of nitrophenol. According to Kekulé, whereas the volatile modification is readily converted into the sulpho acid by the action of concentrated sulphuric acid, it is not possible to prepare a sulpho-acid from the isomeric, non-volatile modification. The same nitro acid was prepared by Kolbe and Gauke, by nitration of paraphenol-sulphonic acid; they could not, however, obtain a nitro acid by the action of nitric acid on meta-phenol-sulphonic acid. I had hoped to obtain such an acid by the action of SO_2HOCl, but it is not so.

When mixed in equivalent proportions, with the addition of CS_2, immediate action takes place, and large quantities of HCl are evolved. On the addition of water, an oil separated, which dissolved almost entirely in hot water;

* *Zeitschrift für Chemie*, 1869, 41.

* *Ann. Chem. Pharm.*, vol. cxx., p. 163.

but from the solution, on cooling, the characteristic long white needles of nitrophenol crystallised out. The filtrate contained only sulphuric acid and traces of nitrophenol.

The sulpho acid, if formed—and it seems to me beyond doubt that it is—in the first instance, is evidently decomposed by water into nitrophenol and sulphuric acid.

It may, perhaps, be possible to isolate it by appropriate treatment.

Action of SO_2HOCl *on Naphthaline.*—1 eq. naphthaline, dissolved in CS_2, was acted on by 1 eq. SO_2HOCl; violent action took place, accompanied by evolution of HCl. The reaction was terminated, after distilling off the CS_2, by heating for some time at 100°. The residue remaining, after treatment of the product with water, consisted almost entirely of naphthaline, since it was volatilised on boiling with water. The naphthaline compound, analogous to sulpho-benzid, is, therefore, not formed in any appreciable quantity. From the aqueous solution a barium salt was obtained containing 24·4 per cent of barium. Baric sulpho-napthalate requires 24·7.

To decide, if possible, whether both the α and β modifications of sulphonaphthalic acid were formed, the calcium salt was prepared and a determination of the water of crystallisation made. The β salt is anhydrous, whereas the α salt crystallises with 2 molecules aq., corresponding to 7·3 per cent; my salt contained 3·6 per cent, and, I believe, therefore, was a mixture of the two isomeric salts. In all probability, the α acid first formed is converted by subsequent heating into β acid. The anhydrous Ca salt contained 8·8 per cent Ca; required, 8·7 per cent.

By the action of 2 molecules SO_2HOCl on 1 molecule naphthalin a disulpho-acid is formed.

The conclusion to be drawn from the above few experiments is, it seems to me, the normal action, so to speak, of SO_2HOCl is to form a sulpho-acid, the Cl of the chloride removing H from the body acted upon and replacing it by the group SO_3H; it is only under certain conditions that both Cl and HO are removed from the chloride, and a sulphobenzid analogous compound formed. What these conditions are I hope to establish by a further more extended series of experiments.

"*On a Water from the Coal Measures at Westville, Nova Scotia.*" By Professor How.

In a paper published in the *Journ. Chem. Soc.* for May, 1870, I described an acid water from the Acadia coal mines at Stellarton, Pictou County, Nova Scotia. The subject of the present communication was obtained from a neighbouring mine in the same county, the Black Diamond Colliery, situated at Westville, a new village about 2 miles west of the former place. This water differs from that got at Stellarton, which was partly of surface origin, in having been taken from a coal-pit; and while that furnished a good illustration of the evils attending the use of such a water in boilers, this gives an opportunity of comparing a water from the productive coal measures with those arising under different geological conditions. In 1865, I described to the Society (*Journ. Chem. Soc.*, vol. xviii., p. 44) a dense brine from Salt Springs, Pictou County, which, though containing more salt than the others, as yet ascertained, probably shows the general character of the brines issuing from the lower carboniferous gypsiferous districts of this province, since it agrees with another of them, from Walton, a distant locality, as I have shown (*Trans. N. S. Inst.*, 1865; and "Mineralogy of Nova Scotia," p. 143), in containing sulphate of calcium as the most abundant ingredient after chloride of sodium, and smaller quantities of chloride and carbonate of magnesium and calcium, though the amounts of these are not alike, either absolutely or relatively. There are other waters arising in these gypsiferous districts in which sulphates are the chief constituents, chlorides being nearly absent. For example, the Spa Spring water of Windsor, Hants County, contained, when I analysed it in 1858, solid matter in the imperial gallon to the amount of 138 grains, of which about 106 were sulphate of calcium, 12 consisted of sulphates of magnesium, sodium, and potassium, and only 0·9 of any chloride, that of sodium (CHEMICAL NEWS, vol. ix., p. 98). These eminently sulphated waters present interesting contrasts with those in which chlorides are by much the preponderating constituents, as the saline water of Bras d'Or, Cape Breton, analysed by myself (CHEMICAL NEWS, vol. ix., p. 97); the brines of Onondaga, New York, examined by Professor Gössmann (*Silliman's Journal*, July, 1867); several others described by Dr. Hunt ("Geology of Canada," 1863, p. 53; and "Contributions to the Chemistry of Waters," *Silliman's Journal*, 1865); and the Wheal Clifford water, analysed by Professor W. A. Miller (CHEMICAL NEWS, vol. x., p. 181), and also with the Kissengen water at Harrogate, examined by Dr. S. Muspratt (CHEMICAL NEWS, vol. xiv., p. 49, and xv., p. 245); and those described by Dr. Hunt (*loc. cit.*), in which chlorides are abundant and sulphates are absent. All these waters, excepting that from Harrogate, which is from rocks at the junction of the permian with the carboniferous, issue from systems older than the carboniferous, and we see the same features presented by waters of intermediate times, for the brines of the valley of the Alleghany river, obtained from borings in the coal formation, are remarkable for containing large proportions of chlorides of calcium and magnesium, though the sum of these is never equal to more than about one-fourth of the chloride of sodium. The presence of salts of barium and strontium in these brines, and the consequent absence of sulphates is, according to Linney, a constant character in this region over an area of 2000 square miles (Bischof, "Chemical Geology, vol. i., p. 377). In this curious circumstance, these waters agree with the Kissengen water and several of the Canadian waters described (*ubi supra*) by Dr. Hunt.

The water now to be described was mentioned as having been taken from the productive coal measures. These constitute the middle coal formation, or the coal measures proper, of this province, according to the arrangement of Dr. Dawson, who says that this series includes the productive beds of coal, and is destitute of properly marine limestones. Beds tinged with peroxide of iron are less common in this formation than in any of the others of the system. Dark-coloured shales and grey sandstones prevail, and there are no conglomerates ("Acadian Geology," 2nd edition, p. 130). These beds are separated from the gypsiferous group before referred to by the underlying millstone grit series, and they represent the lower coal measures of the United States, and in part the coal formation of Great Britain.

Two samples of water were taken from the same pit at the Black Diamond Colliery under somewhat different circumstances, and sent to me for examination, in November, 1870, by A. W. Greig, Superintendent of the Nova Scotia Coal Company, who was desirous of ascertaining whether the water would answer for use in his boilers. The waters were bright and clear, they had no odour, their taste was that of good spring water; they gave, respectively, the following results:—

	Grains in the imperial gallon.	
	No. 1.	No. 2.
Silica	0·63	0·46
Carbonate of calcium	11·55	10·59
,, magnesium ..	3·67	3·57
,, iron	traces	traces
Chloride of sodium	0·84	1·17
Sulphate of potassium	1·14	1·58
,, sodium	4·17	2·50
Phosphoric acid	none	none
Organic matter	none	traces
Carbonate of sodium	3·55	3·35
	25·55	23·22
Free carbonic acid	undetd.	undetd.
Specific gravity	1000·459	1000·339

Hence it appears that the waters were essentially the same. They had an acid reaction on litmus, the paper regained its blue colour on drying, they gave off a good deal of carbonic acid gas, and deposited carbonates of calcium and magnesium on boiling. After two or three hours' boiling, nearly all the calcium was thrown down, and on concentration a mere trace remained dissolved. The waters soon acquired an alkaline reaction on evaporation; when concentrated, they coloured turmeric deep brown, effervesced with acid, and contained sulphates, alkalies, and a notable quantity of magnesia.

The waters, being one and the same, may be spoken of as the water from Westville, and, of course, in reference to the condition after loss of the carbonic acid, which renders the carbonates present bicarbonates, as an alkaline water. It is interesting to find the composition of this water so totally unlike that of any of the waters of this province yet analysed, though this was to be expected, considering the different geological conditions under which they have been examined. The small quantity of chlorides may have some relation to the absence of marine limestones in the productive measures mentioned by Dr. Dawson, and is in very strong contrast with that in the brines from the coal formation of the Alleghany River district referred to above. In the waters containing carbonate of sodium, analysed by Dr. Hunt, which form his fourth class (*loc. cit.*), chlorides are sometimes absolutely and relatively more abundant than in this case, but, on the whole, this is the class to which the Westville or Black Diamond Colliery water belongs.

NOTICES OF BOOKS.

Select Methods in Chemical Analysis (Chiefly Inorganic). By William Crookes, F.R.S., &c. London: Longmans, Green, and Co., 1871.

It will be perceived from the title of this work that the author has not intended to provide the student with a complete text-book of analysis, but rather with a laboratory companion, containing information not usually found in ordinary works on Analysis. The book is essentially a reprint of the important articles on inorganic chemistry in the first twenty volumes of the Chemical News, verified, condensed, and arranged in proper order; and as some of these have proved to be of great value, it was thought that a service would be rendered to analytical chemistry if these trustworthy methods of analysis were systematically arranged in a convenient form for laboratory use. In some instances the descriptions are given in the language of the original writer, but in all cases where the author has improved the processes, the necessary modifications have been introduced.

It is strange that modern works on analysis should ignore about twenty of the elements. Even Fresenius gives only a separate form for their detection. Were investigators more in the habit of looking for the "rare" elements, they would no doubt turn up unexpectedly in many minerals. In this work equal prominence is given both to the rare and to the ordinary elements.

The order in which the analytical separation of the metals is carried out will be readily understood. Take, for instance, the case of copper. After giving the best method for the detection and quantitative estimation of this metal, comes a description of the processes for separating it from those metals which have been previously passed under review, as mercury, silver, and zinc; but no attention is paid to the separation of copper from such metals as lead, tin, &c., which have not previously been treated of. Under the respective headings "Lead" and "Tin," the separation of these metals from copper is described.

A complete list of separations has not been attempted. Where no process of separation or estimation is given, it may be inferred that the author has had no experience in any but the well-known methods employed in most laboratories; and to have introduced these ordinary processes into the work simply for the sake of filling up gaps would have largely increased its bulk without adding materially to its value. To save space, the description of a process is frequently discontinued at the point where the substances under separation are brought to such a state that the concluding steps are obvious.

No special system of weights and measures has been employed; many of the descriptions having been condensed from the original memoirs, it was thought better to retain the system therein adopted, so as to have simple numbers to deal with, instead of having to convert them to one common scale and to introduce decimals; thus—when an author says take 8 grains of a substance, 0·51816 gramme has not been substituted; and where 10 grammes are mentioned, he has not put 154·3840 grains. When not otherwise expressed, all degrees are according to the centigrade scale. Formulæ have been avoided as far as practicable.

CORRESPONDENCE.

COMMERCIAL ANALYSIS.

To the Editor of the Chemical News.

Sir,—I am very glad that Mr. Purser has taken up the subject of commercial analysis as regards one of the commonest substances sold by analysis, namely "superphosphate of lime."

I have been extremely puzzled on many occasions by the discordant results obtained on a well-prepared sample sent to three or four of the leading analysts in the country. In a previous communication to you, on the so-called "reduced phosphates," I advocated the plan that a definite process should be adopted among all commercial analysts, for soluble phosphates, so as to obviate the anomalies which now exist and which tend to bring the practice of commercial analysis, to say the least of it, into ridicule. I would again urge upon those concerned to do away, if possible, with the disgraceful distinction of a "buyer's" and "seller's" analysts. A case occurred to myself a few days ago, in which a sample was well prepared, and divided into two parts, then sealed up in tins, and sent to two well-known men. One reported 21·39 per cent soluble phosphate; the other 26·36. The low report I proved most conclusively to be the correct one. The manure, however, was sold by the analysis, which gave 5 per cent more, and consequently the buyer was requested to pay fifteen shillings per ton more for it than it was worth. Hoping you will lend your aid to the destruction of this system. I am, &c.,

Francis Sutton.

Norwich, April 17, 1871.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Les Mondes, March 16, 1871.

Annuaire du Bureau des Longitudes.—The publication of this small, but extremely useful volume (the eightieth) of a collection not so sufficiently known as it deserves to be, is announced with a few words. The price is, as usual, about two francs; the publishers are MM. Gauthiers-Villars, 55, Quai des Augustins, Paris.

Photographic Post.—Rev. F. Moigno.—This lengthy paper contains an account of this ingenious and useful application of science made during the late siege of Paris.

Urtica Tenacissima.—Dr. Dudouy.—The author, an agriculturist and landed proprietor, states that the plant just named, from which a textile fibre is prepared known under the name of *Ramié* or *Ramaï*, is now successfully cultivated in a portion of Southern France, and also in Algeria. The plant is a native of Java, and a variety of the *Urtica nivea*, which produces the fibre known as China grass. The author states that, from some few experiments made, he is of opinion that this plant, the *Ramié*, might be advantageously grown in the more northerly parts of France. The fibre obtainable from the *Urtica tenacissima* is highly valued for the weaving of fine fabrics—muslins, poplins, &c.—while the remainder of the plant yields excellent fodder for cattle.

Preservation of Potatoes.—Rev. F. Moigno.—The tubers, after having been dug up, which operation should not be done before they have attained proper maturity, may be readily preserved in a sound state by any of the following means:—The tubers are put in pits dug in the soil, and mixed with either charcoal powder, gypsum, ash of coal, peat, or wood, and then kept from the action of direct sunlight, and buried at least half a metre, better 2 metres, under the surface, care being taken that there is proper drainage for water, and some straw placed in the pits.

Leaves of Fir-Trees (Pinus kinds) as Food for Sheep.—V. Chatel.—The young soft twigs of the various species of *Pinus* may, according to experiments made by the author, be mixed with the food of sheep in moderate quantity, being for these animals a healthy stimulant.

Signals for Communication between Fortresses and Distant Armies.—Dr. Le Verrier.—This brief abstract calls attention to a lengthy paper containing the record of a series of experiments made for the Chief Committee of Defence of the Valley of the Rhône, Lyon, for the purpose of communicating, by luminous signals, at distances of 30 to 100 kilometres. It appears that, under the guidance of the well-known *savant* just named, excellent results have been obtained, and so simple in execution that ordinary workmen can satisfactorily manage the signals. The rather considerable expenses of these researches have been defrayed by a private individual, M. Maistre, of Villeneuvette, near Clermont, Hérault.

Sudden and Spontaneous Opacity of the Gas Used to Fill Air Balloons.—Dr. de Fonvielle.—It appears that aëronauts have frequently observed a phenomenon which they term as *le ballon fume sa pipe*—that is to say, the issuing of the gas the air balloon is filled with from the lower opening of the balloon in the form of a whitish smoke. The author supposes that this phenomenon is due to the cold produced by the increase of the volume of the balloon while rising rapidly upwards. Indeed, for a decrease of pressure of 1 millimetre per second of time, the increase of volume is equivalent to that produced by an elevation of temperature of two degrees for a height where the mean pressure would be 660 millimetres of mercury. A similar phenomenon takes place in every instance of the rapid rising of a mass of humid air, which, while becoming dilated, does as the balloon and *fume sa pipe*—that is to say, leaves behind it, in the shape of more or less dense cloud, the watery vapour which it contained previously in the shape of a diaphanous invisible gas.

Moniteur Scientifique, March 1 and 15, Nos. 341 and 342 (double number), 1871.

Review of the Principal Labours of the Late Dr. Pelouze.—Professor J. Dumas.—This lengthy memoir is, notwithstanding its very great intrinsic value, not well suited for any useful abstraction.

Preservation of Raw Butchers' Meat, Fish, Vegetables, Eggs, and other Food by means of Phenicated Water at from one-half to 4 and 5 per cent, according to the Length of Time and the Climate wherein the Preservation is Desired.—Dr. G. Déclat.—The process employed by the author is simple in its execution, since it only consists in first immersing, for some length of time, of the substances alluded to in aqueous solutions, of varying strength, of perfectly pure carbolic acid, and next drying the materials. The process has been tried, and found successful of late, in Paris, with various foods, including vegetables.

Preservation of Meat and other Animal Substances.—E. Pelouze.—The meat, or other animal substance, first cut into pieces of a convenient and not too large size, is introduced into an apparatus wherein it is kept for some time in an atmosphere of carbonic oxide gas under some pressure; and next, the material so treated is dried in a current of dry cold air, so as to remove all traces even of moisture from the substance; after which the meat is superficially treated with an antiseptic solution—either a concentrated brine, or solution of saltpetre or phenicated water—and then packed in hermetically-sealed vessels.

Some Reflections on the State of Science in France: Why did not France find Superior Men at the Time of Danger?—L. Pasteur.—This excellent essay throws prominent light on the disasters which the country alluded to has suffered for the last nine months.

Pigeon Postal Service during the late Siege of Paris.—J. Fleury-Hermagis.—An intelligently written, and, in many respects, valuable paper.

Journal für Gasbeleuchtung und Wasserversorgung, No. 3, 1871.

Hydrostatico-Galvanic Gas Lighter.—Dr. W. Klinkerfues.—From the brief and, as yet, incomplete notice here communicated, it appears that the author, Director of the Astronomical Observatory at Göttingen, has succeeded in contriving an expeditious, automatic, and inexpensive apparatus, whereby the lighting and extinguishing of the gas lights in public lamps becomes automatic and almost instantaneous. Experiments made on sufficiently large scale prove the practicability of this invention, which will be an important improvement in street lighting.

Scientific Hydrotimetry; and on the Necessity of Hydrotimetrical Researches.—Dr. H. Trommsdorff.—This lengthy monograph is valuable to waterworks' engineers and managers; it is not, however, suited for useful abstraction.

Apparatus for Manufacturing Gas from Tar.—M. Craken.—With a woodcut.

Water Supply and Waterworks at Posen.—F. Moore.

Revue des Cours Scientifiques de la France et de l'Etranger, Nos. 42 to 50, inclusive (from September 17, 1870, to March 4, 1871).

These numbers do not contain any original papers or communications bearing upon chemistry and allied sciences.

NOTES AND QUERIES.

Chromates and Phosphates.—Can any of your readers inform me of the names of the manufacturers of chromate of potash (not bichromate), also phosphates of ammonia and of potash; or I would be glad if any maker would send his address to me, under cover to you.—Nemo.

Griffin's "Handicraft."—(1.) Will your readers kindly inform me if Griffin has published any more of his promised Handicraft series—that is to say, any volume besides the "Chemical Handicraft"? (2.) Can they also give me the address of Mr. George Norman, the well-known microscopist of Hull?—S. G.

Ammonia Sulphate from Gas Liquor.—(Reply to "Ammonia.")—Practical information on the manufacture of the above article may be obtained on applying to Dr. Versmann, Chemical Laboratory, 150, Fenchurch Street.

Ammonia Sulphate from Gas Liquor.—If the person who signs himself "Ammonia" in your last week's impression will write to me, giving me the quantity of gas liquor he obtains per week, to be worked into sulphate of ammonium, I will give him, with full particulars, all the practical information he will require—firstly, as to plant, and secondly as to process, &c.—in order to manufacture this salt perfectly white and almost perfectly pure, which may be done as easily and inexpensively as to make it discoloured and impure. I will state my terms when I hear from "Ammonia."—Watson Smith, F.C.S., Prestolee Alkali Works, Stoneclough, near Manchester.

Watering Streets.—Your correspondent "C. T." will find H. O. W. Cooper and E. F. Cooper took out a patent for watering streets with "a liquid or composition (of a deodorising nature) mixed with water," and claims the use of salt mixed with deliquescent chlorides, September 2nd, 1867 (No. 2482); but I am not aware if the patent is still in existence, which depends upon whether the requisite £50 was paid on or before September 2nd, 1870, which he can ascertain for nothing at the Patent Office, or through any patent agent for a small fee. Another patent on the same subject is No. 3337, November 26th, 1867; but the plan is different, and it includes "travelling furnaces for liquefying snow."—Nemo.

MEETINGS FOR THE WEEK.

Monday, 24th.—Medical, 8.
—— London Institution, 4. Mr. R. A. Proctor, F.R.A.S., "On Astronomy." (Educational Course.)
—— Royal Geographical, 8.30.
Tuesday, 25th.—Ethnological, 8.
—— Civil Engineers, 8.
—— Royal Institution, 3. William Pengelly, Esq., F.R.S., F.G.S., "On the Geology of Devonshire, especially of the New Red Sandstone System."
Wednesday, 26th.—Society of Arts, 8.
—— London Institution, 12. Annual Meeting of Proprietors.
—— Geological, 8.
Thursday, 27th.—London Institution, 7.30. Prof. Bentley, "On Economic Botany."
—— Royal Society Club, 6.
—— Royal, 8.30.
—— Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
Friday, 28th.—Royal Institution, 9. Prof. Odling, F.R.S., "On the Revived Theory of Phlogiston."
—— Quekett Microscopical Club, 8.
Saturday, 29th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."
—— Zoological, 1. Anniversary.
—— Quekett Microscopical Club. Excursion to Wandsworth Common; To meet at Clapham Junction at 3 o'clock.

TO CORRESPONDENTS.

W. H. Wood.—Your pamphlet shall be noticed in a future number.

Hugh Burgess, Pennsylvania.—We will communicate with the querist.

P. Holland.—We know of no work of more recent date than Reimann's "Aniline and its Derivatives."

W. H. Walenn.—We are obliged for the communication.

J. S. Skidmore.—Noad's "Quantitative Analysis," recently reviewed in our columns.

J. Blyth.—Thanks for the paper; we shall have great pleasure in inserting it.

Prof. A. Bauer.—Received with thanks.

BOOKS RECEIVED.

Experiments on the Prevention of the Development of Fungi in Aqueous Solutions of Tartaric Acid, and Theories of the Origin of, and Conditions Essential to, Life. By W. H. Wood. Middleshrough: Burnett and Hood.

The Discovery of the Nature of the Spleen, from an Investigation of the Lateral Homologies of the Liver, Stomach, and Intestinal Canal. By Henry R. Silvester, B.A., M.D. Lond. London: John Churchill and Sons.

Letters on Vaccination. By William Woodward, M.D. Worcester: Deighton and Son.

Second Annual Report of the State Board of Health of Massachusetts, January, 1871. Boston: Wright and Potter.

Preparatory Programme of the National University for Industrial and Technical Training.

On the Composition of the Water of the Irish Sea. By T. E. Thorpe, Ph.D., and E. H. Morton.

A Sketch of Romance of Motion; also a Hypothetical Analysis and Synthesis of Nitrogen. By Alec Lee. London: Longmans, Green, and Co.

Annual of Scientific Discovery: or Year-Book of Facts in Science and Art, for 1871. Edited by John Trowbridge, S.B., aided by W. R. Nichols, and C. R. Cross. Boston: Gould and Lincoln. London: Trübner and Co.

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THE CHEMICAL NEWS.

VOL. XXIII. No. 596.

CONTRIBUTIONS TO THE HISTORY OF ORCIN.

No. I.—NITRO-SUBSTITUTION COMPOUNDS OF THE ORCINS.*

By JOHN STENHOUSE, LL.D., F.R.S., &c.

THE action of nitric acid upon orcin has been studied by several chemists, but with comparatively negative results. Schunck† in this manner obtained a red resinous substance, which, by further treatment with the acid, was oxidised to oxalic acid; and in 1864 De Luynes‡ found that orcin dissolved in cooled fuming nitric acid without evolution of nitrous fumes, and that the addition of water precipitated a red colouring matter; the long-continued action of the vapour of fuming nitric acid on powdered orcin likewise produced a red dye apparently identical with the above. These, however, were resinous uncrystallisable substances.

Although under ordinary circumstances only resinous products are obtained by treating orcin with nitric acid, yet, when colourless orcin in fine powder is gradually added to strong nitric acid, cooled by a freezing mixture, it dissolves with a pale brown colouration, but without the slightest evolution of nitrous fumes. If this solution be now slowly dropped into concentrated sulphuric acid, cooled to −10° C., the mixture becomes yellow and pasty, from the formation of nitro-orcin, which is but slightly soluble in sulphuric acid. When this is poured into a considerable quantity of cold water, the nitro body separates as a bright yellow crystalline powder, quite free from any admixture of resin.

After numerous experiments, the following was found to be the most advantageous process for the preparation of nitro-orcin:—6 grms. of colourless orcin were dissolved in 6 cubic centims. of boiling water, and when the solution had cooled to about 50° C., it was added in small portions at a time, and with constant stirring, to 40 cubic centims. of nitric acid (sp. gr. 1·45) which was maintained at a temperature of about −10° C. by immersion in a good freezing mixture. The solution, which was of a very pale brown colour, was now added, in a similar manner, to 120 cubic centims. of concentrated sulphuric acid, also maintained at −10° C. The pasty mass, after being allowed to stand for fifteen or twenty minutes in the freezing mixture, was poured into a beaker containing 300 cubic centims. of water and 400 grms. of ice; the crude nitro compound was then precipitated as a yellow or orange-coloured granular powder. The orcin employed in the preparation of this nitro compound was colourless, having been purified by distillation *in vacuo*. The yield of crude nitro-orcin amounted to 150 per cent of the weight of the orcin.

The crude nitro-orcin was collected, washed with a little cold water, and purified by one or two crystallisations from boiling water (40 parts). It was thus obtained in large yellow needles, which are readily soluble in hot water, and but slightly in cold; the addition of a strong acid precipitates almost the whole of the nitro-orcin from its cold aqueous solution. It is soluble in alcohol, very soluble in hot benzol, and crystallises out in great part on cooling; it is less soluble in ether, and but moderately so in bisulphide of carbon. It dyes the skin yellow, like picric acid, but is tasteless. It volatilises slightly at 100° C., melts at 162° C., and decomposes with slight explosion immediately afterwards. When heated with concentrated sulphuric acid, it dissolves, forming a deep yellow solution, which deposits crystals on cooling, and is immediately precipitated by water. It dissolves in hot strong nitric acid, with evolution of nitrous fumes and formation of oxalic acid. Like picric acid, when treated with calcium hypochlorite it yields chloropicrin at the ordinary temperature. Its aqueous solutions are coloured dark brown by ferric chloride, and completely precipitated by lead sub-acetate.

The analysis of the substance dried at 100° C. was made with the following results:—

I. 0·335 grm. of the substance gave 0·400 grm. of carbonic anhydride and 0·062 grm. of water.

II. 0·306 grm. of the substance gave 0·366 grm. of carbonic anhydride and 0·060 grm. of water.

III. 0·525 grm. of the substance gave 0·629 grm. of carbonic anhydride and 0·096 grm. of water.

	Theory.		I.	II.	III.	Mean.
C_7	84	32·43	32·58	32·63	32·68	32·63
H_5	5	1·93	2·06	2·18	2·03	2·09
N_3	42	16·22	—	—	—	—
O_8	128	49·42	—	—	—	—
	259	100·00				

These results correspond to the formula $C_7H_5(NO_2)_3O_2$, that of trinitro-orcin. It is a powerful acid, much resembling picric acid, but distinguished from the latter by the greater solubility of its salts. I propose, therefore, to call this new substance *trinitro-orcinic acid*.

Potassium trinitro-orcinate.—This was readily prepared from trinitro-orcinic acid by dissolving it in a warm and rather concentrated solution of potassium carbonate. On cooling it solidified to a crystalline mass of fine needles of a deep orange colour, which, after the removal of the mother-liquors by the vacuum filter, or by pressure, was purified by crystallisation from hot water, in which it was very soluble. The salt, dried at 100°, was submitted to analysis.

0·300 grm. of the substance gave 0·155 grm. of potassium sulphate.

	Theory.		
C_7	84·0	—	—
H_3	3·0	—	—
K_2	78·2	23·38	23·19
N_3	42·0	—	—
O	128·0	—	—
	335·2		

The result obtained agrees with the formula—

$$\left.\begin{matrix} C_7H_3(NO_2)_3 \\ K_2 \end{matrix}\right\} O_2.$$

Sodium nitro-orcinate.—This was obtained by adding nitro-orcinic acid to a strong solution of sodium hydrate or carbonate until nearly neutralised, and purifying by re-crystallisation. It forms orange-coloured microscopic needles, strongly resembling the potassium salt.

Ammonium nitro-orcinate was prepared by adding nitro-orcinic acid to moderately-strong ammonia, in quantity insufficient to neutralise it, boiling for a short time, and then setting aside to crystallise. It forms deep yellow silky needles, which are very soluble in water, but much less so in alcohol.

Barium nitro-orcinate.—Trinitro-orcinic acid was dissolved in 400 or 500 parts of boiling water and an excess of barium carbonate, the pale yellow solution filtered from the excess of carbonate and set aside. On cooling, the solution formed a semi-solid crystalline pap, consisting of fine bright yellow needles of the barium salt. The air-dried salt lost 10·7 per cent at 100°, and became of an orange-red colour, but regained its original colour on ex-

* Read before the Royal Society, March 30th, 1871. A preliminary notice with this title was published in the CHEMICAL NEWS, August 26th, 1870 (vol. xxii., p. 98).

† *Ann. Chem. Pharm.*, vol. liv., p. 270.

‡ *Ann. Chem. Pharm.*, vol. cxxx., p. 34.

posure to moist air. A barium determination was made of the salt dried at 100°.

I. 0·235 grm. of the substance gave 0·139 grm. of barium sulphate.

II. 0·328 grm. of the substance gave 0·194 grm. of barium sulphate.

	Theory.		I.	II.	Mean.
C_7	= 84	—	—	—	—
H_3	= 3	—	—	—	—
Ba″	= 137	= 34·77	34·78	34·79	34·78
N_3	= 42	—	—	—	—
O_8	= 128	—	—	—	—
	394				

The orange-coloured anhydrous salt has the composition—

$$\left.\begin{matrix} C_7H_3(NO_2)_3 \\ Ba'' \end{matrix}\right\} O_2,$$

and the yellow needles—

$$\left.\begin{matrix} C_7H_3(NO_2)_3 \\ Ba'' \end{matrix}\right\} O_2 + 3H_2O.$$

Calcium nitro-orcinate, prepared by neutralising the acid with calcic carbonate, is very soluble in hot water, and crystallises out on cooling in interlaced yellow needles, which are rather soluble in cold water. It is but slightly soluble in hot alcohol, and the solution forms a gelatinous mass on cooling.

Magnesium nitro-orcinate is very soluble in cold water, but less so in spirit. It crystallises in minute orange needles.

Lead nitro-orcinate.—This salt was prepared by dissolving the pure nitro-orcinic acid in 1000 parts of water strongly acidulated with acetic acid, and adding a solution of lead acetate likewise strongly acidulated, when the lead nitro-orcinate came out as a bright yellow crystalline precipitate, consisting of tufts of microscopic needles, which are very tough and difficult to reduce to powder. It is almost insoluble in cold water, slightly in hot; it is soluble in hot acetic acid, from which it crystallises unaltered; it is insoluble in alcohol. If alcoholic solutions of nitro-orcinic acid and lead acetate be mixed, the lead salt is obtained as a yellow amorphous precipitate.

Dried at 100°, it gave the following results:—

I. 0·319 grm. of the substance gave 0·207 grm. of plumbic sulphate.

II. 0·536 grm. of the substance gave 0·348 grm. of plumbic sulphate.

III. 0·339 grm. of the substance gave 0·221 grm. of plumbic sulphate.

	Theory.		I.	II.	III.	Mean.
C_7	= 84	—	—	—	—	—
H_3	= 3	—	—	—	—	—
Pb	= 207	= 44·62	44·34	44·36	44·54	44·41
N_3	= 42	—	—	—	—	—
O_8	= 128	—	—	—	—	—
	464					

Its composition is, therefore, represented by the formula—

$$\left.\begin{matrix} C_7H_3(NO_2)_3 \\ Pb'' \end{matrix}\right\} O_2.$$

Copper nitro-orcinate, formed by dissolving cupric carbonate in a strong aqueous solution of nitro-orcinic acid, was with difficulty obtained in the crystalline state in the form of small reddish-brown needles, which are very soluble in water and alcohol, but are precipitated from their solution in the latter menstruum by ether.

Zinc nitro-orcinate, formed from zinc oxide in a similar manner to the copper salt, is likewise very soluble in water and alcohol, and crystallises in tufts of yellow needles.

Silver nitro-orcinate.—Nitro-orcinic acid was dissolved in fifty times its weight of boiling water, an excess of silver oxide added, and, after boiling a few minutes, filtered from the undissolved silver oxide. On cooling, the whole solidified to an orange-red gelatinous mass of the silver compound, which exhibited no signs of crystallisation. It is moderately soluble in hot water, and its solution gelatinises on cooling. When boiled for any length of time its solution slowly decomposes. Dried at 100°, it gave the following results:—

I. 0·458 grm. of the substance gave 0·277 grm. of argentic chloride.

II. 0·398 grm. of the substance gave 0·241 grm. of argentic chloride.

	Theory.		I.	II.	Mean.
C_7	= 84	—	—	—	—
H_3	= 3	—	—	—	—
Ag_2	= 216	= 45·66	45·47	45·57	45·49
N_3	= 42	—	—	—	—
O_8	= 128	—	—	—	—
	473				

This corresponds to the formula—

$$\left.\begin{matrix} C_7H_3(NO_2)_3 \\ Ag_2 \end{matrix}\right\} O_2.$$

Ethylic nitro-orcinate.—Dry and finely-powdered silver nitro-orcinate was digested with ten times its weight of ethylic iodide until the silver salt was completely decomposed, as shown by the pale yellow colour of the silver iodide formed; the excess of ethylic iodide distilled off, and the ethyl nitro-orcinate extracted from the residue by boiling alcohol, from which it crystallised in bright yellow prismatic needles. One or two re-crystallisations rendered it quite pure. It melts at 61·5° C., and is very soluble in hot alcohol, which, if the solution be concentrated, deposits it as an oil that solidifies on cooling.

The substance dried *in vacuo* was submitted to analysis.

0·189 grm. of the substance gave 0·290 grm. of carbonic anhydride and 0·078 grm. of water.

	Theory.	
C_{11}	= 132 = 41·91	41·85
H_{13}	= 13 = 4·13	4·58
N_3	= 42 = 13·33	—
O_8	= 128 = 40·63	—
	315	100·00

It may, therefore, be represented by the formula—

$$\left.\begin{matrix} C_2H_3(NO_2)_3 \\ (C_3H_5)_2 \end{matrix}\right\} O_2.$$

Methyl nitro-orcinate was prepared in a manner precisely similar to that above described, substituting methylic for ethylic iodide. Like the ethyl compound, it forms bright yellow crystals, which, however, have a somewhat higher melting-point, viz., 69·5° C.

Trinitro-resorcinic acid.—As it seemed important to ascertain if the homologues of orcin yielded compounds similar to trinitro-orcin, I submitted resorcin to the action of nitric and sulphuric acids in a manner precisely similar to that above described for the preparation of nitro-orcin.

The resorcin employed was prepared from galbanum by the excellent method given by its discoverers, Hlasiwitz and Barth,* substituting, however, sodium hydrate for potassium hydrate. After purification, it was treated with nitric and sulphuric acids, employing the proportions and methods detailed in the preparation of trinitro-orcin. As might have been expected, the yield of crude substance was larger, being 180 per cent of the resorcin employed. It was collected, washed with cold water, and purified by one or two re-crystallisations from boiling water (30 parts). It is of a paler colour than the corresponding orcin compound, and crystallises in leafy plates. It melts at a higher temperature (viz., 175·5° C.), and is more soluble than nitro-orcin; 156 parts of water dissolve 1 of nitro-resorcin at 14° C., but the presence of even a small proportion of one of the stronger acids renders it almost insoluble. Dried at 100° it gave the following results:—

I. 0·384 grm. of the substance gave 0·412 grm. of carbonic anhydride and 0·043 grm. of water.

* *Ann. Chem. Pharm.*, vol. cxxx., p. 354.

II. 0·418 grm. of the substance gave 0·451 grm. of carbonic anhydride and 0·051 grm. of water.

	Theory.		I.	II.	Mean.
C_6 =	72	= 29·39	29·27	29·43	29·35
H_3 =	3	= 1·23	1·30	1·36	1·33
N_3 =	42	= 17·14	—	—	—
O_8 =	128	= 52·24	—	—	
	245	100·00			

This corresponds to the formula $C_6H_3(NO_2)_3O_2$, that of trinitro-resorcin.

Barium nitro-resorcinate was prepared by dissolving nitro-resorcin in 100 parts of boiling water, and adding an excess of baric carbonate. The filtered solution, on cooling, deposited the barium compound in minute rhomboidal plates of a pale yellow colour. It is much more soluble than the corresponding nitro-orcin compound. The crystals contain three equivalents of water of crystallisation, which they do not lose at 100° C.; but, when gently heated on platinum foil, they assume an orange-red colour, from the loss of water of crystallisation, and at a higher temperature explode with extreme violence, perforating the foil. Both nitro-orcinic and resorcinic acids, and their salts, explode with far greater violence than picric acid and its compounds.

A barium determination of the nitro-resorcinate, dried at 100°, yielded the following results:—

I. 0·409 grm. of the substance gave 0·220 grm. of baric sulphate.

II. 0·444 grm. of the substance gave 0·239 grm. of baric sulphate.

	Theory.		I.	II.	Mean.
C_6 =	72·0	—	—	—	—
H_7 =	7·0	—	—	—	—
Ba'' =	137·2	= 31·59	31·63	31·66	31·64
N_3 =	42·0	—	—	—	—
O_{11} =	176·0	—	—	—	—
	434·2				

The formula is, therefore—

$$\left.\begin{matrix} C_6H(NO_2)_3 \\ Ba'' \end{matrix}\right\} O_2 + 3H_2O.$$

Lead nitro-resorcinate.—The addition of a solution of acetate of lead to an aqueous solution of nitro-orcinic acid gave a yellow gelatinous precipitate of lead nitro-resorcinate. It was found best, however, to prepare it by boiling a solution of 1 part of nitro-resorcinic acid in 200 of water, with a slight excess of lead carbonate, when the lead compound was deposited on cooling in the form of deep yellow needles. It is but slightly soluble in water, very soluble in acetic acid, and precipitated therefrom by the addition of alcohol. Sub-acetate of lead completely precipitates solutions of nitro-resorcinic acid.

Silver nitro-resorcinate. — This was prepared in a manner similar to the nitro-orcinate. A solution of 1 part of the pure acid in 75 of water was boiled for a short time with a slight excess of oxide of silver, and filtered. On cooling, the silver salt was deposited in long fine needles of a yellowish brown colour. They crystallise readily from boiling water, in which they are much more soluble than the corresponding nitro-orcinate. Dried at 100°, it gave the following results:—

I. 0·456 grm. of the substance gave 0·284 grm. of chloride of silver.

II. 0·416 grm. of the substance gave 0·260 grm. of chloride of silver.

	Theory.		I.	II.	Mean.
C_6 =	72	—	—	—	—
H =	1	—	—	—	—
Ag_2 =	216	= 47·07	46·87	47·04	46·96
N_3 =	42	—	—	—	—
O_8 =	128	—	—	—	—
	459				

The analyses correspond to the formula—

$$\left.\begin{matrix} C_6H(NO_2)_3 \\ Ag_2 \end{matrix}\right\} O_2.$$

Trinitro-beta-orcinic acid.—Beta-orcin, $C_8H_{10}O_2$, when treated with nitric and sulphuric acids as above described, gives a yellow substance, which appears to be the corresponding nitro-compound of beta-orcin; but, from the small amount of material at my disposal, I am at present unable to accurately determine its properties.

From some experiments I have made, the action of reducing agents, nitrous acid, &c., upon these nitro compounds promises very interesting results. I am at present investigating this subject. My thanks are due to Mr. Charles E. Groves for the efficient assistance he has rendered in conducting the above investigation.

ON THE CORONA SEEN IN TOTAL ECLIPSES OF THE SUN.

By Professor W A. NORTON.

Results of Observations on the Corona made at the Total Eclipse of August 7, 1869.

THE observations made on the corona on the occasion of the eclipse of 1869 have furnished several striking confirmations of the theory that it is an auroral phenomenon.

(1). *The Observed Form and Structure of the Corona.*—I will first adduce the results of my own observations. These were made at Des Moines, Iowa, with the naked eye and a good opera-glass, and were chiefly confined to the corona. When the totality commenced, and the beautiful corona stood revealed, like a new creation, against the dark background of the sky, almost the first striking feature that caught my attention was the great inequality in the extent of its outstreaming in different directions, and its consequent irregularity of outline. This outstreaming or luminous radiation was particularly conspicuous from the eastern limb, *nearly in the direction of the plane of the ecliptic or the sun's equator.* It could be distinctly traced in that direction to a distance from the sun equal to his own diameter. For an extent of some 15° on either side of the ecliptic, individual hair-like streamers, seemingly nearly parallel to the ecliptic, extended out a large fraction of this distance. From the opposite limb, and in the opposite direction, the coronal streamers were conspicuous, but of less extent than in the direction of the ecliptic toward the east. From the polar regions other pointed masses of light extended out to considerable distances, but not so far as those just noticed. They seemed to be composed, like the others, of rays or hair-like luminous radiations, more or less distinct. The separate luminous lines appeared to Professor Eastman, from the U.S. Naval Observatory (who observed the corona at the same station, through a small telescope) to converge more or less. This convergence I failed to detect; but I distinctly noticed that the outstreaming mass from near the north pole of the sun had approximately the form of a triangle with curved sides, convex outward, but the triangular outline appeared as if resulting from the intersections of individual radiations, rather than as being the definite boundary of a stationary luminous mass.

The corona had a white silvery lustre, and appeared at times suffused with a delicate rosy tinge, but this was probably a subjective effect. No flickering or variation of the lustre of the corona was observable during the totality; nor was there any noticeable change in its general form, or in the extent of its luminous radiations, though I carefully watched for such changes. Dr. B. A. Gould, who was stationed at Burlington, Iowa, and other observers, thought that both the lustre and extent of the radiating masses, or "star points," underwent material variations. A similar difference of opinion is found in the

reports of observations on previous eclipses. If variations in the brightness and extent of the coronal radiations do actually occur, it is favourable rather than opposed to the auroral theory of the corona; but it is probable that the apparent changes are due to inequalities in the interceptive action of the earth's atmosphere on the light of the corona, and especially on the faint light at its outer boundary.

Professor Harkness, of the U.S. Naval Observatory, in his able report of observations on the eclipse, states that the four angles of the trapezoidal outline of the corona were in the middle heliographic latitudes—relying upon the report of another observer; but on a direct examination of the question of the location of these angles, or "star points" of the corona, made since the publication of the report, he has satisfied himself that their actual position was such as I have above reported it from my own observations.*

In the delineations of the corona given by the observers of previous eclipses, two or more conspicuous outward extensions are generally shown, but the positions of these more projecting parts are seldom given with respect to the equator or poles of the sun. The figure of the eclipse accompanying the report of P. Professor Capellotti, of observations on the eclipse of April 15, 1865, made at Chili, is an exception. It shows three principal points of outstreaming of the corona; two lying very nearly in the plane of the sun's equator, and nearly diametrically opposite to each other, and a third near one of the poles of the sun. In the eclipses of 1858, 1860, and 1868, four such points were seen, distributed at about a quadrant's distance from each other. In the eclipse of 1842, but two were noticed, which were diametrically opposite to each other. In that of 1851, there appears to have been no marked deviation from a general uniformity of radiation.

Relying, then, upon the only definite knowledge we have of the location of the more conspicuous portions of the corona, viz., that obtained in the eclipses of 1865 and 1869, we may say that the corona is brighter and more extended about in the direction of the plane of the sun's equator than in any other direction. This striking fact lends a powerful support to the auroral theory of the corona; for, as we have already seen, the streamers proceeding from the lower latitudes on the sun, on opposite sides of the equator, should converge and intersect in the plane of the equator, and for a certain distance on either side of this plane, and, in consequence, the corona should appear to extend farther in the plane of the equator than in other directions. The convergence of individual rays or lines of emanation, we have already seen, was actually noticed by Professor Eastman. It, of course, may happen that inequalities in the amount of outstreaming on opposite sides of the equator may throw the more prominent and conspicuous parts of the corona to the one side or the other of the plane of the equator. It will be observed that, from our present point of view, the extension of the corona in the plane of the sun's equator is a phenomenon kindred to the much greater luminous extension seen in the zodiacal light; the only difference between them being, that in the former the auroral emanations proceed from lower heliographic latitudes, and intersect nearer the sun.

But it may be asked how are we to explain, on the present theory, the "star points" of the corona over the polar regions of the sun. For these, two reasons may be assigned. (1). If we admit a distribution of magnetism on the sun similar to that which prevails on the earth, the auroral streamers should diverge from each other less rapidly in the high than in the low latitudes. (2). Upon opposite sides of a line of no declination traversing the sun's surface, analogous to that which traverses Russia, the natural directions of the streamers prolonged upward would be such as to occasion the convergence and intersections of those proceeding from the opposite sides of this line.

We may say, then, that the more extended portions of the corona, in the eclipse of 1869, were over those regions of the sun's surface, and those only, whereupon the present theory the intersections of streamers might be expected to occur.

In some eclipses distinct luminous curves having the appearance of luminous jets issuing tangentially to the sun's limb, or obliquely inclined to it, and pursuing a course either convex or concave to the limb, have been seen. According to M. Liais, these peculiarities were conspicuously observable in the eclipse of September 7, 1858. While it is possible that such curves may be the result of the intersections of a mass of straight streamers, it is not improbable that they may be actual luminous jets; for if from any cause any portion of the auroral matter should be projected from the sun in a direction oblique to the surface, it would proceed in a convex hyperbolic curve if repelled by the sun, and in a concave curve if attracted. Now I have shown in a previous number of the *American Journal of Science and Arts* (July, 1861), that the portion of cometary matter posited on the convex side of the tail of Donati's comet was actually repelled by the sun, while that on the concave side had become detached from the head of the comet, because of a diminished gravitation toward the sun. Upon our fundamental conception that the coronal matter is essentially in the same physical condition as such cometic matter, and subject to the action of the same solar forces, it may well happen that some individual jets will proceed in convex and others in concave curves, according as the escaping matter is repelled or attracted by the sun.

(2). *Observations on the Physical Constitution of the Corona with the Spectroscope and Polariscope.*—The results of the observations made at the late eclipse, with the spectroscope and polariscope, are strongly confirmatory of the truth of the theory of the corona under discussion.

Professor Pickering, in the report of his observations with a polariscope, says, "The form of polariscope used was that adopted by Arago in his experiments on sky polarisation. It consists of a tube about twenty inches long and two inches in diameter, one end of which is closed by a double image prism of Iceland spar, and the other by a plate of quartz. Looking through the former, we see two images of the latter, which, when the light is polarised, assumes complementary tints. If, now, the corona was polarised in planes passing through the centre of the sun (as is generally admitted), when viewed through the polariscope, in one image the upper and lower parts should have appeared blue, and those on the right and left yellow; while in the second image these colours would be reversed, the yellow being above and below, and the blue on the sides. In reality, the two images were precisely alike, and both pure white, but one was on a blue and the other on a yellow back-ground. From this we infer that the corona was unpolarised, or at least, that the polarisation was too slight to be perceptible."

We may infer from this that the corona is either self-luminous or shines by diffuse reflection; since specular reflection produces polarisation.*

The testimony of the spectroscope is still more decisive. Professors Pickering, Harkness, and Young, agree that the spectrum from the light of the corona was a continuous one, or free from dark lines, but containing one or more bright lines. The absence of dark lines indicates that the corona did not shine by the light of the photosphere, reflected either diffusely or specularly from its substance; since such light, after reflection, should, like the direct solar light, have given a spectrum with the Fraunhofer lines. The presence of bright lines, on the other hand, is a direct indication that the corona was self-luminous; and therefore that its light was the result either of combustion

* Professor Winlock, in his report of observations on the eclipse, says, "the photograph of the corona taken at Shelbyville shows a flattening at the extremities of the sun's axis, and an elevation about the equatorial region." The photographic impressions obtained of the eclipse at the different stations show, however, but a small portion of the outward extent of the corona visible to the naked eye.

* The question whether the light from the corona is in any degree polarised or not cannot be regarded as definitively settled. It is to be hoped that the observations made on the eclipse of December will remove all doubt on this point.

or of electric discharges. As it is hardly supposable that an actual combustion could prevail at the distance of tens and hundreds of thousands of miles from the sun's photosphere, in regions where, if any solar atmosphere exist, the results of recent observations with the spectroscope by Lockyer and Frankland lead us to believe that it can only be the faintest possible trace of it, we must infer that the light of the corona is of electric origin.

In the hands of Prof. Young and Prof. Winlock, the spectroscope has obtained direct evidence of a physical correspondence between the solar corona and terrestrial auroras. Prof. Young observed in the spectrum of the corona a bright line, the position of which he gives as 1474 on Kirchhoff's scale, and which proves to be in coincidence with a small line marked as *iron* on Kirchhoff's and Angström's maps. He remarks that "it turns out also to coincide very closely, if it is not (which is much more probable) absolutely identical, with a line recently discovered by Prof. Winlock, of Cambridge, in the spectrum of the aurora borealis. He also saw two other fainter lines in the spectrum of the corona which coincided quite closely with other lines reported by Prof. Winlock as visible in the spectrum of the aurora. In view of these results of spectroscopic observation, he remarks as follows:—"At present it seems pretty likely that the spectra of the corona and the aurora borealis are identical, with only such differences in the intensity of their lines as we might naturally expect, and that very probably the identity extends to the essential nature of the phenomena themselves."

The detection of the same iron line in the aurora and corona, taken in connection with the well-established fact that the vapour of iron is present in the photosphere and chromosphere of the sun, and that the magnetic features of the aurora lead to the natural conclusion that some form of ferruginous matter constitutes the substance of auroras, for which no terrestrial origin can reasonably be assigned, conducts to the inference that the terrestrial auroral matter is derived fron the sun, and adds to the weight of accumulative evidence in support of the theory I have advocated that the corona is made up of material emanations from the sun.

Note.—Some persons have conjectured that the corona might be produced by the passage of the sun's rays through the earth's atmosphere, but it may readily be shown that this is impossible. When one reflects that the half width of the moon's shadow, in the larger eclipses, is as great as the estimated height of the atmosphere, it will be seen that, to an observer on the central line of the eclipse, the line of sight will not fall upon the illuminated portion of the atmosphere exterior to the shadow, unless inclined under a large angle to the line of direction of the centres of the sun and moon. The corona, therefore, if of terrestrial atmospheric origin ought to present, toward the middle of the eclipse, the appearance of a halo entirely detached from the dark body of the moon and many degrees distant from it. It ought also to increase in brightness from its inner border for a considerable distance outward.

Others have imagined that the corona might be attributable to the passage of the sun's light through a lunar atmosphere; but since some of the streamers, or rays of the corona, have been seen to extend to a distance greater than the sun's diameter, this would require the lunar atmosphere to be of vast extent; whereas no decisive evidence has yet been obtained of the existence of any lunar atmosphere capable of producing a sensible refraction, or reflecting a perceptible amount of the sun's light to an observer on the earth.

Perhaps the more prevalent idea, at the present day, is that the corona, with its rays and tufts of light, is a phenomenon of diffraction produced by the passage of the sun's rays along the denticulated edge of the moon. This theory has an air of plausibility, but it is entirely inadequate to account for the great extent of the coronal rays. The fringes produced by the diffraction of light in its passage near the edge of a body appear to the eye of the observer to extend but a small angular distance from the edge. This would be more strikingly true in the case of a distant body, like the moon.

The only remaining supposition is that the corona is either an envelope of some kind permanently connected with the sun, or is made of material emanations proceeding immediately from the sun, To the large body of indirect evidence that the corona is wholly a solar phenomenon that has been obtained, we may now add that of direct observation, since it apears that "an examination of the photographs of totality" obtained at the eclipse of 1869, shows that as the moon advanced the corona was progressively covered.—*American Journal of Science.*

ON BOILED OILS AND VARNISHES.*

By CHARLES W. VINCENT.

My chief desire on the present occasion is to render as complete an account of the process of oil-boiling by steam as I can, without committing any breach of confidence towards those for whom I have been professionally engaged. A certain amount of reticence is necessitated by this latter consideration, but, at the outset, I may state that it applies only to the particular substances used to give the necessary drying or solidifying qualities to boiled linseed oil, and hence technically termed driers. The process itself may be used with any of the substances commonly employed in boiling oil by fire for the same purpose. The apparatus employed and *modus operandi* being well known throughout the trade, I am rather benefitting than injuring the cause of the manufacturers by publishing an account of the present state of a mode of manufacture which must, eventually, become the principal source of boiled oil.

The first consideration which presents itself to a practical man, on approaching a subject which he wishes to thoroughly master and understand, is, What do I want to do? the second being, How can I do it? Viewed in this way, it became evident to those engaged in this research that oil-boiling might be reduced to a much more simple and perfect operation than that ordinarily pursued.

The principal worker who has published the result of his researches is M. E. Chevreul, who, in 1856, in an admirable paper in the *Annales de Chimie*, definitely pointed out that the act of drying of linseed oil is one of absorption of oxygen, and consequently might more properly be termed solidification than drying (we will, however, retain the accustomed term); next, that many driers act catalytically; and lastly, that too long boiling retarded the drying of the oil. For instance, oil boiled three hours without driers, and afterwards three hours with litharge, dried more slowly than the same oil boiled for three hours with litharge, without any previous heating.

To whom the next discovery is due, viz., that the high temperature attained in oil-boiling by fire is unnecessary, I do not know. I discovered the fact for myself in 1859-60, when engaged in a research for Messrs. Leighton Brothers, of the *Illustrated London News*, on varnishes for printing inks; and at that time I introduced a mode of steam oil-boiling and varnish making which is still carried out by some of the largest makers of printing inks and those kinds of varnishes. The fact is now very generally accepted, though some manufacturers still hold aloof.

There are reasons which interfere with, and which stop the progress of, many arts and inventions, though against reason, and the oil trade is not free from these drawbacks.

* Read before the Society of Arts.

A principal one is, that the manufacturers are, in a great measure, tied down by the wishes and desires of their customers, so that, in many cases, they have to produce rather what the customers think they require than what they really need. It is this multiplicity of objects which causes the great variety of modes of manufacture of linseed oil for painting purposes. It is a common case to find that what one person, a large consumer, calls a thoroughly good boiled-oil, his neighbour, an equally large consumer, will pronounce entirely unfit for use. Now, it is my earnest wish that, as far as possible, you should hold yourselves free from preconceived notions of what constitutes a good boiled oil or varnish conventionally, and, considering merely the uses to which they are required to be put, reflect how far these requirements are met. My own personal experience has placed certain points before me as of paramount importance, and to these I shall direct your attention. Those characteristics which may be essential to the good quality of one mode of boiling oil, but do not forward results produced when these articles are applied to their various uses, I shall neglect.

Before entering upon my subject, I must remark that no paper read before the Society of Arts on oil-boiling and varnish-making can be complete and appropriate without at least some allusion to that communication on the subject presented by Mr. J. Wilson Neil, for which the Isis Gold Medal was awarded to him in the session 1832-3 of the Society, which was published in the *Transactions*, vol. xlix., part 2. It is not within the scope of the present paper to treat of the modes of manipulation, recipes, and precautions which he there so fully, carefully, and, indeed, conscientiously describes; but the fact remains that that paper was the foundation, and remains so to this day, of all the published accounts of modes of manufacturing varnishes. The alterations and modifications which have been from time time worked out are more in the shape of mechanical appliances for facilitating rapidity, easing labour, and providing greater safeguards against fire, than improvements of process. The principle of manufacture remains the same, and the varnishes now made cannot be said to be better in quality, though they may be made at a less cost, and in shorter time, than those produced under the processes detailed by Mr. Neil. Many firms still retain modes of operation almost (in some cases quite) identical with those he describes. The paper is still of such value that the Society would, even now, confer a benefit on the trade were they to republish it.

For a process of oil-boiling by fire, Mr. Neil's is nearly perfect; the new process depends on principles entirely different from those he dealt with, and therefore it is necessary to begin the subject, as it were, *de novo*. Linseed-oil is boiled for two reasons, as you all know—firstly, in order to facilitate its drying when spread on thin surfaces, either alone, or mixed with paint; secondly, that it may serve as a vehicle for the mechanical suspension of the finely-divided particles constituting a paint; that it may give a certain cohesive power to the mixed paint so constituted, enabling it to adhere to the surfaces on which it is spread, neither running into drops, nor leaving the colouring matters behind, but carrying the whole of the colour evenly diffused through it over the whole surface upon which the paint is laid, and when dry, forming together a surface which is, as far as may be, impermeable to gases and liquids. Technically, these are the two qualities which consumers rightly agree in desiring in boiled oil—that it should be a quick-drying oil, and have a good body. But it is frequently the case—and, in my opinion, they err in so doing—that consumers require in boiled oil, besides these two essential qualities, others in reality quite adventitious, to which, either from experience of some particular sample, from the custom of their trade, or from the mode in which they have been led to believe good boiled oil alone can be made, they attach a fictitious importance. It is of every-day occurrence that, unless the oil complies with these artificial demands, it is unhesitatingly condemned, independently of the two essentials upon which all are agreed. For instance, some persons attach the idea of a particular smell to be a good sample of boiled oil. If all were agreed as to what that smell should be, the desire would be easier to satisfy; but taking it for granted that the smell is a guarantee that the oil has been heated for a certain time at a certain temperature, it is obvious that two samples of oil may have that odour, whilst from the different ages of the oils previous to boiling, the quality of the seed from which they were expressed, or the driers employed, their qualities of drying and body may be widely different. Other persons, again, confine in their opinion good quality to a particular shade of colour and degree of brightness; a third party requires a particular shade of colour, body, and smell combined; others quarrel with their manufacturer if he vary his standard shade of colour, smell, &c. The truth is, that these are circumstances independent of the proper treatment of the oil. The varying qualities of oil require a varied treatment to bring them to one standard of body and drying power. If these be the standards to be maintained, the others, which, after all, are only indications that the oil had been manufactured, are of no importance. This applies to the ordinary process of boiling oil by fire, and I do not insist but that, if a uniform quality of oil be employed, and the same process always pursued, the colour and smell would be indications of the success of that particular process upon that particular oil; but, at the present day, the field from which you obtain linseed oil is much wider than it was of old; you cannot always buy the oil you prefer, and, of necessity, the mode of manufacture you adopt has to some extent to vary. If once the idea be thrown over that a high temperature is essential to the preparation of a good boiled oil, you at once obtain modifications of these peculiarities of colour and odour, though the other qualities, body, and drying power may be the same, or even increased.

With these preconceived notions it is that the new process of oil-boiling had, and still has to contend, people imagining that unless the boiled-oil attains a conventional standard of appearance, it of necessity must be bad. One of the advantages of the new process is hereby much diminished, since oil-boilers have to adopt expedients to give a colour to the oil which otherwise it would not have to nearly the same extent. And to the practical man an aim has to be added which frets and annoys common-sense and honesty of purpose, viz., that in addition to that of good and improved methods of manufacture, there must be sufficient similarity in appearance to accustomed samples to avoid awakening those prejudices against anything new, merely because it is new, still largely existing amongst the mechanical part of our population. It is the more to be deplored, since the colour of white zinc, delicate blues, &c., are much deteriorated by the dark oil used for mixing with them.

I discovered, in the beginning of 1860, that with suitable driers, I could obtain an oil which, although it had never been raised above the temperature of 228° F., nevertheless had the body and the drying power of a good boiled-oil. This led me to investigate the subject further, although the trade being limited, and the persons engaged in it having acquired a certain reputation for a good merchantable article, made by processes which they have not unfrequently worked out for themselves, they have not the same incentive to seek for improvements, or even to adopt them when brought before them, as exists in many other manufacturing processes. I do not claim any merit as an originator of steam oil-boiling, for I have reason to believe that it has been practised in Germany for many years past; but as far as I am personally concerned, I may state that all I know concerning the process I have discovered for myself, not even being aware that it was practised by others for some years subsequently to my own discovery and use of it.

Chevreul's experiments, to which I have previously alluded, form a good substratum for a disquisition on this

subject. The principle being clearly laid down that it is the absorption of oxygen by linseed oil which causes its solidification, it follows that all the substances which can be used as driers must be such as are capable of parting with oxygen or dissolving in it, and being themselves oxidisable in combination, they, in that way, increase its absorptive power. There is a class of driers which act whilst mechanically suspended in the oil, as white copperas, &c., for example, increasing its absorptive power by their presence, but leaving no increase of drying power when withdrawn.

The substances which act catalytically belong, in my opinion, to the first class. They part with some of their oxygen to this oil, become to a certain extent deoxidised, and again coming into contact with air they reassume their primal condition, and are ready to do the same work over again.

It is this necessity of re-oxidation which caused the introduction of blowing machines for oil-boiling, when their own proper advantages in rapidly giving body to linseed oil at comparatively low temperatures were at once, and to me unexpectedly, made manifest. These catalytic driers are now generally adopted, the effect of their use being simply this (and you will see that the result is a great advantage upon previous methods), that by treating the oil with these driers under the influence of heat and air, its own absorptive powers for oxygen are greatly increased, and remain so when the primary incentives to absorption are removed. This likewise has to be borne in mind, that what we want to do is not to render the oil solid or partially so now, but to enable it to become so when exposed to the air.

Many years ago, the late Professor Faraday was consulted as to the possibility of hastening the drying of printing inks so that work might be milled (that is compressed between zinc plates passed through closely-set rollers) with less delay after printing. A fortnight was the usual time taken, and, if ordinary driers were used, the whole of the ink would skin on the rollers, with many other inconveniences. Faraday's far-sighted sagacity at once decided that one solution of the difficulty would be by the mechanical introduction of a substance to act as a drier which should be inert till it was exposed to the air. As Professor Faraday's advice and assistance, in a research on the same subject for the same firm was of material service to me, and, indeed, enabled me to supersede the drier in question, I break no confidence by stating that it was binoxide of manganese which almost without experiment he at once pitched upon. By what process of thought he chose this substance I do not know, but the fact remains that he did so introduce it, and for, I think, between thirty and forty years it was used by the same firm. At the Queen's Bible-office they mill their work three days after printing, if it prove necessary. In order to get the binoxide in a state of division sufficiently fine to mix with printing-ink, he devised a series of washing receptacles, like successive stairs, the finer particles passing on to the lower vessels, being longer suspended than the coarser—a simple yet ingenious arrangement, which enabled the ink to be worked without any risk to the plates or formes from grit.

By properly pursuing this investigation, many other substances have been discovered which exercise this property in a high degree. Without becoming in any way altered, they induce an alteration in the linseed oil subjected to their operation. We may imagine the action as similar to that by which spongy platinum explodes a mixture of oxygen and hydrogen, or a platinum wire is kept red-hot by the vapour of ether.

Litharge and the other salts of lead perform the same office whilst in solution in the oil, and to their presence for the most part is the dark colour of boiled oil due. Where the steam process is used, the oil never reaches a sufficiently high temperature to produce carbonisation. By judiciously mixing one of the substances acting catalytically and a lead salt, a drier is obtained, by altering the relative proportions of which the boiler is enabled to produce any required shade of colour. The larger the quantity of litharge, the darker the colour of the boiled oil. A lesser proportion of litharge, accompanied by a greater quantity of the catalytic drier, gives as the product an oil which will dry in the same time, and has less colour. These are the principles of oil-boiling by steam.

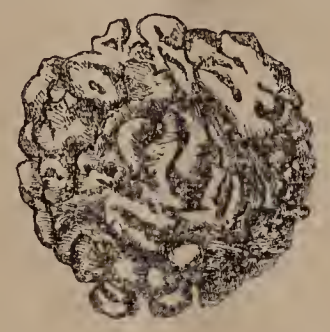
(To be continued.)

NOTES OF

ANALYSES OF AN EARTHBALL AND INTESTINAL CALCULUS FROM THE HORSE.*

By JAMES F. STARK, F.C.S.

The following serves merely to bring under your notice two analyses I had lately occasion to perform. The earthball was extracted, in the end of last year, from the large intestine of a horse belonging to Messrs. Charles Tennant and Co., of St. Rollox. In diameter it measures 5 inches, and weighs a little over 2 lbs. The section



shows that the materials of which it is composed have been deposited under various changes of circumstances—at one time growing solely by accretion of mineral matter, and at another time by mineral matter intermingled with substances of organic origin. In the analysis, a full section was taken, so as to ensure a fair average sample being obtained.

Magnesia	13·585	
Ammonia	8·828	83·197
Phosphoric acid	24·109	
Water (combined)	36·675	
Lime		0·239
Alumina		4·171
Ferric oxide		1·033
Soda		0·358
Phosphoric acid		0·189
Carbonic acid		0·015
Sulphuric acid		0·465
Silicic acid		5·202
Sodium chloride		0·490
Organic matter		4·686
		100·045

The remarkable feature in this analysis—indeed that on which its whole interest centres—is the very large proportion of ammonio-magnesic phosphate present.

Shortly after the performance of this analysis, I had the fact of the formation, to a large extent, of this ammonio-magnesic phosphate within the intestines of horses confirmed, by receiving several calculi that had been passed by a horse, and which the following analysis shows to have been formed almost entirely of this salt. The largest of these calculi measures nearly an inch in diameter. Analysis gave as follows :—

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* Read before the Chemical Section of the Philosophical Society of Glasgow, March 27th, 1871.

Ammonio-magnesic phosphate—

$Mg_2P_2O_7$	44·504	
$(NH_4)_2O$	10·424	
$12H_2O$	43·301	
		98·229
Organic matter—		
Soluble in HCl	0·801	
Insoluble in HCl	0·910	
		1·711
Silicic acid		0·044
		99·984

As to the condition of the blood necessary for the deposition of this ammonio-magnesic phosphate, I have not spent time in speculating—such comes more within the range of the physiologist. It is plainly enough seen, however, that, a small nucleus presenting itself (detained probably in some cavity), we have this salt thereon deposited. Now, if this goes no farther, no serious damage is done; such calculi seem to pass readily through the intestines, being smooth on the surface, and of a shape calculated to facilitate such a passage. It is only when the calculus assumes a nature similar to this large earthball that danger ensues to the animal—only when the ammonio-magnesic phosphate is intermingled with adventitious substances. How this intermingling of mineral and organic matter takes place I think I may be able to show. If we take a quantity of chopped hay, and sift the finer particles away from it, re-sifting these several times through finer sieves, and lastly through muslin, we arrive at a very finely-divided, dark-coloured mass. Again, on boiling a small quantity of this earthball reduced to powder, I find a certain amount of seemingly albuminous matter separate out, floating on the surface of the liquid. If we then suppose the surface of the calculus in part coated with this albuminous matter, any particles of this fine dust coming in contact with it would at once firmly adhere (indeed, examined under the microscope, this dust seems exactly fitted for such a work); over this, more ammonio-magnesic phosphate would be deposited, and so on, the mass gradually increasing in bulk. A substance similar to this dust is likewise found on grain, especially on oats, and is, I believe, sometimes separated, to the great improvement of the meal. In some districts, where this substance is not separated, the miller's horses are very subject to calculi. But here the horses do not suffer alone. Wollaston and Dr. Monro Tertius fully investigated, in their "Morbid Anatomy of the Human Gullet," a peculiar calculus endemic to Scotland, the frequent occurence of which amongst the population was assigned to the abundant use of oatmeal. Dr. Maclagan, in 1841, spoke of such as growing less common.

And now I have only one remark to make as to the practical application of this—one of no inconsiderable importance to the employers of horse-power in our large towns. What I have said points clearly to the necessity there is for thoroughly sifting all hay and grain used in the feeding of horses. Such, doubtless, in large establishments would entail a considerable expense, an expense, however, that in the long run would prove itself to be an economical one. I believe such a course has been pursued by Mr. John Clark, of Glasgow, in some establishments with which he has been connected, with very satisfactory results. When we see the large quantity of matter exactly resembling that met with in this earthball that can be extracted from a single stone of hay, and consider the number of stones consumed by a horse in the course of a year, we cannot fail to see that, by giving the animals the hay unsifted, no inconsiderable risk is run, but that we also condemn them to consume far more than the peck of matter incapable of assimilation, of which we are told we must all, at some time or other of our lives, become the unwilling possessors of.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

April 20th, 1871.

Professor Odling, F.R.S., Vice-President, in the Chair.

The following gentlemen were elected Fellows:—C. C. Grundy, S. B. Lee, G. Sutcliffe, W. Ward.

Mr. C. Haughton Gill read a paper "*On some Saline Compounds of Cane Sugar.*"

Péligot has described a compound of cane sugar and salt to which he ascribed a formula of $C_{24}H_{42}O_{21}NaCl$ (C=6, H=0·5, O=8) which requires 14·92 per cent of sodic chloride: this indicates the replacement of 9 parts water by 58·5 parts of sodic chloride. Blondeau de Carrolles subsequently examined a similar compound, to which he ascribed the formula $C_{24}H_{20}O_{20},NaCl,3HO$, which includes water of crystallisation, of which Péligot makes no mention. Subsequently Mitscherlich denied the existence of the body, and Hochsteller mentioned that other chemists had failed to obtain it. Mr. Gill, being at first unsuccessful in preparing Péligot's compound by the method described, boiled a solution of sugar with an excess of salt for some time, filtered, and set aside the apparently uncrystallisable syrup. At the end of some months a few small, not very transparent, but individually distinct crystals had formed. They were drained, rinsed, pressed, and analysed. The numbers lead to the formula $2(C_{12}H_{22}O_{11})3NaCl.4H_2O$. This compound of an unexpected composition having been obtained, a number of solutions of sugar with various proportions of different salts were made up and set aside to crystallise spontaneously in a dry air space, or in the gentle and regular warmth of a sugar-house working-floor. Where crystals were not obtained by these means a more rapid evaporation was tried, or an alcoholic solution was slowly evaporated, and, in many cases, these and other methods were tried with liquids neutral, alkaline, and faintly acid. The salts employed were the chlorides of potassium, sodium, lithium, and ammonium, the bromides of potassium and sodium, and the iodides of potassium, sodium, lithium, and ammonium. In each case four solutions were prepared, having one, two, three, and four molecules of the salt to the double molecule of sugar, $2(C_{12}H_{22}O_{11})$. None of the potassium salts gave compounds of a definite composition. The mixture containing the chloride in the smallest proportion gave crystals of pure sugar,—those containing the two largest proportions gave a crop of the pure potassic chloride. From the solution containing 2KCl to $2C_{12}H_{22}O_{11}$, anhydrous crystals containing from 1·3 per cent to 19·6 per cent of potassic chloride slowly separated, leaving behind a mother liquid of the consistency of treacle.

The solutions containing potassic bromide behaved in a very similar manner, giving crystals often very clear and sharp, and sometimes 5 or 6 m.m. in extreme dimensions, but always anhydrous and of irregular composition.

Different specimens contained such proportions of potassic bromide as 24·8, 19·15, 16·17, 12·0, 7·56, 28·15, and 22·3 per cent. In appearance the crystals could not be distinguished from those of pure sugar. The solutions containing potassic iodide evaporated to very thick sticky masses, sometimes containing a number of minute crystals which could not be separated from the mother-liquor.

The sodium salts gave more definite results. In the case of the liquid, the solution containing least salt first gave crystals of pure sugar, and then, on further concentration, deposited crystals which are, doubtless, the same as those of the compound examined by Péligot, and are

identical with those obtained from the liquid containing the next higher proportion of salt, viz., NaCl to $C_{12}H_{22}O_{11}$. This compound crystallises in prisms terminated by pyramids; is very soluble in water, less so in spirit. From its solution in hot spirit of 85 per cent anhydrous crystals containing from 5 to 10 per cent of sodic chloride are deposited on cooling or slow evaporation. When to its solution in spirit of not more than 75 per cent ether is added, an oily layer is formed at the bottom of the vessel, and in this crystals form which have the composition $C_{12}H_{22}O_{11}.NaCl.2H_2O$.

The solutions containing sodic bromide can hardly be made to crystallise at all. A small quantity of minute confused crystals were deposited after some months from the solution containing 3NaBr to $C_{12}H_{22}O_{11}$; these, when pressed and dried over oil of vitriol, gave numbers which would point to a formula $NaBrC_{12}H_{22}O_{11}1\frac{1}{2}H_2O$, but it is more probable that when pure it is similar in composition to the analogous compound of sodic chloride.

The solutions containing sodic iodide give crystals of definite and constant composition with remarkable ease. These crystals always have the same composition whatever the proportions of the constituents in the mixture, unless one be in such large excess that it can in part crystallise out before the liquid becomes saturated with the compound.

The solutions containing a moderate excess of sodic iodide yield the best crystals and quickest growth. It can be re-crystallised as often as desired from water or dilute spirit without suffering decomposition, forming large transparent crystals even from small quantities of solution. Their constitution is expressed by the formula $2(C_{12}H_{22}O_{11}).3NaI.3H_2O$. None of the mixtures containing lithium gave any crystals other than those of sugar. Those containing a large proportion of the salt simply dried up to a gummy mass.

The mixtures containing ammonic salts gave no definite compounds. The chloride when in small quantity allowed sugar to crystallise out freely; when in large quantity it hindered crystallisation, but in time allowed the liquids to deposit well-formed, isolated, anhydrous crystals, generally opaque, but having the appearance of a definite body. These crystals, in fact, contained most variable proportions of ammonic chloride.

No results were obtained from the solutions containing ammonic iodide.

The crystals of sugar containing ammonic chloride and the equally distinct though generally smaller ones containing potassic bromide, and those deposited from a hot alcohol solution of the lower salt compound, must be built up by an anhydrous compound of the salt and sugar, isomorphous with sugar itself, crystallising out, together with an excess of the latter.

That the crystals are not simply sugar with adhering ammonic chloride is shown by their individual perfection and by the fact that they are deliquescent, whereas neither constituent is so.

The solutions of all the bodies described in this paper. especially that of the lower salt compound, exhibit persistent supersaturation in a remarkable degree. A saturated warm solution when cooled and shut up in an air-tight vessel with several crystals of the solid body, continues to deposit more of the compound for several months.

It is to be remarked that the sodium salts which give definite compounds are known to crystallise with two molecules of water when deposited at low temperatures, and that the iodide does so at all temperatures up to 20 per cent, whereas the corresponding potassium salts are anhydrous at whatever attainable temperature they may be crystallised. The composition of the sodic iodide compound makes it seem probable that the true molecular weight of cane sugar should be represented by $C_{24}H_{44}O_{22}$.

The measurements of the various crystals mentioned in Mr. Gill's paper were kindly executed by Professor W. H. Miller.

NOTICES OF BOOKS.

Second Annual Report of the State Board of Health of Massachusetts. Boston: Wright and Potter, State Printers. January, 1871.

It is not unfrequently said in this country "they manage these things better abroad." Judging from the work before us this trite saying applies to the great Transatlantic Republic, originally founded by Republican Dutchmen, and years after endowed with a Constitution essentially the work of sound minded Frenchmen, of that "France intelligente, éclairée et libérale," to which fully applies the "Fluctuat nec Mergitur" of the Parisian coat of arms. The report of which the title is above given, is a very valuable contribution to what the Germans aptly term *Sanitäts Polizei*, of which, while the proper equivalent term of it in English is wanting, there is also rather too much wanting of the real tangible facts as put in force and expressed by that terse term. The following is the enumeration of the contents of this work:—General Report of the Board; Expenses of the Board in 1870 (these, we must not omit to state, are, as compared with the work effectually performed very moderate, but amount, moreover, to less than half the sum which the State Legislature of Massachusetts appropriated for the use of the Board in 1870); Report of the Secretary; Poisoning by Lead-Pipe used for the Conveyance of Drinking Water; Trichina disease in Massachusetts; Health of Towns; Charbon (Malignant Vesicle) in Massachusetts; The Causes of Typhoid Fever; Homes for the Poor; Convalescent Homes; The Sewage Question; Correspondence concerning the Effects of Intoxicating Drinks; Analysis of the Mortality of the City of Boston, in 1870; The Ventilation of School-houses; Mystic Pond, and its Sources of Supply; Air and its Impurities; Health of Minors Employed in Factories; Use of Milk from Cows affected with Foot and Mouth Disease. In about 440 pages there is condensed and concentrated a very large amount of valuable information, not bearing upon the State of Massachusetts alone, but also upon various other parts of the world, including Japan, Egypt, and a great number of other countries and localities. Since some portions of this work are specially interesting to chemists we propose to reproduce them at some future time. The volume is very creditably and carefully prepared, and also well printed, being an excellent specimen of an American "blue book," in the orthodox blue cover so well known in this country.

Ueber die Steinsalzablagerung bei Stassfurt und die Dortige Kali-Industrie, sowie über die Bedeutungderselben für Gewerbe und Landwirthschaft. Von C. Reinwarth. Dresden: G. Schönfeld's Verlagsbuchhandlung (C. A. Werner). 1871.

On the Salt Deposit near Stassfurt, on the Potash Industry at that place, and on the Importance thereof for Manufacturing and Agricultural Interests. By C. Reinwarth.

If any proof were required of the practical truth of the motto of one of the City Companies, "*Sal sapit omnia*," the small work before us (43 pages) bears evidence thereof in a very striking manner. The pamphlet contains, first, the detailed description of the discovery of the now renowned saline deposit met with near Stassfurt, now a place of importance as a large manufacturing town, formerly only a small market town, situated in a rural district at a distance of 20 kilometres S.W. from the well-known city of Magdeburg. The description refers to the geognostic features of the country round about Stassfurt, details the results of a series of borings made for the purpose of ascertaining the nature of the minerals found under the surface of the ground, and includes the exhaustive description of

the various saline minerals found among the layers of rock-salt. This is found not only very abundantly, but also of great purity; while interspersed with it are found substances not hitherto found anywhere else (excepting quite recently near Kaluscz, in Galicia, near the Lemberg-Czernowitz Railway, where a saline deposit has been discovered which is both extensive and very rich in potash salts). Among the salts spoken of by the author, we briefly mention the following:—Carnallite, so named in honour of Herr Carnall, a Prussian mining engineer, a double chloride of potassium and magnesium, mixed, however, with other salts; stassfurtite, a peculiar mineral which contains boracic acid; sylvine, a more or less pure chloride of potassium; kainite, a compound containing sulphuric acid, potash, magnesia, chlorine, and water.

In addition to common salt refining, the great industry of Stassfurt is the manufacture of chloride of potassium. The author of this excellent pamphlet gives a minute and lengthy description of the various processes which are employed for separating the various salts from each other, and the utilisation of the by-products thus obtained. We learn from the pamphlet before us that, as far, at least, as geology can give a clue to this matter, the saline deposit found, and successfully wrought, near Stassfurt may extend for several miles farther than the locality alluded to. In order to give some idea of the enormous quantities of saline materials produced, suffice it to say that the weights, obtained in 1869, of the potash salts amounted to 109,075 tons, and of rock-salt to 56,332 tons.

CORRESPONDENCE.

HIGH AND LOW CHEMISTRY.

To the Editor of the Chemical News.

Sir,—As a member of the commitee of the Lincolnshire Farmer's Association, will you allow me to make an appeal to your professional readers to throw a little scientific light on a matter which to us "bumpkins" is very perplexing.

We contract yearly for several thousand tons of phosphatic manure, with the condition that it shall contain 26 per cent of soluble phosphate, or phosphate made soluble! and subject to the analysis of some qualified chemist. Now, we find that, as a rule, A—* is invariably selected by the maker, and B—* by the consumer; indeed, this question involves a difference of many shillings per ton, whether what they term the high or low analyst is appointed. I was not aware, until I came in contact with these chemical perplexities, that science, like theology, had the comical characteristics of high and low. To satisfy you that such differences do exist, I beg you will read the report and correspondence which I send with this letter. The report or analysis was made upon samples of manure sent to A— and B—, taken from the same bulk, and with the greatest possible care. The difference you will observe is 3·6 per cent—equal to eleven shillings per ton, or 14 per cent in the original cost—and this is not a solitary case, for it is notorious in the trade that such differences between these chemists do exist, and that in contracts involving amounts of many thousands of pounds, the question of which of these two chemists shall be accepted as analyst is one of great pecuniary consideration, and is an important condition in the terms of contract.

When we ask for some explanation, you will observe the reply is either nothing, or that there is some difference in the process of analysis! but, I am informed, that to ascertain the amount of soluble phosphate *only*, the process is neither uncertain, difficult, nor complex; therefore, taking the average of a number of cases of analysis, the difference between any two or more competent chemists should not exceed a half per cent, and certainly not such as to constitute the notorious reputations of high and low analysts. It may be that temperament is concerned, and that the high analyst may be so generously inclined as to adopt the Jack-o'-both-sides policy, and, by giving high reports, hope to please buyer and seller; whilst the low analyst, being constituted of more severe and sterner stuff, may care only for his fee, and for neither party.

The claims on chemistry by agriculture are now so extensive and important that I hope these differences between two eminent chemical analysts may be clearly explained, and may not be, as they at present appear, a chemical scandal.—I am, &c.,

W. Little.

The Hall, Heckington,
Lincolnshire.

PS. Since writing the above I observe that a manure maker and a chemist have addressed you on this same subject, as representing a large body of consumers. I hope you will find space for our "growl." Our association now distributes more than four thousand tons of phosphate manure yearly amongst its members, and, in a short time, our demand will be ten thousand tons; consequently, a difference of eleven shillings per ton is a matter of some moment.

* For the present we think it will be better not to publish the well-known names here represented by A. and B.—*Ed. C.N.*

ELECTRO-MAGNETIC THEORIES.

To the Editor of the Chemical News.

Sir,—It is almost needless for me to say that Mr. Highton has altogether misunderstood my meaning when he says "Dr Joule has withdrawn the details of his theory, and substituted others." My object, in the paper to which he refers, was to place the theory in a light which would make it more intelligible to those who have not worked on the subject.—I am, &c.,

James P. Joule.

[As a continued discussion on this subject cannot interest the majority of our readers, we can devote no more space to it.—*Ed. C.N.*]

MISCELLANEOUS.

Royal Polytechnic Institution.—A lecture on "Snow, Ice, and Glaciers," by Mr. Pepper, forms, just now, the staple attraction at this institution. The discourse is one of considerable interest, and is delivered with much tact by the lecturer, who knows well how far he dare trespass upon the patience of his audience without touching the limits of boredom. Some fine photographs of glacier scenery in Switzerland, Tyrol, and the Pyrenees, thrown upon the big screen help to convince the audience of the soundness of the theory of glacier motion which Mr. Pepper brings forward. As usual, there are several other entertainments of a lighter character provided for visitors, and of these, one by Mr. George Grossmith, jun. deserves special mention. This gentleman cleverly hold up to ridicule some of the well-known portrait types to be seen in every photographic album, and moves the audience to laughter by representing to them their own follies. Mr. Grossmith's little entertainment is indeed full of fun and genuine humour.

Improved Automatic Spectroscope.—At the recent *soiree* of the Royal Society, Mr. J. Browning exhibited a spectroscope, in which the rays of light pass through two batteries of prisms, and was then sent back through both trains of prisms by means of a reflecting prism placed behind the last prisms of the train, to the eye of the observer. In this manner a dispersive power equal to nineteen prisms of flint-glass is obtained. Both

trains of prisms, as well as the intermediate prism, are under the control of an automatic movement, which ensures that every ray shall pass at the minimum angle of deviation, for the particular ray under examination. Both the collimator and telescope are fixed; the prisms only are movable—it will readily be understood that this arrangement is highly advantageous. Mr. Proctor suggested this extension of Mr. Browning's automatic spectroscope, and contrived the form of prisms which would enable the automatic action to be carried on, but the mechanical difficulties, in communicating the automatic motion to the second battery were very considerable. These difficulties Mr. Browning has ingeniously surmounted. The D lines in the spectrum of sodium are seen in this instrument, separated by an apparent interval of more than $\frac{1}{8}$ of an inch, and, under favourable conditions of the atmosphere, ten or twelve lines are visible between them. We understand that Mr. Spottiswoode, the Treasurer of the Royal Society, has become the possessor of this instrument.

Quality of the London Gas.—Dr. Letheby, the Chief Gas Examiner appointed by the Board of Trade, has recently reported to the Corporation of the City, and to the Metropolitan Board of Works, on the quality of the gas supplied to the metropolis during the last three months by the Chartered, the Imperial, and the South Metropolitan Gas Companies. He states that the range of illuminating power has been somewhat large in several instances, as in the case of the Chartered Gas, at Gray's Inn Road, and of the Imperial Gas, at Graham Street and Oakley Square, as well as the South Metropolitan Gas, at Hill Street, where the fluctuations have been to the extent of about three candles. The average illuminating power of the common gas supplied to nine testing stations during the quarter, has been as follows:—Firstly, with respect to the Chartered Gas, it was equal to 16·87 standard sperm candles, at Gray's Inn Road; 17·21 candles at Arundel Street; 17·41 at Leadenhall Street; 17·67 at Cannon Street; and 17·93 at Friendly Place, Mile End. Secondly, as regards the Imperial Company's gas, it was equal to 15·68 candles at Camden Street; 16·46 at Graham Street; and 16·49 at Oakley Square. Thirdly, with respect to the South Metropolitan gas, it was equal to 16·15 candles. On two occasions, the common gas of the Chartered Company, at Gray's Inn Road, was a little below the requirements of the Act of Parliament. The average illuminating power of the Cannel gas supplied by the Chartered Company was 23·15 candles at the Cannon Street testing-place, and 25 candles at Arundel Street. As to the purity of the gas, it was reported of as being at all times free from sulphuretted hydrogen, but as containing very variable proportions of sulphur, as from an average of 11·5 grains per 100 cubic feet of the Cannel gas of the City works, to 36·11 grains per 100 feet of the common gas at Gray's Inn Road; and between these extremes, there were the following average proportions of sulphur per 100 cubic feet, namely, 19·69 grains in the common gas from the Great Central works; 25·98 in that from the City works; 26·81 in the Cannel gas at Arundel Street; 27·22 and 28·11 in the Imperial Gas at Graham Street and at Camden Street; 31·35 in the Chartered common gas at Arundel Street; 33·02 in the South Metropolitan gas; 35·21 in the Imperial at Oakley Square; and 35·18 and 36·11 in the Chartered gas at Leadenhall Street and Gray's Inn Road; the range, therefore, in the average amount of sulphur in the Cannel gas supplied by the Chartered Company has been from 11·5 grains at Cannon Street to 26·81 grains at Arundel Street; and in the common gas of the several companies it has ranged from 19·69 grains at Friendly Place to 36·11 at Gray's Inn Road. Dr. Letheby draws attention to these large differences in the amount of sulphur at the different works, and says that the public have a right to the advantages offered by the best system of purification, for in the case of Cannel gas, it must evidently be more perfect at the City works, where the average amount of sulphur is only 11·5 grains per 100 feet of gas, than at the other works, where it is 26·81 grains; and in that of common gas, it likewise must be better at the Great Central works, where it averages 19·69 grains, than at the works which supply the Gray's Inn Road testing-place, where it has averaged 36·11 grains per 100 feet. The quantity of ammonia in the gas supplied from several stations has never exceeded the amount prescribed by the referees, namely, five grains per 100 cubic feet of gas.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Bulletin de l'Académie Impériale des Sciences de St. Petersbourg, vol. xv., No. 3, 1870.

This number contains the following original papers relating to chemistry and collateral sciences:—

Physical Properties and Calorific Power of some of the Petroleums found in the Russian Empire.—H. Sainte-Claire Deville.—This essay treats on the petroleum oils found near to the Caspian Sea, the author having been commissioned by the Russian Government to investigate and report upon the use that can be made of these oils as fuel for steamers plying on the sea just mentioned. The essay is divided into the following sections:—Estimation of the degree of volatility; specific gravity and co-efficient of expansion by heat; elementary composition varied for different samples (but the quantity of oxygen contained in the oils operated upon did not in any case exceed 1·5 per cent, the lowest quantity of that element found in the samples being 0·1 per cent); calorific power, or heat given off by combustion.

The Olivine of the Pallas Iron (Pallas Eisen).—N. von Kokscharow.—A mathematico-crystallographical essay, illustrated with engravings.

Studies on Ozone, Peroxide of Hydrogen, and Nitrite of Ammonia.—H. Struve.—This essay, though marked as a preliminary notice, treats on the presence of ozone, peroxide of hydrogen, and nitrite of ammonia in atmosperic water (rain, snow, hail, clouds), and on the part these substances play in the processes of respiration and digestion (nitrite of ammonia is a constant constituent of saliva and the liquids of the stomach) of men and animals.

Derivatives of Desoxybenzoine.—Dr. Zinin.—This lengthy monograph does not admit of any useful abstraction, notwithstanding the intrinsic merits of the essay, which, to be well understood, would require the reproduction of a large number of formulæ.

Note on the Bodies Corresponding to the Nitrated Products of the Benzoil-Anilide.—Dr. Lazorenco.—A brief paper chiefly containing a series of formulæ.

Jahrbuch der Kaiserlich-Königlichen (Austro-Hungarian) Geologischen Reichsanstalt, No. 4, 1870.

This number does not contain any papers directly relating to chemistry or collateral sciences, but is full of very valuable matter bearing upon the geology, mineralogy, and geognosy of the extensive Austro-Hungarian Empire and some adjacent countries, including Servia and Turkey.

Bayerisches Industrie und Gewerbe Blatt, January, 1871.

This number contains the following original papers and memoirs bearing upon chemistry and collateral sciences:—

Review of the Objects most Noteworthy, in an Industrial and Technical respect, Exhibited at the Exposition held at Cassel.—Dr. E. Wiederhold.—Illustrated with engravings.

The Pigments and Dyes derived from Naphthaline and Phenic Acid.—Dr. L. Janke.—This very lengthy and exhaustive memoir is divided into the following sections:—Naphthaline, preparation and properties; nitro-naphthaline; binitro-naphthaline; naphthylamine (naphtholidine); biamido-naphthaline; pigments obtained from naphthylamine; phenic acid; picric acid; isopurpuric acid; phenyl brown.

Suggestions concerning a Rational Mode of Utilisation of the By-Products obtained by the Manufacture of Gas from Coals.—Dr. A. Wagner.—This essay treats on the best means of utilising coke and breese, gas tar, ammoniacal liquor, and naphthaline.

Revue Universelle des Mines, de la Métallurgie, des Travaux Publics des Sciences et des Arts Appliqués à l'Industrie (double number), November and December, 1870.

This number contains, in addition to a large number of excellent papers relating to mechanical science, enginecring, and railway matters, an essay on the—

Estimation of Sulphur in Cast-Iron, Review of the Methods hitherto Employed, and Description of Two New Processes suited for this purpose.—Dr. L. de Koninck, C.E.—This essay is, in its first portion, a review of the various methods applied, and generally known, for the estimation of sulphur in cast-iron; next, we meet with the detailed description, illustrated by engravings, of two new methods tried by the author and M. Dietz. The principle of the first of these methods is based upon the formation of sulphuretted hydrogen from cast-iron, when treated with dilute hydrochloric acid, and the passing of that gas into a neutral solution of nitrate of silver, and the formation of sulphide of silver, which, being separated by filtration, is treated with bromine in the presence of water, when the following reaction takes place:—

$$Ag_2S+8Br+4H_2O=2AgBr+6HBr+H_2SO_4.$$

Since bromide of silver is insoluble, the estimation of the sulphuric acid, after separation of the insoluble bromide, presents no difficulty. The reader should understand that the authors have devised a properly-arranged apparatus for the execution of this process. The other process is based also upon the evolution of sulphuretted hydrogen from the iron, and the passing of the gas into a neutral solution of sulphate or chloride of cadmium; the result is the formation of sulphide of cadmium, and the setting free of a quantity of acid equivalent to the quantity of the salt converted into sulphide, which free acid is, after the separation of the sulphide by filtration, estimated by means of a titrated solution of ammonia or of caustic potassa.

Zeitschrift für Chemie von Beilstein, No. 2, 1871.

This number only contains the two following original papers:—

Products of the Oxidation of Durol: Cumidic Acid.—P, Jannasch.—Cumidic acid, $C_{10}H_{10}O_4$, is a bibasic acid, homologous with terephthalic, phthalic, isophthalic, uvitic, and xylidic acids. Cumidic acid is insoluble in cold and boiling water, hardly at all soluble in ether and benzol, rather more soluble in boiling alcohol. At a very high temperature, this body sublimes without fusing, previously forming well-defined crystals. Cumidate of lime, $Ca.C_{10}H_8O_4+2H_2O$, forms prismatic crystals, is not acted upon by exposure to air, can be heated to 200° without undergoing decomposition, and is readily soluble in water. Cumidate of baryta, $Ba.C_{10}H_8O_4+2H_2O$, crystallises in rhombs. The author, who is engaged in further experiments on this subject, states that he has found a method of obtaining durol (a hydrocarbon, C_9H_{12}) by the direct methylisation of coal-tar pseudo-cumol.

Some Derivatives of Propionic Acid.—F. Sestini.—The author describes, at length, the mode of preparation of the following substances:—Propionylamide, C_3H_5O,H_2N, a colourless body, readily soluble in alcohol, ether, and chloroform; crystalline; fusing between 75° and 76°; sublimable at a higher temperature. Hydrochlorate (*salzsaures*) of propionylamide, $(C_3H_5ONH_2)2HCl$, a deliquescent substance, readily soluble in alcohol, but not in ether; exhibits acid reaction to test-paper; forms prismatic-shaped crystals. Mercuropropionylamide, $(C_3H_5O)2HgH_2N_2$. Propionyl-anilide (phenyl propionylamide), $C_3H_5O.C_6H_5.HN$. Neutral propionate of ammonia, $C_3H_5O.ONH_4$. Propionic ether is most readily prepared by distilling together a mixture of 24 parts of alcohol at 95 per cent, 18 parts of propionic acid, and 4 parts of sulphuric acid.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin, February, 1871.

This number does not contain any original papers or memoirs relating to chemistry and allied sciences.

Journal für Praktische Chemie, No. 3, 1871.

Behaviour of Normal Iodide of Butyl towards Alcoholic Potassa Solution.—A. Saytzeff.—Continuation and end of this lengthy essay, which, notwithstanding its intrinsic merits, is not well suited for useful abstraction.

Constitution of Diglycolic Acid and Triglycolamidic Acid.—W. Heintz.—This paper, and the next, by A. Claus, are critical reviews of the labours of Professor Kolbe, as published on this subject.

Essay on Structural Formulæ, and on the Doctrine of the Connection (Binding) of Atoms.—Dr. H. Kolbe.—Illustrated by diagrams.

Alleged Unfitness of Potassa for the Formation of Ultramarine.—W. Stein.—This paper records a series of experiments made by the author with the view to elucidate the fact already observed and recorded by the late Professor Gmelin, and afterwards confirmed by Dr. Ritter, that if, instead of soda, potassa be used for the preparation of ultramarine, that pigment is not formed, the conditions of the operation remaining in all respects the same. The results of the author's experiments confirm the original statements, and the explanation of the phenomenon is found in the hygroscopicity of the potasso-alumina silicate, and its consequent more easy decomposition and incapability of preventing the decomposition of the sulphuret of aluminium it envelops.

Volumetric Estimation of Peroxide of Iron by means of Iodide of Potassium.—Dr. A, Schwarzer.

New Phenyl-Diamine.—P. Griess.—The first page of a mono graph on a new phenyl-diamine, different from—

	Point of fusion.	Boiling-point.
α phenyl-diamine (Hofmann) ..	63°	287°
β phenyl-diamine (Hofmann) ..	140°	267°
New phenyl-diamine	99°	252°

The American Journal of Science and Arts, April, 1871.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Some Forms of the Galvanic Battery.—S. P. Sharples.—The account of a series of experiments made with various forms of galvanic batteries and with various exciting liquids. As regards the latter, the author states that the sulphuric acid (1·84 sp. gr.) should be mixed with nine times its volume of water, and allowed to stand until the precipitated lead has settled. The absorbing fluid best suited is stated to be ordinary commercial nitric acid saturated with potassic bichromate, best at a moderately warm temperature. To this solution is added one-third of its bulk of sulphuric acid, and enough water to re-dissolve the chromic acid precipitated; this mixture is less expensive than pure nitric acid alone, and the internal resistance is decreased.

Method of Fixing, Photographing, and Exhibiting the Magnetic Spectra.—Dr. A. M. Mayer.—Reserved for full reproduction, an observation also applicable to the following valuable paper:—

Determination of the Alkalies in Silicates by Ignition with Carbonate of Lime and Sal-Ammoniac.—J. Lawrence Smith.

This number abounds in valuable and interesting papers bearing upon geology and astronomy.

NOTES AND QUERIES.

Sugar Refining.—(Reply to J. T. A.)—Dr. Seyferth's process alluded to by you, not for *sulphuric*, but for *sulphurous acid*, is described in Chemical News, vol. xxii., p. 248.

Densimetre.—Would any of your readers inform me how, and on what principle, the "densimetre" is worked? It is, I believe, an apparatus used to determine the density of gunpowder.—Charcoal.

Mixed Fabrics.—In Chemical News, vol. xxii., p. 169, you published a paper "On the Discrimination of Fibres in Mixed Fabrics." Would the author, Mr. Spiller, please say what strength (Twaddell) the hydrochloric acid should be?—X.

Valuation of Orchil.—Will any of your readers kindly inform me of a ready method among analysts for the quick valuation of *Orchella* weeds? A process exists of dyeing a piece of flannel with the extract, the flannel being subsequently submitted to comparison with other pieces similarly dyed with other samples, but I have not been able to arrive at this end within less time than ten days, whereas I believe it is to be done in a much shorter space of time.—Orchil.

Improved Bunsen Battery.—At the last meeting of the British Association for the Advancement of Science, Professor Williamson brought forward, very prominently, a notice and partial description of a new galvanic battery, invented by Professor Bunsen. He (Professor Williamson) stated that he could not, as yet, give the form, dimensions, exact composition of the exciting liquid, &c., Professor Bunsen, owing to the war, not having at that time forwarded them. He further stated, however, that, *cetcris paribus*, the electromotive force of the new battery, compared to a Grove's, was as 23 to 18; that it required no porous cells or partitions, and had only one (compound) exciting liquid. I have looked through the Chemical News, &c., for it ever since, but have failed to see any account of it. If it is published, I am desirous of ascertaining where. It seems to me to be a matter of great importance if electricity is ever to be of use as a motive power, also for medical and various other purposes.—James M. McCulloch.

MEETINGS FOR THE WEEK.

Monday, May 1st.—Medical, 8.
—— London Institution, 4.
—— Royal Institution, 2. Annual Meeting.
Tuesday, 2nd.—Zoological, 9.
—— Civil Engineers, 8.
—— Royal Institution, 3. William Pengelly, Esq., F.R.S., F.G.S., "On the Geology of Devonshire, especially of the New Red Sandstone System."
Wednesday, 3rd.—Society of Arts, 8.
—— Microscopical, 8.
Thursday, 4th.—Chemical, 8. Dr. Voelcker, F.R.S., "On the Productive Powers of Soils in Relation to the Loss of Plant Food by Drainage."
—— Royal Society Club, 6.
—— Royal, 8.30.
—— Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
Friday, 5th.—Royal Institution, 9. W. R. S. Ralston, M.A., "On Russian Folk-Lore."
—— Geologists' Association, 8.
Saturday, 6th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."

THE CHEMICAL NEWS.

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ON THE ESTIMATION OF PHOSPHORIC ACID IN SUPERPHOSPHATES.

By ROBERT WARINGTON, F.C.S.

THE fact to which Mr. Purser has called attention in a recent number of the CHEMICAL NEWS (vol. xxiii., p. 178), is one only too well known to those concerned in the manure trade. There are, unfortunately, considerable discrepancies in the reports of analysts of superphosphates; a little ventilation of the subject cannot but prove wholesome.

In the first place, are analysts agreed as to what they understand by soluble phosphate; do they understand phosphates soluble in hot or in cold water?

Exhaustion of the superphosphate with hot water may give, according to circumstances, more or less soluble phosphate than exhaustion with cold water. If the superphosphate contain soluble compounds of alumina, it will yield less soluble phosphate when boiled with water than if the exhaustion is conducted in the cold, and the difference between these two modes of operating may be very considerable. A simple experiment will satisfy any one on this point. If a cold solution of superphosphate be treated with alum, or other soluble compound of aluminium, the solution appears unchanged. On applying heat turbidity commences, and when the boiling-point is nearly reached a copious precipitation of phosphate of aluminium takes place; on cooling, the precipitate, if not too considerable, entirely re-dissolves. This reaction is certainly remarkable, and the cause not very apparent: one is tempted to think that the aluminium is, in the case assumed, combined in the cold with sulphuric acid, and in the hot with phosphoric, and that a change of temperature effects a change in the degree of affinity of aluminium for these two acids. The practical result is, however, very plain; superphosphates containing aluminium in a soluble form will yield less soluble phosphate to hot water than to cold.

Most superphosphates yield solutions in the cold which contain traces of alumina; when, however, the superphosphate has been made from Navassa phosphate, or other of the native phosphates rich in alumina, the amount of aluminium salt soluble in the cold may be very considerable; the quantity depends, however, in great measure, on the manner in which the superphosphate has been manufactured. The presence of aluminium in the solution is, of course, at once indicated by the formation of a precipitate on boiling.

Before leaving this part of the subject, I may just mention, that the reaction of ferric salts with solution of superphosphate is wholly different from that described above. Soluble ferric salts produce an immediate precipitate in the cold, and are, consequently, never found, save as traces, in a solution of superphosphate.

From the above considerations, it seems evident that the removal of the soluble phosphate should always be effected with cold water. The maximum amount of phosphoric acid appears to be dissolved when, after thorough exhaustion with cold water, the residue is lastly boiled. In this case, the hot water probably effects a partial decomposition of the reduced phosphates. The increased amount of phosphate rendered soluble by the hot water is by no means considerable, and can hardly be fairly reckoned as soluble phosphate, which, in its character of a manure to field crops, must surely be understood as a phosphate soluble in rain water.

The best mode of extracting the soluble phosphate is, I believe, the following:*—10 grammes of the carefully mixed sample are placed in a mortar. A flask, marked to deliver 1 litre of water, is filled, and fitted as a wash-bottle. The superphosphate is moistened with some of the water, and rubbed smooth with the pestle; more water is added, the whole well stirred, and the solution and fine particles decanted into a bottle. The residual superphosphate is brought to a finer state of division by means of the pestle, and is again treated with water as before; and this rubbing and washing are repeated until the whole of the manure has been transferred to the bottle in a fine state of division. The rest of the water is next added, and the bottle shaken at frequent intervals during three hours. 100 c.c. of the solution correspond to 1 grm. of the superphosphate.

The estimation of the phosphoric acid in the watery solution may, of course, be effected by any of the trustworthy methods with which chemists are acquainted. The most usual plan is to precipitate the phosphoric acid as tricalcic phosphate by means of ammonia, with or without the addition of chloride of calcium, according to the proportion of lime present in the solution. The other methods which seem specially applicable, are precipitation by magnesia, and by uranium.

Precipitation by acetate of uranium cannot be used when alumina is present in any notable quantity, unless the phosphate of aluminium is first removed by acetate of ammonium and separately determined. The uranium method when used gravimetrically, is a very good one: employed volumetrically, it is admitted to be somewhat less accurate.

The magnesia method is available under all circumstances, and is worthy of great confidence. The superphosphate solution is to be treated with excess of oxalate of ammonium; the acid phosphates present liberate enough oxalic acid to keep the phosphates of aluminium and iron in solution. The oxalate of calcium is removed by filtration, and the filtrate treated with citric acid, excess of ammonia, and magnesia mixture.

The usual plan of estimation as tricalcic phosphate, is of quite sufficient accuracy for commercial purposes when properly conducted, but lies open to several distinct sources of error. Thus the "high analyst," by precipitating with a considerable excess of ammonia from a solution containing excess of lime, obtains a phosphate containing notably more calcium than tricalcic phosphate, and his result is, consequently, above the truth. This error is met by some by re-dissolving the phosphate containing excess of lime, and re-precipitating it; the error may, however, be avoided in the first instance, by limiting the amount of ammonia employed. If ammonia is only used in just sufficient quantity to distinctly turn turmeric red, tricalcic phosphate may be successfully precipitated from cold dilute solutions containing much lime in excess. An error of a different kind arises from the difficulty of washing all the sulphates out of the gelatinous phosphate, and from the fact that the washing must not be pressed too far, as loss of phosphate occurs in the later stages of washing. I would strongly advise those commercial analysts who may be in the habit of always estimating soluble phos phate by precipitation with ammonia, to test their result a few times by duplicate analyses executed by the magnesia method, and thus ascertain whether the conditions they habitually employ are consistent with accuracy.

The following results, obtained with two superphosphates, show the amount of agreement that may be expected from the different methods described:—

	Estimation as tricalcic phosphate.	Estimation as phosphate of magnesium.	Estimation as phosphate of uranium.
(1).	24·65 24·55	24·19	24·90
(2).	17·3	16·82	16·50

The determinations of insoluble phosphate in superphosphate are, I fear, more generally inaccurate than

* This method, is I believe, of German origin; I became acquainted with it at Mr. Lawes' laboratory at Harpenden.

those of soluble phosphate; they are usually greatly in excess of the truth. The manufacturer can tell very nearly the total amount of phosphate present in his manure. Thus, if he makes superphosphate with equal weights of Cambridge coprolite and sulphuric acid, he knows that, allowing for the loss in mixing, the product will contain almost exactly 32 per cent of phosphate reckoned as tricalcic; and any nitrogenous matter he may have added to his materials will only diminish the percentage of phosphate. What, then, is his surprise, on receiving the report of the scientific chemist, to find that the soluble and insoluble phosphate added together give a total of over 40 per cent; a result not very uncommon. If commercial analysts would remember that only high-priced superphosphates, made entirely from Sombrerite or bone-ash, can possibly contain 40 per cent of phosphate, they would save themselves from many mistakes.

The origin of this error is that less care is taken in determining insoluble phosphate than in the estimation of soluble: that the portion of the superphosphate undissolved by water is dissolved in acid, and the solution precipitated with a large excess of ammonia, without any attention to the nature of the substances precipitated.

It is best to determine the *total* phosphate present in the superphosphate by one operation, and then, by deducting the soluble phosphate, to find the insoluble. If the superphosphate has not been made from ferruginous or aluminous materials, the solution of the superphosphate in hydrochloric acid, and the precipitation of the phosphates by the limited amount of ammonia already referred to, will give sufficiently accurate results. In all other cases, the hydrochloric solution of the superphosphate should be neutralised as far as possible without causing a precipitate, and then treated with excess of oxalate of ammonium, and subsequently with citric acid and magnesia mixture as already described.

In conclusion, it must not be forgotten that the faulty processes in use by commercial analysts are to no small extent due to the pressure put on them by the public. When the fee given is reduced to the lowest possible point, the analyst is forced to regard the number of his analyses as of more importance than their quality, and to adopt methods, the main object of which is to enable him to snatch a result in the shortest possible time.

ON THE EXISTENCE AND FORMATION OF SALTS OF NITROUS OXIDE.*

BY EDWARD DIVERS, M.D.

METALLIC sodium thrown on a solution of an alkali-nitrate was found by Schönbein to reduce it to nitrite. He contented himself, however, with merely detecting the nitrite by the iodide and starch test. By using the sodium in the form of amalgam the complete reduction of the nitrate to nitrite can be readily effected, and silver nitrite freely precipitated from the solution, by first neutralising it with an acid, and then adding silver nitrate.

But so soon as nitrite is thus formed by the sodium, it itself begins to suffer reduction, as well as the remaining nitrate, by the action of more sodium. This reduction of the nitrite is rendered evident by the effervescence which attends it, the gas given off consisting of pure nitrous oxide. If excess of sodium amalgam be gradually added to the nitrate solution, and its action moderated by keeping the vessel containing the mixture in a stream of cold water, the effervescence only becomes very lively when the sodium added has nearly reached the proportion of two atoms to one of the nitrate used. When four atoms of sodium have been oxidised by the solution, the further addition of it is without effect; no more effervescence takes place, and the sodium remains unchanged in the mercury.

* Abstract of a paper read before the Royal Society, April 19th, 1871.

The very alkaline liquid which is left by the reaction contains a new salt, though in relatively small quantity—the salt of nitrous oxide.

After neutralising the alkaline liquid by acetic acid, it gives a yellow, pulverulent precipitate with silver nitrate. This precipitate, when thoroughly washed from its saline mother-liquor, is almost as insoluble in water as silver chloride; for hydrochloric acid gives no immediate opalescence with water filtered through it. It is quite stable below 100° C., or a little lower than this, and it may be washed with hot water without change. It is also unaffected by light, or by exposure to a pure atmosphere, even when in contact with paper. It is but very sparingly soluble in acetic acid; so that this acid may be added in excess to the original alkaline liquid without removing its property of being precipitated by silver salts. It is soluble in ammonia and ammonium carbonate, from solution in either of which it can be again thrown down by acetic acid, or by cautious neutralisation of the ammonia by dilute nitric or sulphuric acid, or by the volatilisation of the ammonia. It is soluble in either dilute nitric or sulphuric acid, and without immediate decomposition; and it can be re-precipitated from its solution in either of these acids by the cautious addition of ammonia or ammonium carbonate, or by the free addition of sodium carbonate or sodium hydrate, in either of which it is insoluble. It is immediately oxidised by concentrated nitric acid, copious red fumes being produced. Moderately diluted nitric or sulphuric or hydrochloric acid decomposes it, with the evolution of nitrogen, and apparently the production of both nitrous and nitric acids in the solution. It is also decomposed by soluble chlorides and by hydrosulphuric acid. When precipitated from the original liquid, it sometimes becomes dark-coloured; but this change is due to the formation of a black matter derived from the sodium or naphtha and the silver acetate. After washing it, dissolving it in very dilute nitric acid, and filtering the solution, it may be re-obtained by the cautious addition of ammonia, or by the addition of ammonia to alkaline reaction and then a little acetic acid, in a condition in which it is no longer liable to discolouration. It is decomposed by a moderate heat into nitric oxide, metallic silver, and a little silver nitrate—in this respect resembling silver nitrite. My experiments on silver nitrite, about being published, show that nitric oxide may serve as a carrier of atmospheric oxygen to silver nitrite; and it is therefore most probable that in this case also some of the nitric oxide liberated serves to carry a little oxygen to the still undecomposed new silver-salt. During its decomposition by heat it does not fuse or exhibit any other change except that from a bright yellow to a silver-white colour. After a red heat nothing remains but pure silver.

The percentage numbers for the silver found and that required by the formula—NOAg—leave no doubt as to the composition of the salt, although the silver comes out about one per cent too low. But in several of the samples traces of brownish or black matters were detected on solution; and in the last preparation analysed the presence of a little moisture was detected; and a further heating of some of it not used for analysis to dry it more thoroughly caused decomposition to commence.

If the product of the ultimate action of sodium on the nitrate be treated with acetic acid, as directed, until it is neutral to test-paper, it gives no precipitate with any metallic solution except that of silver, so far at least as trial has been yet made. But the precipitation of the silver-salt leaves the solution of an acid reaction to litmus; and even if the solution before precipitation be rendered a little alkaline to litmus, it will, after precipitation, generally react slightly acid. The reason of this is clearly that the sodium salt has a markedly alkaline reaction on litmus; and this is shown to be the case by adding some of the washed silver-salt to a solution of potassium or sodium chloride, which it at once renders alkaline in reaction to litmus. To obtain a solution of the salt free from acid or caustic alkali, the original alkaline liquid is to be treated

with acetic acid in successive quantities until it just ceases to yield a brown precipitate with silver nitrate. Such a solution will yield the yellow silver-salt with silver nitrate as before, and also precipitates with the salts of most of the common metals. Instead of acetic acid, dilute nitric acid may be used in this method for thus neutralising the caustic soda, with the same result as to the alkalinity of the solution to test-paper, and its capability of being precipitated by many metallic salts.

The following reactions having as yet been only observed with a solution thus obtained, it is possible that some of them may not be due simply to the new acid.

Barium chloride gives no precipitate.

Lead acetate gives a cream-white flocculent precipitate, which generally, on standing, changes to a very dense, full yellow precipitate. This precipitate is unchanged by boiling in water, or in its mother-liquor, is soluble in acetic and other acids, slowly, if at all, affected by ammonia or sodium carbonate, and instantly decomposed by caustic soda.

Mercuric chloride gives a cream-white flocculent precipitate.

Mercurous nitrate gives a blackish grey precipitate, not improbably a mixture of mercury and the mercuric salt.

Cupric sulphate, a yellowish olive-green flocculent precipitate, soluble in acids and ammonia, dissoluble in caustic soda, and unaffected by boiling in water or its mother-liquor. The colour of this precipitate closely resembles the colour of a copper salt mixed with a nitrite.

Zinc chloride gives a white precipitate.

Manganese chloride gives a whitish precipitate.

Nickel chloride gives a greenish, almost white precipitate.

Cobalt nitrate gives no precipitate (?).

Alum gives a white precipitate.

Ferric chloride gives a slight reddish-brown precipitate.

Ferrous sulphate gives a whitish precipitate, instantly darkening to dirty blackish green, and eventually reddish brown. On addition of ferric chloride effervescence slowly commences, and with ferrous sulphate the same thing happens, but still more slowly. It is very probable that the iron and aluminium precipitates are simply hydrates; and if so, the action of this salt closely in these cases resembles that of a carbonate.

Silver nitrate behaves as already described.

Sodium chloride gives an alkaline solution with the silver-salt, which is changed to chloride.

Ammonium chloride gives the same result as sodium chloride, but the solution at once evolves ammonia. The existence, therefore, of the ammonium salt of the new acid is problematical.

Potassium permanganate gives the beautiful changes of colour, from its own violet through purple, blue, and chrome-green to manganate green, and then a brown precipitate. The exhibition of this series of colours is more certain in the presence of a little caustic alkali. In this reaction the new salt resembles a nitrite, but it is much more sensitive than this.

Potassium iodide gives no reaction; it does with nitrite.

Iodine solution is decolourised immediately; so that if a nitrite, not in excess, is added to a solution of the new salt, the addition of starch and potassium iodide produces no reaction.

The solution acidified with acetic or hydrochloric acid still gives no reaction with *potassium* iodide, and decolourises iodine solutions, and prevents the action of a nitrite on an iodide.

The acidified solution gives no colouration with ferrous sulphate. In contact with strong sulphuric acid, the mixture gives the colouration like a nitrate does. As is well known, a nitrite, even without addition of acid, gives an immediate colour with ferrous sulphate.

The acidified solution immediately decolourises *potassium permanganate*.

The acidified solution does not reduce *potassium dichromate*.

The solution which has been neutralised with nitric acid instead of acetic acid, will not do for the iron and iodine tests, as this behaves, so far as it has been tried, as if it contains a nitrite.

The acidified acetic acid solution, when heated, evolves *nitrous oxide*. Here again, therefore, the new salt is analogous to a carbonate—$2NOH = N_2O + OH_2$.

When silver nitrite is heated until a greenish yellow, semifused mass remains, and this is washed out with water, the residue consists of metallic silver and a little bright yellow matter, unaffected by light, soluble in ammonia, and decomposed by boiling in water, as will be found described in my paper on the action of heat on silver nitrite already referred to. From the properties of this yellow substance, and from the manner in which it is formed, it is probable that it is the silver-salt of the new acid; but in consequence of the small quantity of it obtainable, and of the admixture of this with metallic silver, a fuller examination of it has not been attempted. If it be this salt, its formation is analogous to that of silver nitrite by heating silver nitrate.

There is some difficulty in selecting an appropriate name for the new acid. That of hyponitrous acid naturally suggests itself as being framed according to the usual method of naming a rising series of oxygen acids, and as associating the new acid with the similarly-constituted acid of chlorine. But I feel that in naming the nitrous-oxide acid regard ought to be had to the possible existence of salts of nitric oxide (which several chemists have believed in), and on better evidence, I think, after a perusal of some of their papers, than is generally supposed. Now, if the nitric-oxide acid should be discovered, and if the term "hyponitric" is to be retained for the acid intermediate to the nitrous and nitric acids, the term "hyponitrous" would belong by analogy to the nitric-oxide acid rather than to the new acid.

This difficulty, however, will vanish if the term hyponitric be allowed to fall out of use, and that of nitroso-nitric, already adopted by several chemists, be generally substituted for it as the name of the nitrogen-peroxide acid; for then the term "hyponitrous" being given to the new acid, there remains the compound term "hyponitroso-nitrous" for the nitric-oxide acid, should this acid ever be obtained.

There will thus be the following series of names:—

Hyponitrous acid	HNO
Hyponitroso-nitrous acid	$H_2N_2O_3$
Nitrous acid	HNO_2
Nitroso-nitric acid	$H_2N_2O_5$
Nitric acid	HNO_3

If, however, the terms "hyponitrous" and "hyponitric" are to be retained for the second and fourth members of the above series, the term "hydro-nitrosylic" may serve for the new acid, according to its constitution.

Hydrogen.	Nitrosyl.
H	NO

ON BOILED OILS AND VARNISHES.*

By CHARLES W. VINCENT.

(Concluded from page 199.)

The risk of boiling over, and being the cause of a conflagration, was one reason for the distance at which oil-boiling businesses were formerly kept from towns; the abominable smell produced by the evaporation and partial decomposition of the oil was another. Nevertheless, it is not convenient, in the extension of businesses of a disagreeable character, to be constantly driven from the centres of industry, where their articles of manufacture have ultimately to be used, and this has been particularly the case with the oil trade. In a place like London, a very large

* Read before the Society of Arts.

part of the consumption of boiled oil must be local; or if the oil be exported it is the same thing as far as the manufacture is concerned, each mile he is driven away from his customer decreasing his profits, without any corresponding advantage. It became, therefore, a matter of absolute necessity that means should be taken to stop the intensely disagreeable smells arising from oil-boiling tainting and poisoning the air of the surrounding neighbourhood. This has been arrived at in a perfect manner, as an adjunct of the steam process.

The best form of pan for oil-boiling is made of copper, on account of its conducting heat better—an iron one will give equally good results, a little more time being, however, required—circular in shape, with a depth about equal to its diameter, and a rounded bottom. This pan is surrounded for half its depth with an iron jacket, between which and its body steam is admitted as the source of heat.

The jacket and pan should be capable of standing a working pressure of 40 lbs. per inch. The top of the pan is closed by a dome rivetted to it and forming part of it. This dome is provided with a man-hole, and in the centre is furnished with a stuffing-box, through which pass two shafts, one encircling the other, and each bearing a fan, which fan, by gearing outside the pan, are made to rotate in opposite directions, intersecting each other in so doing, and, as may be conceived, producing a most thorough admixture of any solid and fluid substances submitted to their dashing and cutting action. At one side of the top of the dome is a kind of cupola, from the top of which issues a three-inch pipe, which is carried down to the ash-pit below the boiler furnace. When at work, care is taken that every joint is tight, hence any vapours arising in the interior of the pan have no chance of escaping into the air till they have passed the burning coals. One of the greatest dangers and also disagreeable nuisances being thus obviated, oil-boiling can be and is carried on without objection on the part of either the insurance companies or the public at large, even in the heart of densely-populated districts. I myself boiled oil and made varnish in Milford Lane, Strand, for three years, without increasing the insurance of the *Illustrated London News*, though I used steam from their boilers for the purpose.

At the lower part of the pan, the inch-pipe from a powerful air force-pump penetrates through the jacket to the interior of the pan.

The workman's first care is to see that all his taps and valves, are in good, efficient order, and turned the right way, after which the mode of manipulation is the following:—

The linseed oil is first shot into a large tank, holding one batch for boiling (about 2 tons is the usual quantity), and allowed to settle for four or five hours, according to the time taken for boiling the previous batch, matters being so arranged that as soon as one batch is pumped up into the pan another is shot into the tank, so that the oil may have as long a time for freeing itself from mechanical impurities as can be spared for that purpose. The waste steam from the pan passes through a coil of inch and a half iron pipe placed in this tank, so that the oil is also being warmed—this much facilitating the separation of particles of dust, mucilage, &c., and also saving time when the actual boiling commences. The time having arrived when the pan is ready, the oil has acquired a temperature of about 95° F. before it is pumped up (in the works I am describing the arrangement of pumps used took about seven minutes to elevate 2 tons of oil 20 feet), full pressure of steam is then turned on the pan, and the fan started. At first there is little or no disagreeable smell to be perceived. If the man-hole of the pan be open, an observer can watch the oil churning round with very little frothing, although the blades of the two fans, rotating in opposite directions, cause such a perfect commingling of the oil and the air that is in the upper part of the pan, that looking in upon the whole fluid mass, it seems as if the bulk were greatly increased, as it goes on swelling and chafing, with a hollow rumbling noise, at the rough treatment it is receiving. A few minutes later, a faint, sickly odour is to be perceived, something like the smell of the raw oil, but intensified as to its woody constituents. The oil is now nearly hot enough for the air to be turned on. This may be done as soon as the steam reaches a pressure of 35 lbs. to the square inch on the pan. Directly this is done, the character and the appearance of the oil changes; the bulk appears to be still more increased, a foaming, seething, whirling vortex presents itself, the colour suddenly changes from the dark brown it previously exhibited to a mass of pale yellow bubbles, and the strong pungent odour universally accompanying the boiling of oil makes itself known and felt by the discharge from eyes and nose, if the curious observer does not take the hint that it is time the man-hole was closed, and the cauldron left to complete its witchery in darkness and solitude, if not in silence. But before it is left to itself, it may be well to observe that, before the air was turned on, a taper would burn in any part of the space above the oil where it was safe from being extinguished by splashes. After the air has been blown through the oil for a short time, if both the flue and air-pumps be stopped, so that a perfectly quiescent atmosphere remains above the oil, it is found to be perfectly incapable of supporting combustion. A few inches inside the man-hole, I have found that a brown paper torch, dipped in turpentine, has been immediately extinguished.

If a dark boiled-oil is desired, the driers should be put in as soon as the oil is thoroughly heated throughout its bulk, which is commonly about half an hour after the steam has an indicated pressure of 35 lbs. They are ground to a fine powder, mixed with oil, and are introduced through a funnel with a stop-cock intervening between it and the pan, in a thin stream, so as to secure as perfect a distribution of them as possible throughout the oil. The quantity of driers used amounts only to about three quarters of a pound to each hundred weight of oil, so that the necessity is obvious of great pains being taken to secure and maintain as complete a diffusion through the liquid as can be attained, and thus coincide with the theory of this method of oil-boiling, which requires that each particle of oil should be in contact or proximity to a particle of the drier used and oxygen at one and the same time.

Subsequent to the introduction of the driers, no attention is required to the process save to see that the air-pump and fan are kept constantly at work, and that the pressure of steam on the pan does not sink at all below 30 lbs.; 35 lbs. being that which ought to be adhered to. The right quantity of air to be introduced for the oxidation of a given quantity of oil has not been ascertained. In practice, it is found that some oils require, and will consume, a larger amount than others; the custom is to throw in as much as the oil will take without frothing up and priming into the condenser. The cooling effect of the air thus introduced is much less than might be supposed; in point of fact, the pressure to which it is subjected in overcoming the weight of the oil, lifting the heavy valves, and the friction tubes it passes through, considerably raises its temperature before it enters the pan; it is generally found to be raised some 20° to 30° above the temperature it previously had by the time it reaches the oil. Indeed, the tubes through which it passes not unfrequently become hotter than the bare hand would be able to hold in a firm grip. In four hours' time the oil is generally fit for removing from the pan to a tank, where it can remain a sufficient time for the major part of the driers to settle out from it. When bright, it is then either pumped or run off into other tanks for warehousing, or may be allowed to remain where it is for future use, if there are plenty of working tanks.

The mode of emptying the pan is by allowing the oil to rush out through a two-inch tap in the centre of the bottom of the pan into pipes which are connected by running joints, and lead it in any desired direction.

Sometimes a little difficulty occurs in dropping the oil, through the orifice of the tap leading into the pan becoming stopped up by an accumulation of driers, or by pieces of the old skins of oil which may have fallen from the sides of the pan. This may be entirely obviated by causing the air to enter the pan through this tap, and putting the second tap beyond the air inlet as an outlet for the oil. The pressure of the air-pump will then suffice to remove any ordinary obstruction; but if a stoppage, nevertheless, should occur, nothing remains but to probe the tap with a long thin rod from the top of the pan. Until this simple expedient of letting in the air through the oil outlet was adopted, this terrible nuisance was of almost daily occurrence.

The vapour which comes off from hot oil into which air has been blown is so intensely acrid and pungent that human nature seems to be incapable of ever getting used to it. For my own part, I would much prefer entering a freshly-opened bleaching powder chamber to an oil-pan, even after the oil has been run off, and the pan cooled down. There was no task in the works more thoroughly abhorred than that ten minutes to a half-an-hour, occasionally longer, during which, with the man-hole open, the workman was raking and poking between the radii of the fans to clear and find the outlet. With this now happily occasional exception, and for the few minutes during which the hot oil is being transferred from the pan to the tanks, this method of oil-boiling produces no disagreeable smell in the vicinity, the fumes being carried away with the surplus air-pump through the oil, and perfectly consumed, so far as noxious odour is concerned, in the furnace of the boiler. Members of this Society have frequently the opportunity of witnessing the efficacy of the operation of this process during some of the lectures delivered here, the air of this hall being kept free from troublesome fumes by a similar arrangement. From this paper the chemical questions involved must be, for the most part, excluded, as it would be scarcely possible for me to enter far into the subject without betraying confidence, the driers used and their *modus operandi* being essentially connected. I may state, however, that the air does not play the part which many have assigned to it, and does in reality effect nothing towards making the oil a drying one. I have boiled linseed oil with air alone, but without driers, for three days consecutively, keeping up a high temperature the whole time, and the resultant boiled-oil has taken precisely the same time to dry as the raw oil from which it was prepared. The body, however, has so much increased that the consistency was more that of a varnish than an oil.

If oil be subjected to heat alone for the same space of time, without any air except such as comes in contact with its surface, there is no such increase in consistence as in the former case; the oil simply becomes more greasy, has less difficulty in penetrating capillary tubes (as, for instance, those of paper, plaster, &c.) than it previously had, and has decidedly less drying powers. The oil which has been boiled with air is less greasy and has a greater consistence. To sum up the process in a few words, I think we show we have the means at hand of meeting all the requisites which were stated to be necessary. The close apparatus in which the work is performed secures the manufacturer alike from annoying his neighbours and injuring his own men. The air and heat secure a sufficient amount of body. The driers produce any required shade of colour. The time of drying *i.e.*, about six hours in summer, and eight hours in winter, according with the old standard of drying power. Oil can be made to dry in much less time, but this is not considered to be desirable. The mechanical appliances reduce the time of the operation to about one-fourth of that which in former years was thought essential. In the ordinary working day, starting with cold oil and a cold boiler, two batches, of two tons each, can be boiled without causing undue driving of men or machinery.

Before leaving this part of the subject, it may be as well to note, that for boiled-oil which has to be exported, long experience has shown that it is advisable in all cases to add to each barrel or drum of boiled-oil a small proportion of raw oil. After a one or two months' voyage the oil becomes brighter than it was when first shipped, and the risk of becoming what is called "fatty," and not free enough in working, is thereby avoided. For a three months' voyage, one gallon of raw linseed may be added to each four gallons of boiled-oil with most beneficial results, particularly if the boiled-oil be anything under one month old.

Varnishes.

As you all know, the main uses to which varnishes are applied are to protect the material over which it is spread from atmospheric influences, accidental rough usage, &c.; to bring out and display more fully the varying texture of different woods; and, finally, to give a fine gloss or appearance of polish to the surfaces to which it is applied. As commonly constituted, an oil varnish (and with these alone I propose to deal) consists of some hard gum dissolved in linseed oil. This is the substance of the varnish, the two being re-dissolved in turpentine to afford the means of spreading them out in thin layers. As far as regards the ultimate value of the varnish, the turpentine is merely so much waste material. It may, if it be of inferior quality, injure the varnish with which it is mixed, but however good it may be, it can do nothing to benefit or improve a varnish which itself is in reality nothing but the mixture of various proportions of some one or more hard gums in linseed oil.

For the convenience of better understanding the *rationale* of the mode of precedure, it will be well if we commence our observations at the end instead of beginning of the process.

Turpentine, being applied for the purpose of thinning down the body of the varnish, and that body being, when cold, almost solid, has to be added whilst the mixture of gum and oil is still hot. The heat causing an increased evaporation, and consequently a loss of turpentine, the temperature is allowed to get as low as can be, consistently with a perfect and complete admixture of the spirit with the matter being secured. This is, to some extent, contrary to old-fashioned notions, but the varnishes resulting from mixing in the turpentine at a high temperature and at a low one being identical, the loss of so much evaporated material is not now taken as an essential part of the process. We have also one less risk of fire, since the whole varnish is now removed far from any fire, and out into the open air when practicable, before the turpentine is added. The quantity of turpentine required under these circumstances is much less than that stated in the various published recipes for varnish making. Enough is commonly added to bring the whole mixture to a consistence a little stiffer than linseed oil. The loss by evaporation, whilst clearing and ageing in tanks, previous to its being sent out, gives it the amount of body which you are accustomed to see it have. The next thing to consider is the body of the varnish. Time would not permit me to give a complete description of the various gums which are or may be used; I can do no better than refer you to Mr. Neil's paper for information on this part of the subject. There is one gum, however, which has come very largely into use since his time, and that is the "kauri," New Zealand gum. It is rather dull in appearance, but it is tolerably hard, and melts at so low a temperature that the dust and chips made by cleaning the outside of large pieces can be utilised by a skilful man, without the gum being coloured by the carbonisation of the woody matter in it. The varnish made from the better qualities of kauri have a very good gloss. When dry, they are pale in colour; dry quickly, and are not so liable to crack when exposed to the sun as better varnishes. After a few months' exposure to wet, however, the whole of the gloss disappears, and though the

protecting surface remains, it becomes, through its abrasion, a harbour for the dust and dirt, and is thus rendered far from ornamental to the place of its attachment.

The temperature at which the mixture of gum and oil intended to be used melts is now recognised as forming a basis for the temperature to which the oil is to be heated previous to the introduction of the melted gum, the whole running, when the two are fairly incorporated, being also kept much under the degree of heat which was formerly considered necessary. In fact, it is in this respect that the present mode of varnish-making chiefly differs from the modes formerly employed, the greatest care being now taken to keep the temperature throughout the process as low as is consistent with perfect admixture of the several ingredients.

The next point to which I shall direct your attention is the apparatus used for the modern process of varnish-making. In the first place, iron vessels generally take the place of copper ones for all common varnishes, the bottom of the gum pots alone remaining copper, as heretofore. The lower temperature employed is found not to affect the metal, and the introduction of impurities from that source is no longer feared.

In the next place, instead of heavy copper stirrers, light thin plates at the end of the rods are used to cut the gum, &c., the greater velocity with which they can be manipulated securing more perfect mixing than of old.

In the third place, a tramway is laid down from the furnace in which the mixed gum and oil is heated, which usually runs from the shed into the open air. If, therefore, from any accident the mixture takes fire, it can be at once removed to a place of safety, where it can be put out at more leisure.

The boiling pot, which holds about 100 gallons, has a closely-fitting conical cover, which, if it can be put on, at once extinguishes the flame if it be not too far a-head. The last improvement has only recently been introduced, and is not yet generally adopted. Two galvanised iron shafts are erected side by side; one corresponds with the gum pot, the other with the boiling pot; the upper end of these shafts alike communicates with the main furnace shaft. The lower ends are fitted with caps, which are so balanced by counterpoises that they can be slid up or down their respective columns. To these caps heads are attached at right angles, which can be brought over, and which fit closely on to the gum pot and boiling pot respectively, and, when in that position, have free communication with the chimney shaft. The front of each hood is cut away and fitted with a diaphragm, slit in such a manner that the contents of the pots can be stirred, and is removable when it is desired to see into them.

The advantages of this arrangement are obvious without further description; in place of the workmen being annoyed by the dense and pungent vapours which escape from the heated gum and oil, the whole are removed, and nothing whatever comes into the air of the shed except at the time when a pot of the gum is being tilted into the boiling pot, or when the whole has to be removed into the air. The reduction of smell, as regards the neighbourhood, is so great, that in the comparatively rare intervals when a disagreeable vapour does come off, the air of the yard mixes with it sufficiently to neutralise the effect exterior to the works. This latest improvement is due to Mr. Bewicke, of Hackney Wick, who, in consequence of a disastrous fire at his works having been caused in a great measure by the men being so suffocated with the fumes from the boiling pot that they were unable, when it took fire, to extinguish it in time, has been impelled to seek for such more perfect appliances as would bring the whole system more under control than had previously been the case. I think you will agree that the plan deserves to succeed, and, from the experience it has already had, I believe it will do so. Old varnish makers, standing at the furnaces when they are in full operation, seem at first almost at a loss to know what stage of the process they are at, from the want of their accustomed choking miasma.

ON IMPROVEMENTS IN THE MANUFACTURE OF CHLORINE.*

By WILLIAM ODLING, M.B., F.R.S.,
Fullerian Professor of Chemistry, Royal Institution.

Chlorine gas was first obtained in 1774, by the Swedish chemist, Scheele, as a product of the action of muriatic acid—or marine acid, as it was then called—on the black earthy mineral manganese. In accordance with the theoretical notions of the day, Scheele gave to his newly-discovered aërial substance the name dephlogisticated marine acid, which, translated into the language of the new Lavoisierian school, became soon afterwards oxy-muriatic acid. Its assumed constitution and relationship to muriatic acid are indicated below.

Muriatic acid.

Hydrogen — Chlorine.

Hydrogen + Oxygen. — Murium + Oxygen.

Water. — Hypothetic dry muriatic acid.

But Davy, in 1810, showed that muriatic acid gas is a substance wholly free from water, and a definite compound of Scheele's gas with hydrogen. He showed further that Scheele's gas, the so-called oxy-muriatic acid, was not only incapable of furnishing oxygen under any circumstances whatever, but that, so far as experience went, it was altogether undecomposable. Being thus an undecompounded and apparently undecomposable substance, he proposed to regard it as an element, and to designate it, from its yellowish-green colour, by the name chlorine, which it has ever since borne. After considerable opposition, especially on the part of Berzelius, who subsequently, however, became a convert, the view of Davy at length prevailed, and has ever since held its ground, not, indeed, without occasional challenging from time to time. Thus Schönbein, in several of his letters to Faraday, maintained very strongly the superior truthfulness of the original view; and still more recently Brodie has represented the comparable units of hydrogen, H_2, muriatic acid, HCl, and chlorine, Cl_2, by the symbols α, $\alpha\chi$, and $\alpha\chi^2$, implying that the units of muriatic acid and chlorine differ from the unit of hydrogen by a combination of the matter of α with the matter of χ and with the matter of χ^2, respectively. But whatever may hereafter prove to be its real nature, chlorine has hitherto proved undecomposable, and is accordingly regarded by the generality of chemists as an element.

Coincident with the discovery of chlorine itself was the discovery of its most characteristic property, that of bleaching vegetable colours. The petals and leaves of plants, infusions of their several colouring matters, as well as fabrics dyed with the several colouring matters, were found to be rapidly bleached when treated either with chlorine gas or with a solution of the gas in water. From the rapid action of the gas and its solution on pronounced vegetable colours, Berthollet, in 1785, conceived the notion that chlorine in one form or other might be used with advantage commercially for the bleaching of calicoes and linens, theretofore always bleached by a tedious exposure to air and sun-light. The processes devised by Berthollet for carrying out the new method of bleaching were introduced into Scotland in 1787, and soon afterwards became extensively employed. Woven fabrics were bleached by immersion in chlorine water, and paper-pulp more especially by exposure to chlorine gas.

The bleaching action of chlorine takes place only in presence of water or moisture, and is dependent on the property of chlorine under suitable circumstances to decompose water, by uniting with its hydrogen to form muriatic acid, and so setting free its oxygen, by which the bleaching is really effected. Thus, when chlorine, blue

* A Lecture delivered before the Royal Institution of Great Britain January 27th, 1871.

indigo, and water are present together, the opposite tendencies of the chlorine to take hydrogen, and of the indigo to take oxygen from the water, are satisfied by the production of chloride of hydrogen or muriatic acid on the one hand, and of oxide of indigo or isatin on the other, this isatin being a soluble orange-red non-tinctorial body:

$$Cl_2 + H_2O + C_8H_5NO = 2HCl + C_8H_5NO_2.$$

Thus, bleaching by the action of chlorine, like bleaching by exposure to air, is really an act of oxidation.

But the action of chlorine on vegetable fabrics is not confined to that of bleaching their attached colouring matter; the fibre of the fabric is itself liable to be attacked both by the free chlorine and the muriatic acid to which it gives origin. Hence it became important to find a substitute for the free chlorine which should have, like it, the property of liberating nascent oxygen so as to bleach, but should not have, at any rate in the same degree, the property of corrosiveness so as to destroy. Such a substitute is offered in the familiar compound known as bleaching powder, introduced into commerce by the Messrs. Tennant, of Glasgow, in the year 1799. This substance is made by exposing slaked lime to an atmosphere of chlorine gas. The chlorine enters into combination with the lime of the slaked lime, to furnish a product which in practice contains some 35 per cent of its weight of chlorine, and consists of a definite compound of lime with its own weight of chlorine, plus a variable excess of slaked lime unacted upon—

$$CaO.H_2O + Cl_2 = CaOCl_2 + H_2O.$$

Bleaching powder occurs as a dry white pulverulent solid, having a characteristic smell, allied to but not identical with that of weak chlorine. It dissolves considerably in water to form a yellow-tinted liquid known as bleaching liquor, having a marked alkaline reaction from the presence in it of excess of lime. Coloured goods immersed for a longer or shorter time in bleaching liquor of greater or less strength and temperature, gradually have their colouring constituents destroyed or oxidated, the bleaching salt $CaOCl_2$, losing its oxygen, and becoming changed into chloride of calcium, $CaCl_2$. It is observable that the bleaching or liberation of nascent oxygen from bleaching powder is much slower than the bleaching or liberation of nascent oxygen by means of free chlorine; to such an extent, indeed, that for many purposes bleaching powder would be quite inapplicable, were it not for the property it has of liberating chlorine on acidification, whereby a still indirect but yet almost immediate oxidation is effected. Ten grains of bleaching powder, for instance, mixed with a little sulphuric or muriatic acid, will evolve some 4 cubic inches of chlorine gas—

$$CaOCl_2 + H_2SO_4 = H_2O + Cl_2 + Ca''SO_4.$$

The superior activity of the chlorine liberated by acidification of bleaching liquor to the original bleaching liquor is taken advantage of commercially. Brown linens, for example, are first steeped in weak bleaching liquor with little apparent result, and then "soured" by immersion in dilute acid, whereby an immediate more or less complete bleaching is effected. Again, in what is known as the "discharge style" patterns or designs are printed with a mixture of gum and tartaric or other acid, upon a red or blue ground, and the printed fabric passed through a solution of bleach. The red or blue ground withstands the feebler action of the bleach, but the acid printed pattern becomes completely decolourised by the chlorine set free locally, as a result of the action of the acid upon the bleach. It is to this facility of management and capability of being used in different ways that the great utility of bleaching powder and its consequent enormous consumption are due. The amount of bleaching powder made annually in Great Britain and Ireland at the present time is estimated at 75,000 tons, representing at the price of £10 a ton, a value of three-quarters of a million sterling.

Bleaching powder being formed by exposing slaked lime to an atmosphere of chlorine, the manufacture of bleaching powder involves the production of chlorine. Indeed, chlorine is produced at chemical works almost exclusively for the manufacture of bleaching powder, and of one other product, namely, chlorate of potash. This latter is made to the extent of about 750 tons yer annum, having at the price of £105 per ton, a value of nearly £80,000 sterling.

The original reaction upon one another of muriatic acid and peroxide of manganese, by which Scheele was led to the discovery of chlorine, is the basis of the manufacturing process which has ever since been almost exclusively followed. The action consists essentially in the excessive oxygen of the peroxide taking away the hydrogen of the muriatic acid and so setting its chlorine free. Peroxide of manganese, more often called simply manganese, is one of that very limited class of native mineral substances containing an excess of oxygen; the only other similarly peroxidised substance abundant in nature being nitre, in its two forms of East-Indian or potash-nitre, and Chilian or soda-nitre. Now, when muriatic acid instead of being acted on by manganese is acted upon by nitre, or by the nitric acid, formerly known as spirit of nitre, produced therefrom, the excessive oxygen of the nitre takes away hydrogen from the muriatic acid and sets its chlorine free. Thus, by addition of nitric acid or nitre to aqueous muriatic acid, a solution of chlorine is produced, recognisable in various ways, and especially by its power of dissolving gold leaf. Now, although nitre or nitric acid is a much more expensive oxygenant than manganese, it was conceived that nitric acid might nevertheless be substituted with advantage for manganese, from the circumstance that, after it had given up its excessive oxygen so as to liberate the chlorine of the muriatic acid, its deoxidised residue could readily acquire fresh oxygen from the atmosphere, and so reproduce nitric acid, to be used as an oxygenant over and over again indefinitely; and indeed, the nitric method of producing chlorine has, for some time past, been, to a limited extent, in actual use.

With regard to manganese, however, until recently no means have been known of causing its deoxidised residue to re-acquire oxygen from the air, and so become in a condition to serve again for the liberation of chlorine. From time to time, more particularly by Messrs. Tennant and Mr. W. Gossage, attempts have been made in this direction, some of them so far successful as to achieve a limited adoption; and at length a process of reviving the spent manganese residue by effecting its aërial oxidation, has proved a decided commercial success. This process, introduced by Mr. Walter Weldon, has the incidental advantage of doing away with the discharge of the extremely corrosive manganese liquor into streams and watercourses, whereby great damage was frequently occasioned. It consists substantially in neutralising with chalk the acid liquor left by the action of the muriatic acid on the manganese, and then pumping air through the neutralised liquor mixed with excess of lime. The original acid liquor is substantially a solution of chloride of manganese with perchloride of iron, chloride of aluminum, chloride of calcium, and the excess of chloride of hydrogen, or muriatic acid, employed. By neutralisation of the acid liquor with chalk, the iron of the perchloride of iron is precipitated, and a pale pink manganous solution left. By treatment of this solution with milk of lime, an almost white precipitate, consisting of hydrated protoxide of manganese with excess of hydrate of lime, is thrown down. Then by blowing air through the resulting thin sludge, raised to the temperature of 140°—160° F. by injection of steam, oxygen is rapidly absorbed, and the sludge rendered of a deep black colour. By subsidence the now black sludge yields a dense deposit, from which the supernatant clear liquid is run off. Finally, the deposit in its moist condition, and containing in every cubic foot about four pounds weight of peroxide of manganese, is acted upon by muriatic acid in the chlorine stills, and the resulting acid liquid treated over again in the same way, and so on indefinitely.

Already the Weldon process of manganese restoration is employed and about to be employed at many large works, to such an extent, that before very long one-half of the entire quantity of bleaching powder produced in the kingdom may be expected to be made with chlorine generated by means of the restored manganese. It is noteworthy that the original pale precipitate of hydrated protoxide of manganese undergoes oxidation very imperfectly save in presence of a considerable excess of lime, and that the black product finally obtained is not simply peroxide of manganese, MnO_2, but a compound of the peroxide with lime, of composition varying in different instances between that indicated by the respective formulæ $CaO.MnO_2$ and $CaO.2MnO_2$, a small portion of the lime, CaO, being usually replaced by its equivalent of protoxide of manganese, MnO. Some idea of the rapidity of oxidation effected under favourable circumstances may be gathered from the following result: Of the entire quantity of oxygen contained in 175,000 cubic feet of air blown into the sludge within five hours, 14·8 per cent, equal to rather more than 4 cwts., was absorbed in the production of 22 cwts. of manganese peroxide, MnO_2. It is obvious that where Weldon's process is carried out, what happens is this :—The original peroxide of manganese first oxidises a quantity of muriatic acid, and sets free its chlorine. Then the spent manganese is caused to acquire a fresh dose of oxygen from the air, and is used to oxidise a fresh quantity of muriatic acid, and so on continuously. Thus, except during the first use of the manganese, the muriatic acid is really oxidised by the oxygen of the air, the manganese serving only as a carrier. Latterly, Mr. H. Deacon, of the Widnes Alkali Works, has conceived the idea of dispensing with the carrier—that is the manganese —altogether. It has been known for some time that when muriatic acid vapour mixed with air is passed through heated tubes, the oxygen of the air effects a partial oxidation of the muriatic acid, setting free its chlorine ; but the reaction is not sufficiently complete to render the process commercially available. Mr. Deacon, however, has ascertained that when a mixture of air and muriatic acid vapour, heated to about 700° F., is passed over a mass of brickwork that has been steeped in solution of sulphate of copper and afterwards dried, an almost complete decomposition of the muriatic acid is effected. The copper salt certainly takes part in the reaction as an intermediary, but in the end is quite unaffected, the sole obvious result being that the chlorine of the muriatic acid is set free by the oxygen of the air, with a rapidity which seems to leave nothing to be desired. The nature of the intermediary action exerted by the copper salt, so ingeniously made serviceable by Mr. Deacon, has not been clearly ascertained. It would appear, however, that by two simultaneous reactions taking place continuously throughout the process, chloride of copper is ever being alternately formed and decomposed ; it being remembered that chloride of copper gives off a portion of its chlorine with much facility, even by heat alone. The extreme result would, probably, be somewhat as follows :—

$$CuCl_2 + O = CuO + Cl_2$$
$$CuO + 2HCl = CuCl_2 + H_2O.$$

Although this new process has not yet been worked on a manufacturing scale, considerable experience has been acquired with regard to it, and the difficulties to which it at first seemed liable have been successfully provided against. One inevitable difficulty arising from the diluted state of the produced chlorine, has proved to be less serious than was anticipated. This state of dilution is at all times considerable ; for the chlorine being generated by the action of atmospheric oxygen on muriatic acid, every volume of chlorine produced, even in an absolutely perfect working of the process, must be necessarily diluted with twice its volume of atmospheric nitrogen. Nevertheless, according to Mr. Deacon's experience, the diluted state in which the chlorine is always obtained has not been found to interfere with its application to the manufacture either of chlorate of potash or bleaching powder. The requisite plant for carrying out the invention on a large scale is now being erected at Messrs. Gaskell and Deacon's works.

For experimental purposes the new process of continuous chlorine production may be performed with oxygen instead of air. It then manifests a striking reverse relationship to a process of continuous oxygen production, in which oxide of cobalt acts as a permanent intermediary.

In the one experiment the gas goes in oxygen and comes out chlorine, muriatic acid being decomposed and water formed. In the other experiment the gas goes in chlorine and comes out oxygen, water being decomposed and muriatic acid formed. In the presence of slaked lime to arrest the muriatic acid, this last reaction takes place readily and completely below the boiling-point of water.

NOTICES OF BOOKS.

Chemisches Apothekerbuch : Theorie und Praxis der in Pharmaceutischen Laboratorien Vorkommenden Pharmaceutisch, Technische und Analytisch-Chemischen Arbeiten. Von Adolf Duflos, Docteur der Philosophie und Medicin, Königlichem Geheimen Regierungs-Rathe und Professor. Fünfte Bearbeitung. Nebst Hülfstabellen für die praxis in pharmaceutischen laboratorien und vergleichen der uebersicht der nomenclatur der arzneilich angewandten chemischen präparate der Pharmacopœa Germaniæ, oder Pharmacopöen von Preussen, der Schweiz, England, Frankreich, und Russland, wie der von Hanover, Hessen, und Schleswig-Holstein. Mit 180 in den text gedruckten abbildungen nach original zeichungen und einem spectralbilde nach Bunsen und Kirchhoff. Ferdinand Hirt, Verlags und Königliche Universitäts Buchandlung, Breslau. 1867.

Chemical Apothecaries' Book: Theory and Practice of the Pharmaceutical, Technical, and Analytical Operations and Work to be performed in Pharmaceutical Laboratories. By Adolf Duflos, M.D., Ph.D., Privy Councillor of His Imperial and Royal Majesty of Germany and Prussia, and Professor at Breslau University. Fifth edition, thoroughly re-written and revised ; to which are added tabular forms for assisting in the practice of the pharmaceutical laboratory, and a comparative review of the nomenclature in use for the chemical preparations which are officially prescribed by the Pharmacopœæ of Germany, Prussia, Switzerland, England (Ph.B.), France, Russia, Hanover, Hesse, and Schleswig-Holstein. Illustrated with 180 woodcuts, made from drawings expressly prepared for this purpose, of apparatus in actual operation, and also coloured spectra according to Bunsen and Kirchhoff. Ferdinand Hirt, Breslau. 1867.

It is gratifying to us to have the opportunity of once more bringing under the notice of our readers a work of so illustrious and no less modest a *savant* as Dr. Duflos. We say modest, because it should be understood that under the title above quoted the author has produced a compendium of chemistry, theoretical, practical, applied, and analytical, such as no other exists in any language. The work is professedly written only as a guide-book for pharmaceutists, but we can readily show our readers that this book is really a *vade mecum* for all classes of chemists, the author's practical experience of the laboratory extending over fully half a century.

The first ninety pages of the work are devoted to physical chemistry, including a very excellent and readable chapter on crystallography, copiously illustrated by woodcuts, and containing, for every crystalline system, an enumeration of the common minerals and a large number of chemical preparations which belong to it. The theory of chemistry, equivalents, atoms, allotropy, isomery, &c., is very lucidly set forth, and is followed by a short chapter on the origin and decomposition of compound bodies, fermentation, ferments, decay, contagion. The

next large division of the book is headed "*A-metals*" (*a privans*); under this division are treated oxygen, sulphur, selenium, tellurium, the haloids, phosphorus, boron, silicon, and lastly carbon. It is a noteworthy feature of this work that under carbon are treated, not simply carbonic acid and the other compounds of carbon usually supposed to belong to inorganic chemistry, but there are also introduced those organic compounds which contain carbon and hydrogen, as well as those which, in addition to these elements, contain oxygen; so that the chapter on carbon, some 260 pages, contains a great portion of the organic part of chemistry. Under nitrogen, again, after having treated of the compounds that element forms with oxygen, those of nitrogen and hydrogen are treated of, and immediately after the chapter on the ammonia bases follows the chapter on the vegetable alkaloids, after which the compounds of nitrogen and carbon are treated of, ending with a chapter on uric acid. The large section then following treats on the metals, inclusive of the more rare ones, and not omitting any preparation of even slight importance, while, of course, those used pharmaceutically or industrially are very fully described. It is a prominent feature that minute and careful directions are given for making all the preparations. The chapter on Chemical Analysis treats, in a succinct, but clear manner, on this subject, inclusive of blowpipe testing. There is next added to this work, first, a series of no less than twenty-six tables for various purposes. To quote the full titles of all would occupy far too much of our space, but they commence with a table exhibiting the elasticity and vapour density of aqueous vapour at temperatures from $-20°$ to $+30°$, and end with a tabulated form of the boiling-point of aqueous saline solutions according to the varying quantity of salts therein contained. We next meet with a comparative tabular review of the nomenclature of the chemical preparations officinally used and found in the Pharmacopœæ of Germany, Prussia, Switzerland, England, France, and Russia; and, after that, two similar tabulated reviews bearing upon the French and German Pharmacopœæ, and also the comparison between the sixth and seventh editions of the *Pharmacopœa Borussica* and the Pharmacopœæ of Hanover, Hesse, and Schleswig-Holstein. The last pages of this work are devoted to two indices, one containing the German, the other the Latin names. There is met with at the beginning of the book an excellent descriptive index of all the woodcuts, and thirty-two pages of *addenda et corrigenda*, all proving that the author has spared no pains or trouble to produce, what this work really is, a magnificent bequest to his many pupils.

A Concise View of the Law connected with Letters Patent for Inventions. By JAMES JOHNSON, Barrister-at-Law, and J. HENRY JOHNSON, C.E. London: Longmans, Green, and Co.

THIS little work, condensed from the "Patentees' Manual," by the same authors, certainly fulfils its title. All the leading principles and salient points of Patent Law are explained in as clear and simple a manner as the subject is capable of, while the details of the various Acts of Parliament are left to more pretentious works. The book contains a useful summary of the patent laws of foreign countries and the British Colonies.

CORRESPONDENCE.

ON THE SPECTRUM OF THE AURORA.

To the Editor of the Chemical News.

SIR,—In the last number of the CHEMICAL NEWS some remarks are made by Professor W. A. Norton, on the presence of iron in the spectrum of the aurora. In connection with this subject I may mention that, in a paper in the CHEMICAL NEWS, July 12th, 1867, after alluding to the earth acting by virtue of its magnetic attraction, and drawing to its surface any substances of a highly magnetic character, such as iron, which might approach within the sphere of its influence, I observed that "this would occur mostly in the vicinity of the north and south poles. A large quantity of finely divided meteoric dust descending in this manner into the earth's atmosphere, and becoming ignited therein, might contribute to the phenomena of aurora borealis and australis."—I am, &c.,

JOHN A. R. NEWLANDS, F.C.S.

13, Knowle Road, Brixton,
April 29, 1871.

INTESTINAL CALCULI.

To the Editor of the Chemical News.

SIR,—In your last issue Mr. Stark describes and figures a calculus, the weight of which was a little more than 2 lbs. Whenever he is in London I shall be happy to show him one of these concretions weighing 9½ lbs., and having a circumference of 22 inches. It, also, is from a horse, and is almost entirely composed of phosphate of magnesium and ammonium. Fragments of hair and vegetable fibre have apparently assisted the crystals of the salt in forming a compact hard mass; the motion of the animal, in walking, probably still further solidifying and rounding the ball. A section of the stone shows it to be built of a series of concentric strata, varying in colour from dirty white to dark brown, the tints depending on the different amounts of organic matter in the several layers of mineral deposit. The nucleus on which the calculus has been formed, in this instance, is a small piece of flint, and the section has been so made as to exhibit the pebble *in situ*.

These calculi are not unfrequently found in the intestinal canal of herbivorous animals. At the Pharmaceutical Meeting at which my specimen was shown, some eight or nine years ago, Dr. Edwards remarked that in the Liverpool Museum there were two such stones, weighing respectively 11 lbs. and 13 lbs.—I am, &c.,

JOHN ATTFIELD.

17, Bloomsbury Square, W.C.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Annalen der Chemie und Pharmacie, March, 1871.

This number contains the following original papers and memoirs:—

Products obtained by the Distillation of a Mixture of Butyrate and Acetate of Lime.—Dr. F. Grimm.—This very lengthy essay treats on—Propyl-methyl-keton—

$$\left\{\begin{matrix} C_3H_7 \\ CO \\ CH_3 \end{matrix}\right.$$

iso-amyl-alcohol; pinakon, $C_{10}H_{22}O_2$; ethyl-methyl-keton—

$$\left\{\begin{matrix} C_2H_5 \\ CO \\ CH_3 \end{matrix}\right.$$

dimethyl-keton and dipropyl-keton.

Fatty Acids contained in the Fusel Oil from Hungarian Wine, and on some Derivatives of Caprinic Acid.—Dr. F.

Grimm.—The subjects treated of in this exhaustive essay are—Caprinate of methyl—

$$\left.\begin{matrix}C_{10}H_{19}O\\CH_3\end{matrix}\right\}O;$$

caprinate of amyl—

$$\left.\begin{matrix}C_{10}H_{19}O\\C_5H_{11}\end{matrix}\right\}O;$$

caprinon—

$$\left\{\begin{matrix}C_9H_{19}\\CO\\C_9H_{19}\end{matrix}\right.$$

caprinyl chloride—

$$\left.\begin{matrix}C_{10}H_{19}O\\Cl\end{matrix}\right\}$$

caprinic acid anhydride.

Synthesis of the Oil of Rue.—Dr. E. von Gorup-Besanez and Dr. F. Grimm.—After referring to the researches of Drs. Geisecke, Fittig, and Strecker, bearing upon the constitution of the essential oil of rue (*Ruta graveolens*), as consisting of a mixture of ketons—viz., nonyl-methyl-keton, or caprinyl-methyluret, the authors describe, at great length, the experiments made by them for producing, synthetically, the oil alluded to by treating the fractional distillation products from a mixture of chemically pure caprinate and acetate of lime, so as to obtain caprinyl-methyluret, which agreed, in all properties, with that found in natural oil of rue.

Contribution to our Knowledge of Cholic Acid.—Dr. E. von Gorup-Besanez.—This paper is divided into the following sections:—Action of phosphoric chloride upon cholic acid, $C_{24}H_{40}O_5$; action of fusing caustic potassa upon cholic acid.

Advantageous Mode of Preparing Glycoholic Acid.—Dr. E. von Gorup-Besanez.—Fresh ox-gall is evaporated to dryness on a water-bath, exhausted with alcohol at 90 per cent, the alcohol removed by distillation, the residue treated with milk of lime, next gently heated, filtered, the filtrate treated with dilute sulphuric acid, avoiding excess. The glycoholic acid is deposited after a few hours' rest, and purified, first by filtration, pressing between folds of blotting paper, and re-dissolution in lime-water and re-precipitation by means of sulphuric acid.

Extraordinarily Large Quantity of Clay contained in a Human Lung.—Dr. E. von Gorup-Besanez.—This paper gives an account of a pathologico-chemical fact. The lungs of a person who had during lifetime worked in a factory, and been there exposed to the inhalation of mineral dust, were found to contain, in 1000 parts, no less than 19·91 parts of mixed sand, clay, and silica, giving, for the entire weight of the lungs, 1·5 kilos. 29·86 grms.

Caution.—Dr. E. von Gorup-Besanez.—This short paper gives an account of the effects of the explosion of only 10 drops of nitroglycerine, which, by one of the pupils of the author, in his laboratory, were put into a small cast-iron saucepan, and heated with a Bunsen gas-flame. The effect of the explosion was that the forty-six panes of glass of the windows of the laboratory were smashed to atoms, the saucepan was hurled through a brick wall, the stout iron stand on which the vessel had been placed was partly split, partly spirally twisted, and the tube of the Bunsen burner was split and flattened outwards. Fortunately, none of the three persons present in the laboratory at the time were hurt. The author takes the opportunity of referring to a communication Dr. E. Kopp, bearing upon the conditions under which nitroglycerine explodes or burns off quietly. When nitroglycerine is caused to fall drop by drop on a thoroughly red-hot iron plate, it burns off as gunpowder would do under the same conditions; but if the iron is not red-hot, but yet hot enough to cause the nitroglycerine to boil suddenly, an explosion takes place (*Comptes Rendus*, vol. lxiii., p. 189).

Zinc-Calcium Salt of the Ethylen-Lactic Acid as a means to obtain that Acid in Pure State.—Dr. W. Heintz.—This essay and the two following, by the same author, do not admit of any useful abstraction, notwithstanding their intrinsic merits.

Products of the Decomposition of Ethyliden-Chlorpropionic Acid and Ethylen-Iodpropionic Acid when Boiled with Milk of Lime.

Nature of the Lactic Acid of Meat—Muscular Tissue.

Action of Salts on Alcohol.—Dr. K. Kraut.—This paper contains the following sections:—Action of acetate of zinc upon alcohol; action of valerianate of zinc upon alcohol; action of formiate of zinc upon alcohol; action of the acetates of ammonia, soda, magnesia, protoxide of mercury, and oxide of silver upon alcohol.

Naphthyl-Purpuric Acid and the Derivatives thereof.—Dr. E. von Sommaruga.—This monograph, filled with a great number of lengthy and complex formulæ, is divided into the following chapters:—Salts of naphthyl-purpuric acid; products of the decomposition of naphthyl-purpuric acid; oxidation by means of nitric acid; action of fusing caustic alkali; indophan.

Description of an Apparatus suited to Exhibit the Liquefaction of Dry Ammoniacal Gas in Lecture-Rooms.—Dr. F. C. G. Müller.—Illustrated with a woodcut.

Synthesis of Coniine.—Dr. H. Schiff.—This exhaustive monograph does not admit of any useful abstraction, since it would require the reproduction of a series of very lengthy and complex formulæ.

The Titanic Acid obtained by Fusion with Phosphor-Salt in the Blowpipe-Flame is not Anatase.—Dr. A. Knop.—This paper mainly contains the description of the results of some blowpipe experiments made with the view to prove that the substance first obtained by Professor G. Rose while fusing titanic acid with phosphor salt, is not anatase (octahedrite of Dana's "System of Mineralogy," 5th edition, p. 161), but is simply phosphorate of titanic acid, $3TiO_3+PO_5$. In 100 parts—$3TiO_2$, 63·4; PO_5, 36·6.

New Solvent for Indigotine.—Dr. A. A. de Aguiar and Dr. A Bayer.—The solvent here alluded to is aniline, as obtained by the destructive distillation of indigo, and applied by the authors to obtain from commercial indigo the indigotine in pure state.

Bromated Derivatives of Acetic Acid Anhydride.—Dr. H. Gal.

Polytechnisches Journal von Dingler, first number for March, 1871.

This number contains the following original papers relating to chemistry and collateral sciences:—

Caloric Studies on the Cupola Furnaces Used in Iron Foundries.—Dr. C. F. Dürre.—This lengthy algebraico-physical monograph is, notwithstanding its great practical value, not well suited for any useful abstraction.

Preparation for Obtaining Decorative Colours on Metals.—Dr. Puscher.—The author employs for this purpose a solution of hyposulphite of lead in hyposulphite of soda, the latter in the proportion of 3 parts, by weight, of dry salt to 1 part of the acetate of lead, the quantity of water used for making the solution being about sixteen times the weight of the first-named salt. The clear solution of the salts deposits, when heated to about 100°, a thin layer of sulphuret of lead upon any metal—brass, zinc, copper—placed in the solution, and thereby produces a beautiful display of various colours on these objects.

Analysis of a so-called Native Meerschaum from New South Wales.—M. J. Stingl.—Sp. gr., 1·899. 100 parts of this mineral contain—Silica, 89·46; peroxide of iron, 0·45; alumina, 1·43; magnesia, 0·85; soda, 0·29; water driven off at 100°, 3·95; water driven off at red heat, 3·18; carbonaceous matter, 0·36. Since the silica is contained in this substance in such a state as to be nearly all soluble in caustic potassa solution of 1·35 sp. gr., this mineral might be applied as a raw material for making a soluble silicate; but the author observes that the boiling of this mineral with a concentrated solution of caustic soda only dissolved 19·2 per cent of the silica present. The term meerschaum is quite a misnomer for this mineral.

Action of Phosphorus upon Aniline.—M. A. Stiassny.—When phosphorus and aniline are heated with water, aniline red is formed, the aniline being oxidised by the decomposition of the water in the presence of phosphorus. When aniline, boiling at 185°, was heated with phosphorus, only a cherry-red colouration ensued; but if the same reaction were tried with an aniline boiling at a higher temperature, a brownish red colour was produced.

Conversion of Yellow Phosphorus into the Red Amorphous Modification thereof.—M. A. Stiassny.—The experiment here described is mainly the combustion of phosphorus under a bell-jar, when at the same time a small vessel is placed under the jar containing a solution of bichromate of potassa. At first, the combustion of the phosphorus proceeds as usual; but, when the quantity of air present has become too small to admit of the formation of phosphoric acid, phosphorous acid is generated, and this, reacting upon the chromate of potassa, reduces it partly to chromic oxide, while the phosphorous acid is thereby converted into phosphoric acid. This reaction is accompanied by the evolution of green-coloured vapours, while the non-consumed yellow phosphorus is converted into the red-coloured allotropic modification thereof.

Second number for March, 1871.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

New Method for Testing Malleable Metals and Alloys, and Description of the Apparatus to be Applied for that purpose.—Dr. G. Bischof.—This lengthy and valuable essay could only be well understood by the reproduction of a series of engravings illustrating the apparatus. The method is based on mechanical principles.

Manufacture of Bessemer Steel at Séraing Societé Anonyme de John Cockerill, près Liège Belgique, and on the Quality of the Steel as Applied in Various Ways.—Dr. C. F. Dürre.—A valuable contribution to the literature on Bessemer steel, but far too lengthy to admit of further notice here.

Analysis of a Sample of East Indian Tinkal (Pounxa).—Dr. H. Vohl.—This mineral was formerly the chief source of borax in Europe, when tinkal was rather largely imported from India. 100 parts of the mineral contain—Boracic acid, 36·8881; soda, 16·4771; silica, 0·0545; sulphuric acid, 0·2201; chlorine, 0·1681; peroxide of iron, 0·0068; alumina, 0·0013; protoxide of manganese, a trace; lime, 0·2415; magnesia, 0·1917; potassa, 0·0132; carbonic acid, a trace; water, 44·6451; sand and insoluble matter, 0·7775;—total, 99·6850. The mineral contains about 54 per cent of anhydrous borax. The author did not find any fatty or soap-like matter, nor any organic substance, in this specimen.

Bayerisches Industrie und Gewerbe Blatt, February, 1871.

This number contains the following original matter bearing upon chemistry and allied sciences:—

Varnish to Protect Polished Metals from Rusting.—Dr. C. Puscher.—The author recommends the use of a solution of paraffin (1 part, by weight, in 3 parts of petroleum) as a varnish which may be usefully applied to polished metals, especially as, after having brushed this liquid over the surface of the metals, they may be gently wiped clean with a soft piece of flannel, so as to leave only a very thin film of the varnish, yet sufficient for the protection of the polish.

Mixture for Producing so-called Pharaoh's Serpents which are not attended with Injurious Fumes.—Dr. C. Puscher.—Mix intimately together 2 parts of bichromate of potassa, 1 part of nitrate of potassa, and 3 parts of white sugar. This mixture should be pressed in paper or tinfoil cones, and, if intended to be kept for any length of time, the paper should be varnished over with sandarac varnish. A small quantity of balsam of Peru may be added to perfume the mixture, so as to cause its combustion to be attended with a pleasant odour. The greenish coloured very porous mass, which assumes the serpent shape, is a mixture of carbonate of potassa, oxide of chromium, carbon, and a small quantity of neutral chromate of potassa. The author states that this mass is an excellent substance for polishing all kinds of metals, and may be, moreover, usefully employed as an absorbent for moisture, instead of chloride of calcium, since this mass, without becoming pasty, absorbs, in twenty-four hours, about 20 per cent of moisture. The mixture above specified should be kept in a dark place.

Coating Brass with Copper.—Dr. C. Puscher.—Dissolve 10 parts, by weight, of sulphate of copper, and 5 of sal-ammoniac, in 150 parts, by weight, of water. Place the brass, well cleaned and free from fatty matter on its surface, into this mixture, leave it in it for a minute, let the excess of liquid drain off first, and heat the metal next over a charcoal fire, until the evolution of ammoniacal vapours ceases and the coppery film appears perfect. Wash with cold water, and dry. The coating of copper adheres firmly.

Journal für Praktische Chemie, No. 4, 1871.

This number contains the following original papers and memoirs:—

Periodides of the Alkaloids.—Dr. S. M. Jörgensen.—This paper is the second instalment of a very exhaustive monograph on this subject, and contains the following sections:—Methyl-chinin-triiodide, $C_{20}H_{24}N_2O_2,CH_3,I_3$; ethyl-chinin-triiodide, $C_{20}H_{24}N_2O_2,C_2H_5,I_3$; cinchonin-triiodide, $C_{20}H_{24}N_2O,H,I_3,H_2O$; methyl-cinchonin-triiodide, $C_{20}H_{24}N_2O,CH_3,I_3$; ethyl-cinchonin-triodide, $C_{20}H_{24}N_2O,C_2H_5,I_3$; methyl-chinidin-triiodide, $C_{20}H_{24}N_2O_2,CH_3,I_3$; methyl-cinchonidin-triiodide; strychnin-triiodide, $C_{21}H_{22}N_2O_2HI_3$; methyl-strychnin-triiodide, $C_{21}H_{22}N_2O_2,CH_3,I_3$; ethyl-strychnin-triiodide; brom-ethyl-strychnin-triiodide, $C_{21}H_{22}N_2O_2,C_2H_4Br,I_3$; amyl-strychnin-triiodide; amyl-strychnin-pentaiodide; brucin-triiodide; brucin-diiodide; methyl-brucin-triiodide; methyl-brucin-pentaiodide; ethyl-brucin-triiodide; ethyl-brucin-pentaiodide; amyl-brucin compounds: amyl-brucin-triiodide; amyl-brucin-hexaiodide; allyl-brucin compounds; allyl-brucin-triiodide; allyl-brucin-pentaiodide; igasurine.

Behaviour of the Lithia-Containing Minerals with the Spectroscope; and on the Detection of Thallium in the Sphalerite found at Geroldseck, in Breisgau (a Province of the Grand Duchy of Baden).—Dr. von Kôbell.—This paper gives an account of the testing of various minerals containing lithia with the spectroscope. The author puts the pulverised minerals on a piece of platinum foil formed like a channel, perforated with small holes, and held by means of a pair of platinum forceps. The author observes that, unless the mineral to be tested for lithia is previously well moistened with hydrochloric acid, the detection of lithia may fail, since only the spectrum of the flame tinged by chloride of lithium exhibits the characteristic line. Several minerals were also tested for thallium, and more especially some zinc ores (sphalerite is a kind of zinc blende). Most of these minerals gave a negative result; but the sphalerite from Geroldseck, and a similar mineral from Herbesthal (a small Prussian village, close to the Belgian frontier), gave very evident indications of the presence of thallium.

Some New Derivatives from Albumen.—M. O. Loew.—This paper treats on some substances obtained from albumen by treating it (with avoidance of heating) with a mixture of fuming nitric and concentrated sulphuric acids. One of the bodies thereby obtained is a yellow-coloured powder, insoluble in water, alcohol, and dilute acids, slightly bitter tasted, soluble in dilute alkalies, and precipitable therefrom without alteration. This body is hexanitro-albumen-sulpho acid—

$$C_{72}\left\{\begin{matrix}H_{101}\\(NO_2)6\\SO_2OH\end{matrix}\right\}N_{18}SO_{22}.$$

The other substances described at length are—Hexamido-albumen-sulpho acid—

$$C_{72}\left\{\begin{matrix}H_{101}\\(NH_2)6\\SO_2OH\end{matrix}\right\}N_{18}SO_{22};$$

albumen-sulpho acid—

$$C_{72}\left\{\begin{matrix}H_{107}\\SO_2OH\end{matrix}\right\}N_{18}SO_{22}$$

Contribution to our Knowledge of the Geminated (Gepaarten) Compounds of the Pentatomic Nitrogen.—M. C. W. Blomstrand.—The first instalment of a lengthy essay on this subject, which, although valuable in a scientific point of view, is not well suited for abstraction.

Declaration by Professor J. von Liebig.—This eminent *savant* states that several articles have been sold and manufactured, among others by M. J. P. Liebe, apothecary at Dresden, under the title of Liebig's unfermented extract of malt, Liebig's extract of malt with iron, iodine, quinine, iodide of iron, &c.; Liebig's condensed milk; and Liebe-Liebig's food for infants. Professor von Liebig desires it to be known that he never gave his consent to the use of his name in any of these articles, nor does he know anything about their real value, his name having been made use of without his consent and entirely against his will. The only dietetic preparation to which the Professor's name is attached with his full knowledge is the extract of meat prepared at Fray-Bentos, and the strict condition has been imposed upon the Company that no extract of meat shall be delivered to the trade unless it has been previously tested and approved of by Dr. M. von Pettenkofer.

NOTES AND QUERIES.

Cellulose.—Would any of your readers kindly inform me of any reliable process for determining the amount of woody fibre contained in hay, straw, esparto grass, &c.?—Papyrus.

Cryolite.—Can any of your chemical correspondents kindly inform me, through the medium of the Chemical News, who are manufacturers of aluminate of soda made from cryolite.—Alumina.

Analysis of Iron Ores.—I should be glad to know of a good method of estimating organic matter in iron ores. Also whether the iron in the residue, insoluble in HCl, is always in the form of persalt.—L. Peters.

Phlogiston.—A correspondent sends us the following short report of a lecture at the Royal Institution, April 28th, 1871.

> The gist of a lecture you here may now see as
> A point that no soul will insist on:—
> "We can square ancient thoughts with modern ideas,
> If we 'Energy' read for 'Phlogiston!'"

Manipulating India-Rubber.—Will any of your readers kindly inform me where I can find any simple process (suitable for amateurs—*boys*), by which india-rubber can be softened sufficiently to be pressed into a mould, and retain the shape but recover its elasticity. Also, if there is any way of colouring, either india-rubber in its ordinary state, or the white solution of it used by manufacturers. The colour wanted is blue, or red, or flesh-colour.—F. Bankes.

Dr. Dingler's Compressed Leather.—Having tried Dr. Dingler's method of rendering leather cuttings plastic, and failed in so doing, I would kindly ask that gentleman, through the medium of the Chemical News, if he will be kind enough to give the full and exact manipulation—how to accomplish the method of rendering leather cuttings soft and plastic, as recommended in your journal of January last; by so doing he will confer a favour to many of your readers as well as myself.—A Leather Worker.

Aluminium for Batteries.—I have lately seen a proposal to use aluminium foil instead of platinum in nitric acid batteries. I am, however, inclined to think that the change would be disadvantageous, and should be glad if you could find room for the following account of an experiment I made to test its utility. I took a small piece of foil and put it into some acid which had been used for some time in the battery with a solution of salt in the outer cell. In a few hours it was eaten through. Hence I conclude that the aluminium would not do when salt is used, and I should suppose that it would also be necessary to use nitric acid free from hydrochloric acid, which would add considerably to the expense of working.—T. H. Waller.

MEETINGS FOR THE WEEK.

Monday, 8th.—Royal Geographical, 8.
—— Royal Institution, 2. General Monthly Meeting.
Tuesday, 9th.—Ethnological, 8.
—— Civil Engineers, 8.
—— Photographic, 8.
—— Royal Institution, 3. Charles Brooke, M.A., F.R.S., "On Force and Energy."
Wednesday, 10th.—Society of Arts, 8.
—— Geological, 8.
Thursday, 11th.—Royal, 8.30.
—— Royal Society Club, 6.
—— Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
Friday, 12th.—Royal Institution, 9. Col. Jervois, R.E., C.B., "On the Defence of the United Kingdom."
—— Quekett Microscopical Club, 8.
—— Astronomical, 8.
Saturday, 13th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."
—— Quekett Microscopical Club. Excursion to Chislehurst; to meet at Charing Cross Station at 2 p.m.

TO CORRESPONDENTS.

A. N. P.—Names of works on the subject have often appeared in our Correspondence column.

R. Warington.—The communication arrived too late for insertion in our last issue.

W. R.—Consult our advertising columns.

A. E. Davies, Ph.D.—Received with thanks.

J. P.—Our advertisement columns would be the proper place for your communication.

W. Rowley.—Reimann's "Handbook of Aniline." Our publisher will send it to you post free on receipt of 10s. 10d.

J. Jones.—The journal has been received, for which accept our thanks.

Just published, 8vo., price 2s. 6d.,

ANALYTICAL TABLES

FOR

STUDENTS OF PRACTICAL CHEMISTRY.

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London: J. and A. Churchill, New Burlington-street.
Liverpool: Adam Holden, 48, Church Street.

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Select Methods in Chemical Analysis, chiefly Inorganic. By William Crookes, F.R.S., &c., Editor of the Chemical News and of the *Quarterly Journal of Science*.

London: Longmans, Green, and Co., Paternoster Row.

Just published, price One shilling,

A Concise View of the Law connected with Letters Patent for Inventions. By James Johnson, of the Middle Temple, Barrister-at-Law, and J. Henry Johnson, Assoc. Inst. C.E., Solicitor and Patent Agent, Lincoln's Inn Fields and Glasgow. Authors of the "Patentees' Manual."

London: Longmans, Green and Co., Paternoster Row.

Royal 8vo., pp. 388, price 7s. 6d. Third Edition.

The Patentee's Manual; being a Treatise on the Law and Practice of Letters Patent, especially intended for the use of Patentees and Inventors. By James Johnson, Barrister-at-Law; and J Henry Johnson, Assoc. Inst. C.E., Solicitor and Patent Agent.

The call for a Third Edition of this work is conclusive proof that it satisfies a want on the part of Patentees and Inventors, to whom a plain statement of the law bearing upon the subject of Letters Patent for Inventions is obviously a matter of great importance. Whilst the exposition of statutes and judicial decisions is expressed in plain and popular language, no sacrifice has been made of legal accuracy, and it will be found that the work contains a concise but ample and strictly correct enunciation of the law, with an examination of the decided cases to the latest date.

London: Longmans, Green, and Co.

Just published, demy 8vo., 6s., by post 6s. 6d. (vol. i.),

Transactions of the Chemical Society of Newcastle-upon-Tyne.

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The Session 1870—1871 will commence on the 3rd of October, when the Laboratories will be re-opened at 10 a.m. for Instruction in Practical Chemistry as applied to Pharmacy, Medicine, Analysis, &c. Pupils can enter at any period. Terms moderate.

THE CHEMICAL and TOXICOLOGICAL CLASS will meet as usual every Monday and Thursday evening, at 8 p.m., commencing October 3rd.

The LATIN CLASS for the reading of Physicians' Prescriptions, Cæsar's Commentaries, &c., every Tuesday and Friday evening, at 8 p.m.

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THE CHEMICAL NEWS.

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RESEARCH ON A NEW GROUP OF COLLOID BODIES, CONTAINING MERCURY, AND CERTAIN MEMBERS OF THE SERIES OF FATTY KETONES.*

By J EMERSON REYNOLDS, M.R.C.P. Ed.,
Vice-President R.G.S.J., Keeper of the Mineral Department, and Analyst to the Royal Dublin Society.

(1). *Colloid Body Obtained by Uniting Acetone with Mercuric Oxide.*—When solution of mercuric chloride is slowly added to a mixture of acetone with dilute aqueous solution of potassium hydrate, the murcuric oxide first precipitated is dissolved, with the production of a clear colourless liquid. The addition of the mercurial solution can be continued until a white precipitate makes its appearance, the alkali being still in excess. If the solution be filtered at this point, an apparently opalescent, yellowish-coloured liquid is obtained. If one portion of this alkaline solution be boiled for a few minutes, a thick gelatinous mass suddenly separates, and further ebullition is rendered difficult, if not impossible. Another portion of the liquid, when treated with an acid in slight excess, gelatinises, and if the original solution be moderately strong, the vessel in which the experiment is made may be inverted without risk of spilling its contents. Finally, if some of the mercuric solution be exposed over sulphuric acid *in vacuo*, it leaves on partial evaporation a gelatinous mass, on the surface of which latter crystals of potassium chloride soon make their appearance. When the desiccation is complete, a yellowish resinoid body is obtained, together with a large quantity of very beautiful acicular crystals of the chloride, and a certain amount of potassic carbonate.

Preliminary experiments similar to the foregoing were sufficient to indicate that the chief compound produced in the reaction above referred to might be regarded as a colloid body. I therefore took advantage of the late Professor Graham's beautiful dialytic method for effecting its purification from crystalloids, and have met with complete success.

Preparation of Colloid Liquid.—40 grms. of pure mercuric chloride are to be dissolved in about 500 c.c. of hot water and the solution then allowed to cool, even though crystals of the salt separate. 29 grms. of potassium hydrate are next dissolved in about 300 c.c. of water; 15—20 c.c. of acetone should now be placed in a capacious glass balloon, and diluted with 250 c.c. of water. The reaction is then to be managed as follows:—About 150 c.c. of the alkaline solution should be added to the aqueous acetone, and then 250 c.c. of the mercuric chloride gradually poured in. Re-solution of the mercuric oxide first thrown down proceeds slowly at the outset, if the mixture be not warmed. After a time the oxide quickly re-dissolves, if the contents of the balloon are briskly agitated. When the first half of the mercuric solution has been added, the remaining 150 c.c. of potassium hydrate are to be cautiously poured in and the residual mercuric chloride then mixed with the precautions already stated.

The solution prepared in the manner described is usually turbid, but can be easily filtered clear, from the small amount of mechanically suspended matter. The filtrate should next be placed on a large hoop dialyser, covered as usual with carefully prepared parchment paper, and the vessel floated on a considerable volume of distilled water. After two days' action the diffusate will be found to contain a large quantity of potassium chloride, some potassium hydrate, and but a very small amount of mercury. The process of diffusion is to be continued, the diffusate being replaced by pure water twice each day, until the liquid on which the dialyser floats no longer affords a cloud when treated with a solution of silver nitrate. The process may then be considered terminated, and the pure colloidal liquid obtained. The contents of the dialyser can be now removed, and should be free from all odour of acetone. A few drops, when evaporated to dryness on platinum foil and the residue ignited, should volatilise completely.

Analyses of the Anhydrous Compound.—I took a considerable volume of the very carefully prepared colloid liquid and divided it in two parts. One portion was very cautiously evaporated to dryness, and the resinoid residue very finely powdered and carefully dried. The second portion was precipitated by the addition of dilute acetic acid, and the gelatinous precipitate washed rapidly and completely with the aid of Bunsen's filter-pump, and the residue dried. The desiccation in each case was effected at first *in vacuo* over sulphuric acid, and finally on the water-bath. The compound easily bears a temperature of 100° C.

On analysing both products, I found that they were practically identical in composition. As the presence of the volatile metal introduced some difficulty in the determinations, it is necessary to describe the plan of analysis adopted.

The mercury was, in some cases, obtained in the metallic state by distillation with quicklime; but I much prefer to digest a weighed quantity of the compound in a sealed tube with dilute hydrochloric acid, and, after complete solution has taken place, to break the tube, precipitate the metal from the contents by sulphuretted hydrogen, and weigh the mercury in the usual way as sulphide. The results are very satisfactory.

The presence of a large proportion of mercury in the compound rendered the accurate determination of carbon and hydrogen somewhat difficult. At the outset of this investigation I arranged a very troublesome process of analysis, similar to that employed by Messrs. Frankland and Duppa in their analysis of mercuric ethide and its analogues. In all later combustions I have adopted the very simple and satisfactory plan of placing in the anterior part of the combustion-tube a layer of 15 centims. of any metal capable of easily amalgamating with mercury. Gold and silver foil answer the purpose well: no doubt tin foil might be used in the same way. It is scarcely necessary to add that the temperature of the anterior part of the combustion-tube containing the gold or silver was in each instance very carefully regulated.

Results were obtained which showed the composition of the body to be well represented by the formula—

$$[(CH_3)_2CO]_2Hg_3O_3.$$

Properties of the Colloid Body, Hydrated and Anhydrous.—Analogy would lead us to conclude that the collodial liquid obtained by dialysis is a hydrate of the body represented by the above formula; but since evaporation *in vacuo* is sufficient to remove the water completely, the hydrate can possess but little stability. Properly speaking, this hydrate is no doubt a true liquid, and as such is miscible with other liquids.

The reaction of this hydrate is neutral to test-papers.

When the aceto-mercuric hydrate contains 5 per cent of the anhydrous compound, it will, if quite pure, remain liquid for twelve or fourteen days, toward the end of this time becoming gradually less fluid, until the whole "sets" to a firm jelly. The same result may be bronght about in a few seconds by the addition to the perfectly neutral liquid of very minute quantities of any of the following substances:—Hydrochloric, acetic, nitric, sulphuric (incompletely), chromic, oxalic, tartaric, or citric acids; by potassium, sodium, ammonium, barium, and calcium hydrates; by calcium chloride, mercuric chloride, sodium acetate, and other neutral salts. Contact with certain in-

* Abstract of a paper read before the Royal Society, April 20th, 1871.

soluble powders, such as calcium carbonate, and even alumina, induces pectisation.

Elevation of temperature quickly determines the gelatination of the liquid. If containing 5 per cent of the ketone compound, a very firm jelly is produced on heating to 50° C. In one experiment, a quantity of the liquid was taken and some bright, carefully cleaned copper gauze introduced The liquid did not pectise, nor did any trace of mercury deposit on the copper, after standing for a day. The temperature of the whole was then raised to 50° C.; a transparent jelly was at once produced, of such strength that the vessel in which it was contained could be inverted without any risk of loss, This jelly, enclosing the bright copper gauze, has remained in my possession for eight months without giving the slightest indications of a disposition to change.

Small zoological specimens, when enclosed in the same way in a jelly of the mercuric ketone compound, were found to keep well when carefully cleansed before they were sealed up in the gelatinous envelope.

By evaporation, a liquid containing 8 per cent of the ketone compound was obtained, but it pectised in a few hours. A 2 per cent hydrate retained its liquidity for several months. When once a jelly has formed in any of these liquids, I have not succeeded completely in reconverting it to the liquid state by very cautious treatment with potassium hydrate, even when aided by diffusion.

When the colloid hydrate was treated with sulphuretted hydrogen, mercuric sulphide was produced. The liquid filtered from the sulphide yielded *acetone* on distillation. Digestion with dilute hydrochloric acid likewise effected the decomposition of the colloid body, mercuric chloride being produced and acetone liberated. Nitric and sulphuric acids, when dilute, did not decompose the compound with the same facility as hydrochloric acid. Treatment of the hydrosol with copper, zinc, or iron *at ordinary temperatures*, failed to effect the substitution of either metal for the mercury in the compound. Prolonged contact with each of the two last-mentioned metals caused pectisation, the metal subsequently becoming encrusted with a white substance. Heat produced the same result more rapidly.

When the anhydrous compound was cautiously heated, a quantity of acetone distilled over, and, as the temperature rose, empyreumatic products and mercury were evolved. When the heat was suddenly applied, much metallic mercury distilled over, and a minute quantity of a liquid, having the odour of mercuric methide, was produced, together with carbonic anhydride and other gaseous bodies not particularly examined.

Detection of Acetone in the "Methylated Spirit" of Commerce.—Since acetone is, according to my experience, invariably present in the wood-spirit of commerce, the reaction with mercuric oxide in presence of potassium hydrate, already described, becomes virtually a test for the presence of pyroxylic spirit in any mixture. The ordinary "methylated spirit" of commerce is such a mixture, and the acetone present in it can be detected with great facility in the following way:—Take 200 c.c. of the spirit and rapidly distil off 50 c.c.; dilute the distillate with an equal volume of water, and slightly warm with addition of a few c.c. of solution of potassium hydrate. On cautious addition of mercuric chloride, the oxide first thrown down is speedily re-dissolved: excess of the mercuric salt must be carefully avoided. The alkaline liquid should be filtered clear, much of the alcohol allowed to evaporate slowly, and the residue then divided in two portions. One part is to be violently boiled for a few minutes; a yellowish-white gelatinous precipitate will suddenly make its appearance, if the acetone compound be present. In the second portion, dilute acetic acid, when added in slight excess, should produce a bulky, white, gelatinous precipitate containing, when washed and completely dried, between 78 and 79 per cent of mercury. Finally, by means of the above test the admixture of "methylated spirit" with alcoholic solutions may be detected with facility. In all such cases, however, a specimen of a pure alcoholic solution should be tested at the same time as the suspected sample, distillation of considerable quantities of the liquids being resorted to in every instance.

(2). *Bodies containing Mercury and Higher Members of the Fatty Ketone Series.*—Experience has proved that several ketones of the fatty series are capable of uniting with mercuric oxide in the presence of alkali to form compounds analogous to that obtained with acetone. The higher ketones being insoluble in water though soluble in alcohol, it was found to be extremely difficult to prepare the colloidal hydrates or hydrosols of the mercuric compound.

In the case of butyrone, however, I have succeeded completely in obtaining a hydrosol.

The general reactions of the several solutions leave no doubt that the compound contained in each belongs to the group of colloid bodies and not to that of crystalloids.

The following results have been obtained:—

(*a*). *With Propione.*—The rectified products of the careful destructive distillation of barium propionate was dissolved in alcohol and mercuric chloride added; excess of strong aqueous solution of potassium hydrate was then poured in; on gently warming the mixture the mercuric oxide dissolved in great part. The liquid, filtered as clear as possible, was found to be rich in mercury, and when treated with excess of dilute acetic acid, gave a white gelatinous precipitate precisely analogous to that obtained under similar circumstances from the potassium di-aceto-mercurate.

The precipitate was found on analysis to contain 72·26 per cent of mercury; the formula requires 73·17 per cent. On treatment with hydrochloric acid, the precipitate afforded mercuric chloride and an oily liquid insoluble in water, possessing the odour of and characters attributed to propione.

(*b*). *With Butyrone.*—The rectified product of the destructive distillation of calcium butyrate, when dissolved in alcohol and treated with mercuric chloride and potassium hydrate, gave, on warming, a liquid similar to that obtained with propione. Like the latter, it afforded a white precipitate on treatment with excess of dilute acetic acid. The precipitate, when carefully washed and dried, contained 69·1 and 69·27 per cent of mercury; the formula $[(C_3H_7)_2CO]Hg_3O_3$ requiring 68·49 per cent. The freshly prepared precipitate, when digested with excess of hydrochloric acid, afforded mercuric chloride and a small quantity of a liquid insoluble in water and recognisable as butyrone.

The alkaline alcoholic solution of the butyrone mercuric compound was diluted with its own volume of water, then filtered and carefully warmed in order to slowly evaporate the alcohol as much as possible. When a considerable portion of the latter had been removed in this way, the clear residual liquid was placed on a dialyser, and the attempt made to diffuse away the crystalloids from the liquid. After treatment for five days, most of the chloride had diffused away, and a liquid was obtained which pectised on heating to near the boiling-point, and on the addition of a small qnantity of a mineral acid. The precipitate first formed by hydrochloric acid was easily redissolved on heating with excess of the reagent and the odour of the ketone developed. The attempt to convert the alcosol of the mercuric compound into the hydrosol had therefore been attended with success.

(*c*). *With Valerone.*—Obtained by purifying the product of the distillation of calcium valerate previously mixed with one-sixth of its weight of quicklime. When this was dissolved in alcohol and the solution was rendered alkaline by potassium hydrate and mercuric chloride dropped in, the mercuric oxide first precipitated was speedily re-dissolved, but from the solution no precipitate was thrown down on addition of an acid, previously mixed with alcohol. I have not obtained any compound from this alkaline liquid possessing distinctive properties. It

is not improbable that the valerone mercuric compound, if formed, is more easily decomposed by acids than the corresponding bodies obtained with other ketones; hence the difficulty of isolating it.

I may now be allowed to summarise the results of the investigation detailed in the foregoing pages. It has been shown :—

1. That certain fatty ketones can be made to unite directly with mercuric oxide.

2. That the resulting compounds afford a new group of colloid hydrates, analogous in properties to the silicic, aluminic, and other hydrates already made known by the researches of Professor Graham.

3. That the new hydrates may best be regarded as extremely feeble conjugate acids; the chief member of the group (that derivable from acetone) being probably tetrabasic and capable of affording very unstable salts.

4. That the generating reaction for these bodies may, when carefully controlled, be employed as a test for the presence of a ketone, especially for acetone in certain organic mixtures.

5. That the generating reaction for the di-keto-mercurates may, when carefully controlled, be employed as a test for the presence of a ketone—especially for acetone—in certain organic mixtures.

ON THE

INCREASE OF ELECTRICAL RESISTANCE

IN

CONDUCTORS WITH RISE OF TEMPERATURE,

AND ITS

APPLICATION TO THE MEASURE OF ORDINARY AND FURNACE TEMPERATURES;

ALSO ON A SIMPLE METHOD OF MEASURING ELECTRICAL RESISTANCES.*

By C. W. SIEMENS, F.R.S., D.C.L.

THE first part of this paper treats of the question of the ratio of increase of resistance in metallic conductors with increase of temperature.

The investigations of Arndtson, Dr. Werner Siemens, and Dr. Matthiessen, are limited to the range of temperatures between the freezing- and boiling-points of water, and do not comprise platinum, which is the most valuable metal for constructing pyrometric instruments.

Several series of observations are given on different metals, including platinum, copper, and iron, ranging from the freezing-point to 350° C.; another set of experiments being also given, extending the observations to 1000° C. These results are planned on a diagram, showing a ratio of increase which does not agree either with the former assumption of a uniform progression, or with Dr. Matthiessen's formula, except between the narrow limits of his actual observations, but which conforms itself to a parabolic ratio, modified by two other coefficients, representing linear expansion and an ultimate minimum resistance.

In assuming a dynamical law, according to which the electrical resistance of a conductor increases according to the velocity with which the atoms are moved by heat, a parabolic ratio of increase of resistance with increase of temperature follows; and in adding to this the coefficients just mentioned, the resistance r for any temperature is expressed by the general formulæ, $r = \alpha T^{\frac{1}{2}} + \beta T + \gamma$, which is found to agree very closely both with the experimental data at low temperatures, supplied by Dr. Matthiessen, and with the author's experimental results, ranging up to 1000° C. He admits, however, that further researches will be necessary to prove the applicability of the law of increase expressed by this formula to conductors generally.

In the second part of this paper it is shown that, in taking advantage of the circumstance that the electrical resistance of a metallic conductor increases with an increase of temperature, an instrument may be devised for measuring with great accuracy the temperature at distant or inaccessible places, including the interior of furnaces, where metallurgical or other smelting operations are carried on.

In measuring temperatures not exceeding 100° C. the instrument is so arranged that two similar coils are connected by a light cable containing three insulated wires. One of these coils, "the thermometer-coil," being carefully protected against moisture, may be lowered into the sea, or buried in the ground, or fixed at any elevated or inaccessible place whose temperature has to be recorded from time to time; while the other, or "comparison-coil," is plunged into a test-bath, whose temperature is raised or lowered by the addition of hot or cold water, or of refrigerated solutions, until an electrical balance is established between the resistances of the two coils, as indicated by a galvanoscope, or by a differential voltameter, described in the second paper, which balance implies an identity of temperature at the two coils. The temperature of the test-solution is thereupon measured by means of a delicate mercury thermometer, which at the same time tells the temperature at the distant place.

By another arrangement the comparison-coil is dispensed with, and the resistance of the thermometer-coil, which is a known quantity at zero temperature, is measured by a differential voltameter, which forms the subject of the second paper; and the temperature corresponding to the indications of the instrument is found in a table prepared for this purpose, in order to save all calculation.

In measuring furnace temperatures the platinum-wire constituting the pyrometer is wound upon a small cylinder of porcelain contained in a closed tube of iron or platinum, which is exposed to the heat to be measured. If the heat does not exceed a full red heat, or, say, 1000° C., the protected wire may be left permanently in the stove or furnace, whose temperature has to be recorded from time to time; but in measuring temperatures exceeding 1000° C., the tube is only exposed during a measured interval of, say, three minutes, to the heat, which time suffices for the thin protecting casing and the wire immediately exposed to its heated sides, to acquire within a determinable limit the temperature to be measured, but is not sufficient to soften the porcelain cylinder upon which the wire is wound. In this way temperatures exceeding the welding-point of iron, and approaching the melting-point of platinum, can be measured by the same instrument by which slight variations at ordinary temperatures are told. A thermometric scale is thus obtained embracing without a break the entire range.

The leading wires between the thermometric coil and measuring instrument, which may be, under certain circumstances, several miles in length, would exercise a considerable disturbing influence if this were not eliminated by means of the third leading wire before mentioned, which is common to both branches of the measuring instrument.

Another source of error in the electrical pyrometer would arise through the porcelain cylinder upon which the wire is wound becoming conductive at very elevated temperatures; but it is shown that the error arising through this source is not of serious import.

The third part of the paper is descriptive of an instrument for measuring electrical resistance without the aid of a magnetic needle or of resistance scales. It consists of two voltameter tubes fixed upon graduated scales, which are so connected that the current of a battery is divided between them, with one branch including a known and permanent resistance, and the unknown resistance to be measured. The resistance and polarisation being equal, and the battery being common to both circuits, these unstable elements are eliminated by balancing them from the circulation, and an expression is found for the un-

* Abstract of a paper read before the Royal Society, April 25th, 1871.

known resistance X in the known resistances C and γ of the voltameter, including the connecting wires and the volumes V and V′ of gases evolved in an arbitrary space of time within the tubes, viz. :—

$$X = \frac{V}{V'}(C+\gamma) - \gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Changes of atmospheric pressure affect both sides equally, and do not therefore influence the results; but a reading of the atmospheric pressure is obtained at both sides by lowering the little supply reservoir with dilute acid to the level indicated in the corresponding tube. The upper ends of the voltameter tubes are closed by small weighted levers provided with cushions of india-rubber. But after each observation these levers are raised, and the supply reservoirs moved so as to cause the escape of the gases until the liquid within the tubes is again brought up to the zero line of the scale, when the instrument is ready for another observation. A series of measurements are given of resistances varying from 1 to 10,000 units, showing that the results agree within 1 per cent with the independent measurements obtained of the same resistances by the Wheatstone method.

The advantages claimed for the proposed instrument are, that it is not influenced by magnetic disturbances, or the ship's motion if used at sea; that it can be used by persons not familiar with electrical testing; and that it is extremely simple and easily procured.

ON THE

CAUSES OF HIGH AND LOW ESTIMATIONS OF SOLUBLE PHOSPHATES.

By ARTHUR E. DAVIES, Ph.D., F.C.S., &c.

In answer to Mr. Little's appeal in the last number of the CHEMICAL NEWS, will you permit me to make the few following observations?—That gentleman, after referring to the wide difference in the percentage of soluble phosphate returned by different analysts from the same sample, and the importance of this difference in a commercial point, says: "When we ask for some explanation, you will observe the reply is either nothing, or that there is some difference in the process of analysis." In order to give a fuller and more complete explanation than the above, I have hastily put together the following observations, for which I trust you will be able to allow me space in the next number of your journal:—

The material known in commerce as superphosphate is prepared by certain chemical processes from bones, either raw or reduced to ash by burning, coprolites, apatite, &c. These materials all agree in containing a certain proportion of phosphoric acid in combination with lime in the form of tricalcic phosphate, or, as it is more commonly called, bone-earth or insoluble phosphate of lime. The object of the treatment to which these materials are submitted is not to increase the amount of fertilising matter which they contain, but, by changing it into a more soluble form, to render it more easily and rapidly available for the purposes of the plant. This change is effected by acting upon the bone or other phosphatic material with oil of vitriol. When this is brought in contact with the tricalcic or insoluble phosphate, the latter yields up to the former two of its three equivalents of lime, sulphate of lime or gypsum being produced, whilst the place of the lime is supplied by two equivalents of water, the product being the monocalcic or soluble phosphate—

$$3CaO.PO_5 + 2HOSO_3 = 2CaOSO_3 + 2HO.CaO.PO_5.$$

It is the great difference in the results obtained by different chemists in estimating these soluble phosphates which has given rise to the distinction of high and low analysts. The source of these differences lies, not in any manipulative error on the part of the analyst, but in imperfections in the process by which the analyses are made. These processes are either those by which the phosphate is estimated by direct precipitation, as tricalcic phosphate, or those by which the phosphates are decomposed, and their phosphoric acid estimated separately. When the first of these plans is followed a suitable quantity of the sample, accurately weighed, is ground in a small mortar with a little cold water, the undissolved matter allowed to settle, and the clear, or almost clear, supernatant liquid poured on a filter. This operation is repeated three or four times, by which the whole of the monocalcic or soluble phosphate should be dissolved and pass into the filtrate. The undissolved residue is then transferred to a beaker, and boiled with water two or three times, the liquid, after each boiling, being poured through the filter. The object of this operation is to extract sufficient sulphate of lime to re-convert the monocalcic into tricalcic phosphate. The insoluble residue is dissolved in hydrochloric acid, the sand being of course removed by filtration. We thus obtain two solutions, the water containing the soluble phosphate, and the acid the insoluble. It is with the former of these alone that we have at present to deal. If sufficient sulphate of lime has been dissolved to completely re-convert the monocalcic into tricalcic phosphate, these are precipitated from the water solution by simply adding a slight excess of ammonia. The monocalcic phosphate then takes back two equivalents of lime, and is re-converted into tricalcic phosphate, in which state it falls as an insoluble precipitate. If however, sufficient lime has not been dissolved, as sometimes happens, then chloride of calcium must be added in addition to ammonia, and some chemists always add the salt without first ascertaining whether or not the solution already contains a sufficient quantity of lime. It is to errors introduced at this part of the process that the inaccurate results of the so-called high analysts are due. It has just been shown that, for the complete precipitation of the soluble phosphate, by ammonia, a certain quantity of lime is required; and as it is impossible to obtain exactly the required quantity, we may consider that it is absolutely necessary that the liquid should contain an excess of lime. Should the ammonia employed as a precipitant contain—as it frequently does—carbonic acid, more or less lime will be thrown down as carbonate, and introduced into the results of the analysis as phosphate. This, however, is not the most common cause of error, and it is one which is entirely avoided if the ammonia is free from carbonate. When the solution contains an appreciable excess of lime, the spongy precipitate of phosphate thrown down by ammonia will retain a certain quantity of lime mechanically, which it is quite impossible to remove by any practical amount of washing. It may, however, be got rid of by re-dissolving the well-washed precipitate in hydrochloric acid, and re-precipitating with ammonia, when the phosphate of lime will be thrown down in a pure state. Unfortunately, however, the practice of chemists with regard to re-dissolving, differs. Some (the low analysts) always re-dissolve, and their results are, consequently accurate. Others (the high analysts) never do so, and they necessarily weigh and calculate as phosphate a quantity of lime existing in a different and valueless form. The only truly accurate and reliable processes are those in which the phosphoric acid is estimated apart from the lime. For these the solutions are prepared in the way already described; but, instead of estimating the phosphates by direct precipitation, they are decomposed, and the phosphoric acid separated and estimated by one of the well-known processes. In my own laboratory the plan adopted is to precipitate with an excess of ammonia, re-dissolve the precipitated phosphate in acetic acid, and separate the lime as oxalate. In the filtrate from the oxalate of lime the phosphoric acid is estimated as phosphate of magnesia. In this manner the amount of phosphoric acid may be determined with almost absolute accuracy, and from it the amount of soluble phosphate is readily calculated. Were some such process adopted by all chemists

we should soon cease to hear of the great differences in results now so frequently obtained from the same sample by two chemists of equal and acknowledged competence.

ARSENIC IN PYRITES AND VARIOUS PRODUCTS.*

By H. A. SMITH.

The difference existing between the amount of arsenic in pyrites in published analyses, and that found in practical working, led the author to make the following series of experiments, the results of which are arranged in three tables.

Table I., Part I., gives the analysis of various specimens of pyrites; these are extracted from Richardson and Watts's "Technology." In Part II., the author's analyses are given of a few specimens of the same species of ore.

Table II. gives the amount of arsenic in a certain kind of pyrites, in the sulphuric acid manufactured from it, and in the various products in the manufacture of which the sulphuric acid had been used; also the amount of arsenic in the chamber and flue deposits.

Table III. is calculated from Table II., giving the amount in a manner more useful for practical purposes.

Table I.

Pyrites.	Arsenic per cent.	Pyrites.		Number of analyses made.	Arsenic per cent.
Spanish	0·21 to 0·31	Spanish	Mason's.	10	1·7453
			Tharsis.	10	1·6517
Belgian	Trace.	Belgian.		8	0·9437
Westphalian ..	Trace.	Westphalian.		8	1·8783
Norwegian ..	None.	Norwegian	Hard.	8	1·6490
			Soft.	8	1·7085

Table II.

Substance in which arsenic is found.	Number of Analyses.	Arsenic per cent.
Pyrites before burning	8	1·649
,, after ,, ...	4	0·465
Sulphuric acid	15	1·051
Deposit in flue leading from pyrites kiln to lead chamber	4	46·360
Deposit on bottom of lead chamber ..	5	1·857
Hydrochloric acid	8	0·691
Sulphate of soda	15	0·029
Soda waste	6	0·442
Carbonate of soda	12	—
Recovered sulphur (Mond's process) before purification began	4	0·700
Mond's process, latest methods	2	0·000

The pyrites used for analysis in Table II. was the hard Norwegian, and this, as may be seen in Table I., comes next in order in freedom from arsenic to the Belgian. The Belgian itself was put aside, owing to the fact that it made too much "smalls" in breaking for the kilns, which became an expense as well as inconvenience.

Table III.

	Tons As.
100 tons of hard Norwegian pyrites (Table I.) contain, before burning	1·649
Ditto, after burning	0·465
100 tons of hard Norwegian pyrites make 140·875 tons of H_2SO_4, containing	1·481
140·875 tons of sulphuric acid make 104·9 tons of HCl, containing	0·724
140·875 tons of sulphuric acid make 204·12 tons of Na_2SO_4, containing	—

This Table gives the amounts of arsenic on a practical scale, so that the total impurity may be seen at once.

We see, by this Table, how the arsenic persistently adheres to the products in the manufacture of which acid made from pyrites has been used, and how it becomes distributed through the acid and soda in various stages.

The arsenic also has an effect on the nitric acid supplied to the lead chamber, being converted from arsenious to arsenic acid, and thereby causing a slight loss of the nitric acid gas. In the deposit in the chamber (mentioned in Table II.) were found crystals of arsenic acid, as a proof of this result.

ON THE ELECTROMOTIVE AND ELECTROLYTIC PHENOMENA DEVELOPED BY GOLD AND PLATINA IN SOLUTION OF THE ALKALINE SULPHIDES AND SULPHURETTED HYDROGEN.*

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

The author shows that electric currents of considerable intensity are developed by either gold or platinum under the following circumstances:—

Two plates of gold (prepared chemically pure) are taken; one is placed in a cell charged with sea-water, and the other in a porous cell charged with sulphide of ammonium, which cell is partially immersed in the former. These plates are connected voltaically; a common galvanometer inserted in the circuit will then have its needle vigorously deflected—and the direction of this indicates the electric current is from the gold in the porous cell to that in the outer cell.

Electric currents of equal strength were also developed by charging both cells with sea-water, or solution of potash or ammonia, and administering sulphuretted hydrogen to either cell.

By charging one of the cells with potash and ammonia, and the other with sea-water, a current of electricity was also produced, but of far feebler intensity than those above cited, and which might be owing to traces of sulphur in these alkaline solutions.

The currents obtained by either of these metals in alkaline sulphides had sufficient intensity to decompose certain metallic solutions—for instance, those of copper, gold, and silver—the metal being deposited in a compact and regular form.

To avoid any errors which might be caused by films of air, &c., adhering to the metallic plates, and so getting contact with the solutions, these plates were always, prior to their immersion in these solutions, raised to a red heat, and while in this state plunged into water or the saline solutions about to be used, which liquids themselves had been previously boiled to expel the gases dissolved in them.

The author further found that both gold and platinum are positive to iron in solutions of alkaline sulphides, and he gives a list of various metals, in the order of their negativeness relatively to each other, in such solutions, of which the following is a copy (the order is from negative to positive):—

Carbon.	Mercury.
Iron.	Lead.
Gold.	Tin.
Platinum.	Copper.
Silver.	Zinc.

To obtain these results, it is necessary to avoid the presence of caustic alkalies in these solutions. The capability of gold and platinum to generate electric

* Read before the Manchester Literary and Philosophical Society, April 4th, 1871.

* Abstract of a paper read before the Wellington Philosophical Society, January 29th, 1871.

currents under these circumstances—currents of such intensity as to decompose the metallic solutions cited—the author takes to be conclusive of the correctness of his opinion that the absorption of sulphur by these metals (as described in a former paper of his) is a true chemical act.

In conclusion, the author alludes to the announcement, by Professor Becquerel, in a series of papers, of a certain electromotive power of gold and platinum in pure water, also in neutral and saline solutions, abstracts of which announcements, as given in certain numbers of the CHEMICAL NEWS, were laid upon the table for reference of members, and from which the author considers his own researches are quite distinct, in method and results, to to those entered upon by Professor Becquerel, and for the following reasons :—(1) Professor Becquerel describes these electric phenomena as being due to the action of liquids on gases already absorbed by gold and platinum, whereas his results are obviously due to the action of these sulphides upon the metal itself. Further, these electric currents, as produced by Professor Becquerel, were "very feeble," while those described by the author of this paper have such an intensity as to be able to perform the functions of an electrotyping cell, it being, therefore, impossible that "capillary affinity plays a very important part in their production," as is asserted by Professor Becquerel in the case of the electric currents he describes.

DETERMINATION OF ALKALIES IN MINERALS, &c., BY IGNITION WITH CARBONATE OF LIME AND SAL-AMMONIAC.*

By J. LAWRENCE SMITH.

1. IN this description of separating and determining the alkalies, it is my intention to give the minutest details, although it may be thought useless by some; but numerous analyses have given me the experience here detailed; and I am convinced that analytical chemists, if they follow them out, will never resort to any other known method after a few trials; and if there be a better method, it is yet to be discovered. The presence of boracic, hydrofluoric, and phosphoric acids in the minerals in no way interferes with the process. Even in silicates *soluble in acids*, I prefer this method, in common with other analysts, for its ease and accuracy.

2. During the latter part of 1852 I made the researches, the details of which were published early in 1853.† Since that time I have employed the process many hundreds of times with the greatest satisfaction, most accurate results, and ready manipulation; some minor points were not completed satisfactorily until several years after the first notice of the method; these have been subsequently perfected, and in my mind there is nothing further now to be desired. I might state that many analytical chemists have for years constantly used this process to the exclusion of all others.

3. The purpose of this article is to give all the perfections, and a minute detail of the manipulations, with whatever precautions are necessary, all of which are simple and easily executed. In the two articles on the subject of alkali determination in minerals, published in 1853, the whole subject was reviewed, which is needless to recur to now. I then reviewed the process by caustic baryta and its salts, by hydrofluoric acid, also detailing some experiments on the separation of the different alkalies from each other, microscopic examinations of the same, &c. It was shown that, after the caustic alkalies, the most powerful agent to attack silicates at a high temperature is caustic lime, a fact not new to chemists, for as early as 1847 I used this process for attacking silicates, and others had done the same prior to that period. But for the purpose of arriving conveniently at *a quantitative determination of the alkalies in silicates*, certain methods of manipulation, qnantity of material, admixture, &c., had to be discovered, and in them resides the success of my process—converting the most difficult parts of the analysis of a silicate into the easiest.

4. The methods of analysis of the silicates by caustic baryta and its carbonate are well known; but for various reasons fully detailed by Rose, in his "Analytical Chemistry," are now no longer used. The method still employed extensively is the one proposed by Berzelius with hydrofluoric acids, and when applied with numerous precautions, it seems to decompose all silicates; still, according to Rose, there are siliceous compounds that cannot be completely decomposed by hydrofluoric acid.* Dismissing all criticism, I at once proceed to the method which is the subject of this article.

5. *Decomposition of Silicates by Ignition with Carbonate of Lime and Sal-Ammoniac.*—This mixture of carbonate of lime and sal-ammoniac is used simply for the purpose of bringing, in a most thorough manner, caustic lime to act upon the silicates at a red heat.† The first step in the process is to have pure *carbonate of lime.*

6. This is made in my laboratory as follows :—Take as good marble as can be conveniently found (not dolomitic), or calc spar, and dissolve in hydrochloric acid. (It is not necessary that the acid be perfectly pure). Add an excess of the marble and warm the solution; to it add lime-water or some milk of lime, made from pure or nearly pure lime, until the solution is alkaline to test-paper; the lime is added to precipitate any magnesia, phosphate of lime, &c., that may have been in the marble. Filter this solution, and, after heating it to at least 160° F., precipitate with carbonate of ammonia.‡ The carbonate of lime thus precipitated is to be thrown on a filter and well washed with pure rain-water or with distilled water.

7. Thus prepared, the carbonate of lime is a dense powder and perfectly pure; or, if it contain any impurity, it will be a trace of carbonate of baryta or strontia, which in no way interferes with the use of the carbonate of lime.

8. *Sal-Ammoniac.*—To give to this reagent the most convenient form, take fragments of clean sublimed sal-ammoniac, dissolve them in water over a gentle fire, filter, evaporate the filtered solution over a steam-bath, or on a sand-bath, or any other convenient gentle heat, and as the small crystals deposit themselves, stir the solution to keep the crystals small; when one-half or two-thirds of the sal-ammoniac is deposited, pour off the liquid without waiting for it to cool, throw on a cotton filter, and dry the crystals at the temperature of the atmosphere. In this way sal-ammoniac is furnished that is easily pulverised.

9. *Vessels for Producing the Decomposition.*—The ordinary platinum crucible can be employed for this purpose, and is now most commonly used, and for many years was used by myself. It is, however, found that, while this method exceeds in precision and ease all other known methods for alkali determinations in silicates, there was yet a very minute quantity of alkalies lost by volatilisation, and while the method gave satisfaction to those who

* From "Select Methods in Chemical Analysis," by W. Crookes, F.R.S., &c. London: Longmans.

† Shortly after my first publication, M. St. Claire Deville made known his method of analysing the silicates by *fusion* with lime; but the nature of his process, and the objects to be arrived at, were quite different from those attained by the process under consideration.

* The process used by Deville in fusing with lime is in most cases better than that by hydrofluoric acid, and one that I should use in preference to all others, except the one now being described.

† Chloride of calcium at a red heat will dissolve more or less caustic lime.

‡ This precaution must not be overlooked, as it is desirable to obtain the precipitated carbonate of lime as dense as possible. If the carbonate of ammonia be added to the cold solution, the precipitate, at first gelatinous, will ultimately become much more dense and settle readily; the same is true if the mixture be heated after the addition of the carbonate of ammonia; but in neither case will it be as dense as when the carbonate is added to the hot solution of chloride of calcium. The reaction in this process of analysis is in no way effected by the form of the carbonate of lime; but, by using the denser form, the mixture occupies less space in the crucible in which it is heated.

used it, I yet continued my researches to overcome even this small loss. This has been successfully accomplished, and for some time I have been in the constant habit of using an improved form. Instead of the ordinary platinum crucible, I use an elongated one, which may be made of various dimensions. The one employed, with from ½ to 1 grm. of silicate, is of the following form and dimensions.

10. An elongated slightly conical crucible with rounded bottom, and having a cover with or without a central wire to hold the cover; its length is 95 m.m.; diameter of opening, 22 m.m.; diameter of small end, just at the turn of the bottom, 16 m.m.; and weighs about 35 or 40 grms. It can be made lighter, but experience has shown that it is better if stiff and solid. Messrs. Johnson and Matthey, Hatton Garden, London, have made a number for me, and they have my drawings and directions. It is thus made for the purpose of having that portion of the crucible containing the mixture heated strongly, while the upper portion is below a red heat.

11. *Manner of Heating the Crucible.*—If the ordinary crucible be used, it is heated in the manner usually employed for the fusion of silicates. If the new form of crucible be employed, then the upper part may be grasped by a convenient metallic clamp in a slightly inclined position, and a moderate blast from the table blowpipe made to play upon it for about twenty-five or thirty minutes. But now gas is to be found in every well-mounted laboratory, Bunsen burners of all dimensions are used, and, when properly applied, can be made to give gradations of heat, from the mildest to that sufficient to melt cast-iron. A simple, cheap, and convenient furnace, with a properly arranged draft, can be made to accomplish all silicate fusions without the aid of any manual labour, and therefore I employ such an appparatus, of which a description will be given at the end of this article.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

May 4th, 1871.

Dr. Warren De La Rue, F.R.S., Vice-President, in the Chair.

The following gentlemen were elected Fellows:—R. S. Best, C. S. Cross, W. H. Darling, G. H. Ogston, J. Schweitzer, W. A. Smith.

Dr. Voelcker delivered a lecture "*On the Productive Powers of Soils in Relation to the Loss of Plant Food by Drainage.*"

The lecturer began by showing the futility of the belief that a soil-analysis could reveal whether a land was productive or not. To those who only imperfectly know the teachings of modern agricultural science it appears very simple to remedy a deficient soil by finding out, through analysis, the wanting constituent, and then to supply them. But this is not so. Not only is it difficult exactly to analyse a soil, but many other conditions besides the composition of the land have to be observed. Experience has furnished examples that two soils may be equal as to the respective amounts of potash, soda, lime, or phosphoric acid they contain, and yet be very different in their productiveness. The mentioned substances may be in different states of combination. Then, much depends on the physical properties of a soil. Land may contain all the important plant foods and still be yielding but poor crops, if it possesses not sufficient depth. A further point of consideration is the character of the undersoil and its relation in the situation to the surface soil. Lastly, climatological circumstances have to be considered. These two latter conditions are not revealed by soil analysis. Of course it is not to be denied that there are cases where analysis may ascertain and remedy the deficiencies of a soil. It is well known that in spite of good physical conditions of a soil, and of its possessing useful constituents, as potash, phosphoric acid, &c., it yet may be barren, simply because some injurious substance,—as, for instance, iron sulphate—is present in it. Here an analysis of the soil shows whence the mischief arises and suggests the remedy. And so it is with regard to substances which have a beneficial effect upon a land. Yet, in both these cases, the amounts may be so small as not to permit their exact determination by analysis. The addition of 3 cwts. of good superphosphate of lime to 1 acre of soil, 6 inches deep, increases the turnip crop in a favourable season from 6 to 10 tons per acre. Now these 3 cwts. to 1 acre hardly add 1-60,000th per cent of phosphoric acid to the land, and this small portion can, by analysis, certainly not be estimated with accuracy. More than anything this fact shows the futility of attempting to determine the productiveness of a soil by a mere analysis. It also obliges to oppose the views of those who advocate that in a system of rational farming there should be kept up a debtor and creditor account as regards the constituents which are removed from the soil in the crop grown upon it and the quantity of fertilising matter restored to it in the shape of manure.

The fertility of the soil cannot be maintained, much less increased, if only as much fertilising constituents would be applied to the land as one removes from it in the crops. Dr. Voelcker thinks that three to five times as much fertilising matter have to be put upon the land than is taken out from it in the crops.

Dr. Voelcker then discussed the relative values of various mineral salts as manures, quoting in support of his views the results of the classical field experiments of Lawes and Gilbert, and this, then, led the lecturer to speak of the examination of land drainage waters. Lawes and Gilbert, throughout a long series of experiments on the growth of wheat, have experienced a great loss of nitrogen, no matter whether ammonia salts or nitrates or nitrogenous organic matters were applied to the crop. Similar loss was found when barley or meadow hay was grown. The amount of nitrogen supplied in the manures was greater than that recovered in the increased produce.

It appeared to Dr. Voelcker that the nitrogen lost might have passed into the drain, and, at his request, Messrs. Lawes and Gilbert directed holes to be dug in the experimental wheat-fields at the end of each drain, passing right through the middle of each experimental plot. An opportunity was then afforded to collect the drain-waters, which then had been analysed by Dr. Voelcker. Of course it was known with what manures each plot had been treated.

The waters were collected—

1866.	Dec. 6.	At a full flow of the drain.	
1867.	May 21.	,,	,,
1868.	Jan. 13.	,,	,,
1868.	April 24,	,,	,,
1869.	Dec. 29.	At an enormous flow of the drain.	

A large table hung in the room contained the detailed figures of the analyses. It showed the relation of nitrogen as nitrates and nitrites found in the drain waters to the various manures employed. There would have to be considered the loss of rain-water by evaporation, but on this point as yet no reliable data exist.

But it is clear, from all the observations that, in whatever form the nitrogen is applied to the soil, a large proportion of it is carried off chiefly in the form of nitrates. Nitrate of soda, especially, seems to be rapidly removed by the rain. It should, therefore, be applied to the soil late in spring, the best time in average season being, probably, the middle of March. Farm-yard manure, on the other hand, experience has shown is best applied in autumn.

At all times of the year, but especially during the active period of growth of the crops, nitrates are found in the watery liquid which circulates in the land, whereas ammonia salts are never met with in any appreciably large quantities. It may therefore be assumed that it is chiefly, if not solely, from the nitrates that the crops build up their nitrogenous organic constituents.

Dr. Voelcker's analyses of drainage waters further showed that potash and phosphoric acid, which certainly are the most important mineral constituents for the plant, are almost entirely retained in the soil, whilst the less important, as lime or magnesia, or sulphuric acid, pass with greater readiness out of the land.

The interesting lecture concluded by pointing to the importance of attaining at an accurate knowledge of the capacity of different soils to work up, so to speak, the crude fertilising matters into suitable food for the plants.

The Chairman, in proposing a vote of thanks to the lecturer, expressed how highly the Society appreciated the paper just read. Referring to the observation that the soil cannot retain some salts, especially nitrates, he remarked how important it was to put the various manures at the proper season upon the land. Alluding to the field experiments of Messrs. Lawes and Gilbert, he said that the facts brought to light in those investigations will form the most valuable material for theoretical speculations for a long time to come. He finally expressed his regret at the necessitated absence of Drs. Frankland and Odling.

Dr. Gilbert fully agreed with Dr. Voelcker as to the inutility of soil analyses. He, too, regretted Dr. Frankland's absence, as their views on the subject of the lecture differed somewhat. Dr. Frankland spoke of possible mixtures of the waters from plot to plot; but such had not been the case. As to the difference between the results of the drain-water analyses made by Dr. Frankland and those made by Dr. Voelcker, he (Dr. Gilbert), ascribed to the circumstance that Dr. Frankland had only samples of dribbles of the drains, whilst the samples investigated by Dr. Voelcker were collected at full flows. Referring to Dr. Voelcker's observation, that such important constituents as phosphoric acid and potash were not carried out from the ground, Dr. Gilbert remarked, that even by an increased addition of ammonia to the soil, no increase of phosphoric acid in the drain waters was to be noticed. This seems to destroy the theory according to which ammonia serves to dissolve the phosphoric acid. The speaker then referred to some experiments regarding the evaporation of rain water, which are carried on at present at Rothamsted.

Dr. Marsden thought that great analogy exists between animal and vegetable physiology. The soil does not retain those substances it has no need of, but holds well those necessary for the life of the plant. Of course, how they are retained we do not know with certainty. From the circumstance that nitrates do readily leave the ground, whilst no ammonia is met with in the drain waters, he concluded that nitric acid is not absolutely necessary for the plant.

Mr. Warington could not clearly see the analogy drawn by the previous speaker; the body is a *living* filter, the soil is not. As to the opinion of nitric acid not being required by the plant, he thought that the wonderful crops obtained with nitrate of soda manures are sufficient fact to refute it. Alluding, then, to the main results of Dr. Voelcker's paper, he said that they spoke very much against the utility of applying sewage manure by irrigation, since this would have to go on during periods when the activity of the vegetation is almost nought, and would thus entail loss of much valuable plant constituents. As to Dr. Voelcker's statement that nitrates are not well retained by the soil, he thought there were some experiments on record which would tend to show the contrary.

Mr. Grantham briefly mentioned that there are now drain gauges set up on the fields where irrigation experiments are carried on under the direction of the British Association Committee, and he thinks that soon some results will have been obtained bearing upon the question of the evening.

NOTICES OF BOOKS.

Die Prüfung Chemischer Gifte, ihre Erkennung in Reinem Zustande und ihre Ermittelung in Gemengen. Von Adolf Duflos, Docteur der Philosophie und Medicin, Königlichen Geheimen Regierungsrathe und Professor. Ein Leitfaden bei Gerichtlich-Chemischen Untersuchungen für Aerzte, Apotheker, Gerichtliche Chemiker und Criminal Richter. Mit vierzig in den text gedruckten abbildungen nach original zeichnungen. Ferdinand Hirt, Verlags und Königliche Universitäts-Buchandlung, Breslau. 1867.

The Chemical Testing for Poisons: their Detection in the Pure State as well as when they occur in Mixtures. By Adolf Duflos, M.D., Ph.D., Privy Councillor of H.I.R. Majesty of Germany and Prussia, and Professor at Breslau University. A Guide to Forensico-Chemical Research for Physicians, Apothecaries, Forensic Chemists, and Judges sitting in Criminal Courts. With 40 woodcuts prepared from original drawings. F. Hirt, Breslau. 1867.

From the preface of this volume we learn that the author has embodied, in the 210 pages constituting the book, the chief and leading features of a series of lectures given by him annually, for a consecutive period of twenty-five years, on the subject of chemical toxicology. The work is particularly intended as a guide-book for those German pharmaceutists who, under the regulations of the Medicinal-Polizei of Prussia, are occasionally required to institute chemico-forensic researches in cases where the detection of poisons may be required.

The arrangement of the contents of this useful work is novel and ingenious. It is well-known that most toxicologists arrange the various poisonous substances they treat of according to the action they exercise upon the living human system. The author has, very wisely, we think, introduced a different arrangement, more based upon chemical principles, viz.:—Poisons of the Chloroïd kind (chlorine and alkaline compounds, bromine, iodine, and their alkaline compounds); Poisons belonging to the Acid Substances, sub-divided into Mineral and Vegetable Acids; Alkaline Poisons (caustic alkalies, caustic baryta, &c.); Carbonates of the Alkalies; Sulphuretted Alkalies; Poisons belonging to the class of Saline Substances (alum, nitrate of potassa, chloride of barium, carbonate of baryta, chromate of potassa, &c.); Poisons of the Metalloid kind (Phosphorus); Poisons derived from the heavy metals (among these we quote as instances—Arsenical and antimonial, lead, copper, mercurial poisons); Cyanogen Poisons (the various poisonous compounds of cyanogen, inclusive of oil of bitter almonds and the like); Poisons derived from, or belonging to, the Alkaloids (coniine, nicotine, aniline, atropine, &c.). Since the author has had a very extensive practice as forensic chemist, the various processes detailed in this work for the detection of poisons, either by themselves, or in complex mixtures with organic matter, are only those which, by practical experience, he has found to answer the purpose readily and with certainty, if the operator attends to the directions given, which, as far as regards the necessary apparatus, are illustrated with excellent woodcuts. This little volume contains a good amount of valuable information, very clearly and practically described, and its publication has highly benefitted the German pharmaceutists, who, as *kreiszapotheker* (district pharmacists), are often required to test, for the presence of poisonous or noxious substances, articles of daily use. For this purpose full directions are clearly set forth.

Proceedings of the American Pharmaceutical Association at the Eighteenth Annual Meeting, held in Baltimore, September, 1870. Philadelphia: Sherman and Co. 1870.

This volume of 352 pages contains, in addition to subject-matter more particularly interesting to the Members of the American Pharmaceutical Association, a series of valuable papers relating not only to pharmacy, but extending to various other subjects bearing upon physical science in general. Among these, we quote the titles of the following papers and memoirs, regretting that space forbids us to do more at present:—"On a Morphio-metric Process for the Pharmacopœia," by W. Procter, jun.; "On Filtering-Papers and Filters," by J. M. Hirsch; "On the Specific Gravities Indicated by Baumé's Hydrometers," by Dr. W. H. Pile; "Glycerine and its Qualities," by J. P. Remington. The work, moreover, contains an excellent review of the progress of pharmacy, pharmacognosy, and pharmaceutical chemistry, with abstracts from a large number of periodicals published in different languages; reports of committees; laws relating to pharmacy in the states of Maryland, Rhode Island, and Pennsylvania; list of publications received; and concludes with a comprehensive alphabetical index. We must not omit to call attention to some valuable communications on the hydrate of chloral, from the hands of several gentlemen who have been practically engaged in its preparation and the investigation of its properties.

CORRESPONDENCE.

PRECIPITATION OF MUDDY MATTER FROM WATER BY SALINE SOLUTION.

To the Editor of the Chemical News.

Sir,—In No. 554 of the Chemical News (July 8th, 1870) I learn that Dr. C. Schlœsing has re-discovered the fact that muddy waters are clarified by dilute solution of various lime salts, also by caustic lime. I say a *re-discovery* because, in No. 435 of the same journal (April 3rd, 1868), I announced that a great many neutral saline solutions had the same effect upon such waters, chloride of calcium being amongst them.

Dr. Schlœsing states 1-10ooth part of this salt in one part of muddy waters clarifies them "in a moment." By allowing some hours for the operation, however, one grain of this salt is sufficient for 10 ounces, or say 1 to 50,000.

In the same communication where these results of mine are given, I also stated that magnesia had the same effect as saline solutions upon these waters: this being comparatively a tasteless substance, its use would perhaps be preferable to that of any other for purifying potable waters.

You observe, in your notice of this paper by Dr. Schlœsing, that Professor Johnson explains the clearing of muddy waters by certain bitter or other vegetable substances; but I do not know that he ever explained the *rationale* of the action of saline solutions upon such waters, or even noticed they had any action on them. I conceive this to be quite different for the two kinds of substances used, as will be seen on reference to my communication.

I will only further notice that, in a paper "On the so-called Molecular Movements of Microscopic Particles," I find Professor Jevons has observed the precipitation of clay by the same kind of substances as those cited here, and he attributes this, I believe, to the circumstance that these saline additions render the water a conductor of electricity; the movement and suspension of these clayey particles, before such additions, being supposed by him to be owing to the non-conducting power of pure water: the particles themselves he infers to be charged with electricity.

It is difficult to conceive how they become charged with electricity, or retain it when charged for any length of time, in a liquid which is by no means a *non*-conductor. I should rather refer the *suspension* and *precipitation* of these particles to chemical, and their *movements* (as Mr. Dancer does) to thermal, agency, if these cannot be explained or attributed to the simple effects of manipulating such substances under a high-powered microscope.—I am, &c.,

William Skey.

Wellington, New Zealand,
March 3, 1871.

MISCELLANEOUS.

The Royal Society.—The following gentlemen are reecmmended by the Council for election into the Royal Society, on June 8th; William Henry Besant, M.A.; William Budd, M.D.; George William Callender, F.R.C.S. William Carruthers, Esq.; Robert Etheridge, F.R.S.E.; Frederick Guthrie, B.A.; John Herschel, Capt. R.E.; Alexander Moncrieff, Capt. M.A.; Richard Quain, M.D.; Carl Schorlemmer, Esq.; Edward Thomas, Esq; Edward Burnet Tylor, Esq.; Cromwell Fleetwood Varley, C.E.; Arthur, Viscount Walden, P.Z.S.; John Wood, F.R.C.S.

Scientific Differences.—A great deal has been made of the differences of opinion of medical witnesses in disputed cases of injury for which compensation is claimed by legal processes. We have taken occasion to point out that these differences are, in great measure, the residuum of a great mass of agreement. For, in the vast majority of cases, doctors do agree; and such cases are settled by arbitration out of court. It is commonly only the cases which admit of various prognostication, and on which an agreement is not arrived at, that come into court. Under any circumstances, a large proportion of "difficult cases" are cases in which the medical opinion is arrived at by a balance of probabilities rather than of certainties, and by a varying estimate of the uncertain factors in the problem. But how is it with experts not medical, dealing with matters of fact, and employing the simple and unerring processes which physical science supplied? We read in the Chemical News of "buyers' analysts" and "sellers' analysts," who always manage to give different results in the examination of the same samples in commerce. Here we are a little puzzled; and if it be possible, as we read, for unconscious bias to add or take away 5 per cent of soluble phosphate from a sample, we shall expect to find chemists treating medical difficulties of agreement, in whatever case, with the utmost possible indulgence.—*British Medical Journal.*

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Moniteur Scientifique, April 1 and 15, Nos. 343 and 344 (double number), 1871.

This number opens with the report, by Dr. A. Proust, on the—

Means of Preventing Asiatic Cholera from Invading Europe.—A very lengthy document, but one of considerable value and interest.

The following original papers bear upon chemistry and allied sciences:—

Nature of the Purgative Principle contained in Senna.—Drs. E. Bourgoin and M. Bouchut.—The drug here spoken of has been frequently the subject of chemical analysis by various scientific, as well as pharmaceutical chemists, among whom we quote MM. Bouillon-Lagrange, Braconnot, Lassaigne, Batka, Kubly, Dragendorff, and others. The authors describe, in this lengthy memoir, the mode of their operations, as well as the physiological effect of the substances they have separated from the senna leaves, and which substances are mucilaginous matter, cathartine, catharto-mannitc, cathartic acid, and chrysophanic acid. It remains, however, an undecided matter whether these principles are, as such, present in the dried senna leaves, or are produced by the reaction which ensues when the leaves are treated with hot water, as the first operation required to prepare the various substances above named, none of which are obtainable in crystalline state.

Intervention of the Académie des Sciences in the General Questions of the Scientific Organisation of France.—Dr. H. Sainte-Claire Deville.—A lengthy and important paper, but one of local interest only.

Propagation of Heat through Two Layers of Different Liquids: Posthumous Researches of Dr. Despretz.—Communicated by Dr. Saigey.—The contents of this important paper, chiefly consisting of a series of tables exhibiting the results of the experiments, are not well suited for abstraction, an observation which applies to the following.

Calorific Effects of the Sun at the Two Extremities of the Terrestrial Atmosphere.—Dr. Saigey.

Although strictly belonging to the domain of astronomy, we quote the titles of the two following papers:—

Observations on the Black Colour of the Heavens (Noir du Ciel).—Dr. Saigey.

Zodiacal and Antizodiacal Light.—Dr. Saigey.

Observations on the Present State of Meteorology.—Dr. Saigey.—From this paper we quote the following particulars relating to the periods since which metcorological observations have been more or less regularly and continuously made:—Paris, 1735; by Dr. van Swinden, at Zwanenberg (half way between Amsterdam and Haarlem), 1736; Rev. Bordier, S.J., at Chandernagor, India, 1741; Rev. Amyot, S.J., at Pekin, China, 1757; at Neuchâtel, Switzerland, 1754; at London, by Professor Cavendish, 1763; at Brussels, by the Rev. Chevalier, 1763.

Polytechnisches Journal von Dingler, first number for April, 1871.

This number contains the following original paper relating to chemistry and collateral sciences:—

Crude Gold obtained at Lend, near Gastein (Austria).—E. Priwoznik.—This paper treats on a peculiar kind of thin matt, or scoria, which very strongly adheres to, and outwardly covers the ingots of gold obtained by amalgamation at the locality above named. Deducting such substances as are obviously accidental in this scoria, it was found to consist, in 100 parts, of the following sulphurets:—Silver, 61·99; iron, 29·04; lead, 5·33; copper, 3·64. The authorities of the Mint at Vienna, of which the author is the chemist, having been consulted with the view to ascertain the best and cheapest means to prevent the formation of this matt, a series of experiments were made by the author with the crude gold previous to fusion. It was found to consist, in 100 parts, of—Gold, 66·56: silver, 23·69; mercury, 5·78; iron, 1·14; lead, 0·66; copper, 0·87; sulphur, 0·47; antimony, a trace; silica, a trace. The experiments on the smelting of this gold proved that, if it were treated with a mixture of 2 parts of saltpetre and 1 part of borax in graphite crucibles, a metal was obtained free from adhering scoria, and composed, in 1000 parts, of 723 of gold and 250 of silver. The ingots were quite bright and smooth.

Journal de Pharmacie et de Chimie, October, 1870.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Thermo-Chemical Researches on Sulphurets.—Dr. Berthelot.—This essay is divided into the following chapters:—Reaction of the alkaline sulphurets on solutions of metallic salts; reaction of acids on alkaline sulphurets; reaction of sulphuretted hydrogen on various metallic salts; and reaction of acids on metallic sulphurets.

Chemical Research on the Crystalline Substance which Covers the Vanilla Pod.—M. P. Carles.—This paper treats on the nature of the crystalline substance which may frequently be seen on the vanilla pods, and have been considered by various authors to consist of benzoic or cinnamic acid, or coumarine. The author finds that this crystalline matter fuses at about 81°; boils, with partial decomposition, at 280°; is very readily soluble in alcohol, ether, chloroform, and sulphide of carbon; difficultly soluble in cold water; exhibits a strongly acid reaction to blue litmus paper; and decomposes carbonates. Elementary organic composition, $C_{16}H_8O_6$. The author has prepared several salts of this acid; but, as to its true chemical constitution, his labours are not yet completed.

New Process for the Preparation of Bromhydric Acid.—Drs. Champion and Pellet.—This process is based upon the reaction ensuing between bromine and paraffin, the former being heated to about 65°, the latter to 185°, and the operation conducted in suitably arranged apparatus; by continuing the action until no more bromhydric acid is formed, there remains a residue, consisting, in 100 parts, of—Carbon, 63·0; hydrogen, 5·3; bromine, 31·6. The bromhydric acid thus obtained has, at 0°, a sp. gr. of 1·78; its formula is $BrH,2HO$. When heated, hydrobromic acid gas is given off and a hydrate obtained, which boils at 126°, and has a sp. gr. of 1·48; formula, $BrH,10HO$.

Conversion of Chloral into Aldchyde by Inverse Substitution.—M. Personne.—When hydrate of chloral is treated with zinc-dust and hydrochloric acid, the chloral-hydrate is converted into aldehyde, which may thus be readily obtained. The author has also obtained a compound of anhydrous chloral with ammonia, $C_4HCl_3O_2,NH_3$, or the aldehydate of trichlorated ammonia, comparable to the aldehydate of ammonia, $C_4H_4O_2,NH_3$. The body first named is prepared by causing dry ammoniacal gas to act upon small quantities of anhydrous chloral, which should be strongly cooled. The substance in question is a white, fusible, volatile body, the odour of which is comparable to aldehydate of ammonia. When larger quantities of anhydrous chloral (say 2 or 3 grms.) are operated upon, there is also generated a liquid which was found to consist of chloroform and formamide.

November and December (double number), 1870.

This number contains no original papers.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 5, 1871.

This number contains the following original papers and memoirs:—

Chlorine Substitution Products of Ether.—O. Jacobsen.—The author describes, at great length, and with the quotation of a large number of complex formulæ, the products of the action of chlorine upon ether. These products are—A real monochlor-ether, boiling at 98° (formula, $CH_3.CHCl.O.C_2H_5$; a dichlor-ether—

$$CH_2Cl.CHCl.O.C_2H_5;$$

a trichlor-ether, $CHCl_2.CHCl.O.C_2H_5$; a tetrachlor-ether—

$$CCl_3.CHCl.O.C_2H_5;$$

and, lastly, a pentachlorether, $CCl_3,CCl_2.O.C_2H_5$.

Use of Bromine instead of Chlorine for Analytical Purposes.—H. Kämmerer.—The contents of this paper record the fact that, instead of chlorine-water, a solution of bromine in the same liquid answers not only as well as, but, in many instances, better than chlorine-water, while there is neither difficulty in keeping, nor, when required, in preparing the bromine solution.

Organic Sulphuric Acid Derivatives.—H. Kämmerer.—This paper contains the rectification of some particulars concerning sulphobenzoic acid anhydride, formerly made public by the author and Dr. Carius; but this communication is only a preliminary notice.

Contribution to our Knowledge on Sulpho-Nitrogen Acids (Schwefelstickstoffsäuren).—A. Claus.—The continuation of the author's essay on sulphamine acids. The substance here treated of is sulphydroxylaminate of potassa, $(NO_2H_2).(SO_3K)$.

Constitution of Chrysanissic Acid.—H. Salkowski.—A lengthy monograph describing a series of reactions and the behaviour of chrysanissic acid (the product of the action of fuming nitric acid upon nitroanissic acid) with various reagents, in order thereby to elucidate the true constitution of chrysanissic acid. Formula, $C_7H_5N_3O_7$.

Some of the Nitro Compounds of Anthrachinon.—R. Bœttger and T. Petersen—And—

Observations and Remarks on the above-mentioned Essay—C. Liebermann—Are, although valuable, not suitable for abstraction, owing to their great length.

Nature and Constitution of Tannic Acid.—H. Schiff.—The main point of interest of this valuable essay is, that tannic acid is an alcoholic anhydride of gallic acid, very probably digallic acid. The relation between these acids is elucidated by the following formulæ;—

$$C_6H_2\begin{cases}CO.OH\\OH\\OH\\OH\end{cases}$$

Gallic acid.

$$C_6H_2\begin{cases}CO.OH\\OH\\OH\\O\end{cases}\qquad C_6H_2\begin{cases}OH\\OH\\CO.OH\end{cases}$$

Tannic acid.

Law of Dulong and Petit in Gases.—Dr. F. Mohr.

Absolute Magnitude of Chemical Motion (Affinity).—Dr. F. Mohr.

Metallic Nature of Hydrogen.—Dr. F. Mohr.

Lecture Experiments.—Dr. A. W. Hofmann.—This paper treats on the following subjects:—Eudiometer with movable wires for conducting the spark, illustrated with a woodcut; doubling of the volume of carbonic acid when converted into carbonic oxide by absorption of carbon. This experiment cannot be described without the reproduction of the woodcut just mentioned.

Isodicyanic Ether Compounds which Occupy the Mean between the Cyanic and Cyanuric Acid Ethers.—Dr. A. W. Hofmann.—This essay contains the following sections:—Diphenyl-allophanic acid ethylic ether, methylic ether, amylic ether; sulphuretted diphenyl-allophanic acid amylic ether; action of phenol upon phenyl-dicyanate; diphenyl-biuret; triphenyl-biuret.

Contribution to the History of Naphthazarine.—Drs. A. A. de Aguiar and A. G. Bayer.

Isomeric Cyanate of Potassa.—A. Bannow.—The author communicates, in this paper, some of his experiments on the action of

caustic potassa upon paracyanogen, whereby a salt is obtained forming long needle-shaped crystals, and found, on analysis, to consist of—C, 14·8; N, 17·0; K, 48·2; O, 19·72; a salt, therefore, of the same composition, and agreeing in properties with the ordinary cyanate of potassa. This salt is also formed when chloro-cyanogen is passed into a cold aqueous solution of caustic potassa.

Preparation of Aceto-Chloral from Aldehyde.—A. Pinner.—After first referring to a series of experiments formerly made on the action of chlorine upon aldehyde, the author describes, in this paper, other experiments of the same nature, but so conducted as to eliminate all hydrochloric acid at the moment of its generation. This was effected by adding to the aldehyde lumps of marble, care being taken also to cool the liquid by means of ice. After the action of the chlorine had been continued for about two days, the fluid was distilled from the chloride of calcium; to the aqueous solution thus obtained, four times its bulk of concentrated sulphuric acid was added, and again distilled. The result was that the author obtained about 20 grms. of a fluid which was found to possess all the properties of aceto-chloral, also agreeing therewith in the property of being converted, by the action of alkalies, into formic acid and chloroform.

Combination of Aldehyde and Sulphaldehyde.—A. Pinner.—The contents of this paper contain the description of a new compound which was obtained by the author while preparing sulphaldehyde according to the directions given for that purpose by Dr. Weidenbusch (*Ann. Chem. Pharm.*, vol. lxvi., p. 158). The new body here alluded to is an oily substance, $C_2H_4O+C_2H_4S$; the quantity of sulphur was found by the author to vary from 30·65 to 31·39 per cent. This oil freezes at −8°, and melts at −2°; it boils at 35° with partial decomposition. The vapour density of this oil was found to be 61·2.

Conversion of Bromobenzoic into Isophthalic Acid.—E. Ador and V. Meyer.—The potassium salt of pure mono-bromobenzoic acid is fused at a strong heat along with its own weight of formiate of soda; the resulting deep brown mass is treated with water, the solution acidulated with hydrochloric acid, and next well shaken up with ether. The acid which is left after distilling off the ether is dissolved in ammonia, purified by means of animal charcoal; boiled with water, in order to extract any benzoic acid which might have been regenerated; then made into a barium salt; and the pure isophthalic acid obtained by the decomposition of this salt. The acid thus obtained fuses at 300°, sublimes without decomposition, is soluble in boiling-hot water, and consists, in 100 parts, of—C, 57·83; H, 3·62; O, 28·95. The baryta salt is crystalline, and readily soluble in water; the silver salt is a precipitate insoluble in water.

Biuret and Analogous Compounds.—Dr. A. W. Hofmann.—This lengthy essay is divided into the following sections:—On the preparation and properties of biuret (discovered by Dr. Wiedemann in 1847; see Poggendorff's *Annalen*, vol. lxxiv., p. 67, 1849); substituted biurets; action of ammonia on allophanic acid-ethyl ether; action of aniline on the same body; action of ethylamine on the same body; action of aniline on biuret; action of heat upon urea in the presence of amylic alcohol.

Observations and Remarks on a Paper by M. Mohr, "On the Unequal Conductibility of Heat by Gases."—Dr. R. Clausius.

Law of Avogadro.—A. Naumann.

Mellittic Acid.—Dr. A. Baeyer.—This memoir is divided into the following sections:—Prehnitic acid; mellophanic acid; prehnomalic acid, $C_{10}H_8O_9$; hydro-pyromellittic acid; bromalophthalic acid; tartrophthalic acid.

Annalen der Chemie und Pharmacie, April, 1871.

This number contains the following original papers and memoirs:—

Contribution to our Knowledge of Brom- and Dibrom-Benzoic Acid.—E. Angerstein, L. H. Friedburg, and H. Hübner.—This paper is the first part of a lengthy monograph on the isomerism of the aromatic acids, and is divided into the following sections:—On the brom-benzoic acid which is formed by the action of bromine on benzoate of silver; experiment made to try whether, by the conversion of β-brom-nitro-benzoic acid into meta-amido-benzoic acid, other amido-benzoic acids are formed; dibrom-benzoic acid, dibrom-nitro-benzoic acid, and dibrom-amido-benzoic acid; on the conditions of the generation of ortho-mono-brom-benzoic acid; examination of crude brom-benzoic acid to discover therein another acid than the ortho-mono-brom-benzoic acid fusing at 155°; action of bromine upon benzamide; action of bromine upon benzo-nitrile; researches on the preparation of meta-mono-brom-benzoic acid (brom-salylic acid).

Action of Chlorine upon Aldehyde.—G. Krämer and A. Pinner.—This lengthy memoir contains the following chapters;—Description of the mode of experimenting and the origin and properties of the substances operated upon; croton-chloral; croton-chloral-hydrate; dichlor-allylen; dichlor-dibrom-propylen; trichlor-crotonic acid; monochlor-crotonic acid.

Mulberry Leaves from Turkestan.—Dr. E. Reichenbach.—This paper contains, in the first place, a report from M. Adamoli, on the different kinds of mulberry trees met with, partly in a wild state, partly cultivated, in Turkestan (Central Asia). The average quantity of nitrogen contained in five different varieties of these leaves amounts to 3·73: while the Japanese mulberry leaves only contain 3·29 per cent, and Chinese mulberry leaves 3·13 per cent of nitrogen. There is added to this paper, as an appendix—

Essay on the Silkworm Disease, in reference to the Observations mentioned in the Paper of Dr. E. Reichenbach.—Baron von Liebig.

Boiling-Point and Specific Volume of Allylic Alcohol.—B. Tollens.—The boiling-point of the perfectly anhydrous allylic alcohol was ascertained to be 96°, tried on the first portion of this liquid; after being doubly rectified over lime, 94·5°. The specific volume (not to be confused with specific gravity, which, at 0°, is 0·7846) is 73·92.

Conversion of Formic Acid into Methylic Alcohol.—M. A. Lieben and M. A. Rossi.—The authors describe at length a series of experiments made with formiate of lime, taken in quantities of 10 grms. at a time, and submitted to dry distillation, care being taken to condense the volatile products by means of a cooling mixture. Among these products the authors found a comparatively small, yet perfectly recognisable, quantity of methylic alcohol. 250 grms. of the formiate of lime gave between 3 and 4 grms. of that fluid, which, in order to make quite sure about its nature, the authors converted into iodmethyl.

Estimation of Carbonic Acid in Spring Waters.—Dr. K. Knapp.—This paper contains the record of a series of experiments instituted by the author with the view to test the correctness and sensitiveness of Dr. Pettenkofer's method of estimating the quantity of carbonic acid contained in water, by adding, to a known bulk thereof, first, chloride of calcium solution, sal-ammoniac, and, next, a measured quantity of lime-water of known strength. After having been left quietly standing for fully twelve hours, the quantity of lime which remains in solution is estimated by titration with a solution of oxalic acid, 2·8636 grms. of that acid to 1 litre of water. Every c.c. of that solution corresponds to 0·001 grm. of carbonic acid. The point of neutralisation of the lime-water is ascertained by means of turmeric paper.

Excrements of the Common Bat (Rhinolophus hipposideras).—Prof. Wöhler.—After briefly referring to the researches on the excrements of the Egyptian bat by Dr. Popp (see CHEMICAL NEWS, vol. xxii., p. 202), the author states that Dr. Ehlers had the kindness to supply him with the material above named, taken from a locality where it had accumulated to a layer of 3 inches' thickness. This substance does not contain any trace of urea, nor any uric or oxalic acids; the bulk is made up of the undigested horny mass of the wings of insects. Dried at 100°, this substance gave 8·25 per cent of nitrogen, and left, after ignition, 6·25 per cent of ash, containing potassa, soda, lime, magnesia, oxide of iron, chlorine, sulphuric, silicic, and phosphoric acids, the latter being 36 per cent of the weight of the ash.

Butyric Acid from Various Sources.—Dr. C. Grunzweig.—(Preliminary notice.)—The main point of interest in this short notice is, that the butyric acid met with in the fruits of the carob tree (*Ceratonia siliqua*, L.) is isobutyric, not normal butyric acid.

Decomposition of Cyanogen by an Alcoholic Solution of Hydrochloric Acid.—J. Volhard.—Into an absolute alcoholic solution of hydrochloric acid gas cyanogen gas was passed. When the alcoholic acid solution is employed for this purpose undiluted, the result of the reaction is only the formation of sal-ammoniac; but, if the alcoholic fluid is previously diluted with more alcohol, oxamide is also formed, as well as oxalic ether, ethylic chloride, and some formic ether. The reaction takes place according to the formula—

$$C_2N_2+4C_2H_5OH+4HCl=C_2O_2(C_2H_5O)_2+NH_4Cl+2C_2H_5Cl.$$

Lecture Experiment to Prove that Mercury is Heated while a Galvanic Current Passes through it.—Dr. F. C. G. Müller.—Take a glass tube, 6 c.m. long and about 6 m.m. diameter; heat it before the blowpipe, and, while soft, reduce its diameter in the centre to ⅓ m.m., and next bend it to a U-shape; fill it with mercury, fasten it in a clamp, and dip afterwards in the metal the wires of a galvanic battery. This having been done, the metal (mercury) in the narrowed portion of the tube will be observed to boil rapidly, while a continuous series of sparks will be also exhibited.

Annales de Chimie et de Physique, August, 1870.

This number only contains the following original paper:—

Universal Method of Reduction by, and Saturation with, Hydrogen of Organic Compounds.—Dr. Berthelot.—This monograph, containing about 250 pages, is divided into the following sections:—Introduction; series of the properly so-called fatty bodies; aromatic series; nitrogenised bodies; complex and polymeric carburets; carbonaceous matters (*matières charbonneuses*).

Journal für Gasbeleuchtung und Wasserversorgung, No. 4, 1871.

This number contains the following original paper bearing upon chemistry and allied sciences:—

Changes of the Quality of the Water of Rivers.—Dr. H. W. Langhaus.—This essay treats on the effect which the passing of rivers through more or less densely populous districts has upon the quality of the water of such rivers. As an instance, the author gives an analysis of the water of the river Pegnitz, on entering the city of Nürnberg, and on leaving it. As might be expected, the total quantity of solid matter in the water is increased; for, while that amounts (in 100,000 parts, by weight) on entering the city to 20·82, it has increased on leaving to 21·17. For every litre of water flowing through the city, there has been added to it—Potassa, 0·0016 grm.; soda, 0·0013; oxide of ammonium, 0·0005; silica, 0·0004; nitric acid, 0·0016; chlorine, 0·0020; soluble organic matter, 0·0084; insoluble organic matter, 0·0023; suspended matter, 0·0084. On the other hand, the following substances have decreased in quantity to the litre of water by passing through the city:—Lime, 0·0006 grm.; magnesia, 0·0018; alumina and oxide of iron, 0·0002; sulphuric acid, 0·0002.

No. 5, 1871.

This number only contains the following original paper relating to chemistry:—

Hydrotimetry.—M. E. Grahn.—This paper is a review of, and reply to, a series of critical remarks, made by various parties, on a paper published on this subject in this periodical some time ago. The chief point of discussion is on the value of the various methods in use for estimating the hardness of the water.

Zeitschrift für Chemie von Beilstein, No. 3, 1871.

This number contains the following original papers:—

Estimation of the Value of Hydrate of Chloral.—Dr. C. Müller.—Since the value of the hydrate of chloral is indicated by the quantity of chloroform it yields when decomposed by caustic alkalies, the author places 25 grms. of the hydrate of chloral to be tested in a glass tube divided into 1-10th c.c.. The solid substance having been placed therein, a good cork is selected for closing the tube immediately after the addition of caustic potassa solution, and the tube, having been closed, is placed in a cooling mixture. After the first violent action has ceased, the reaction may be finished by shaking the tube for a few moments, after which it should be left standing for some hours quietly, in order to obtain a complete separation of the chloroform. The number of c.c. this fluid occupies in the tube, having been read off, has only to be multiplied with its specific gravity at the temperature of the fluid to yield, by a simple calculation, the percentage quantity of the chloroform thus formed.

Bromsulpho-Benzoic Acid and Acids Derived from it.—Dr. J. R. van Lennep.—This paper contains, in a catalogical form, the enumeration of the salts of ortho-mono-bromsulpho-benzoic acid, and treats on the action of bromide of phosphorus upon bromsulpho-benzoate of sodium, thihydro-brombenzoic acid and its salts, and meta-thihydro-benzoic acid.

New Mode of Formation of Para-Oxybenzoic Acid.—Dr. I. Remsen.—The main gist of this paper is, that when sulpho-benzoate of potassium is treated with fused caustic potassa, there is, along with oxybenzoic acid, also formed para-oxybenzoic acid. The author also observes that crude sulpho-benzoic acid should not be used for the preparation of pure oxybenzoic acid.

Action of Hydrogen (Sodium-Amalgam) upon Essential Oil of Bitter Almonds.—Dr. H. Ammann.—(Preliminary notice).—The author's scientific labours have been cut short, he having been killed (as we learn from this paper) while in the battle-field in the late war. When sodium-amalgam acts upon the essential oil of bitter almonds, either in aqueous or alcoholic solution, there are formed, beside benzyl-alcohol, simultaneously, two isomeric compounds, one of which is identical with Zinin's hydrobenzoine, while to the other the name of isohydrobenzoine has been given. This latter substance is a solid body, soluble in water to some extent, readily soluble in alcohol, and fusing at 119·5; formula, $C_{14}H_{14}O_2$. The author briefly mentions the action of chloracetyl, nitric acid, and chloride of phosphorus upon this substance, as well as upon hydrobenzoine.

Neues Jahrbuch für Pharmacie, von Dr. F. Vorwerk, February, 1871.

This number contains the following original papers relating to chemistry and allied sciences.

Tea-Trade between Russia and China.—K. A. Popow.—This very lengthy essay contains some matters which are interesting to all who use the infusion of this herb, a national beverage with only three European nations, those which have the most extensive and oldest connections with Asia—viz., the Russians, English, and Netherlanders. Tea has been used in Russia since 1638; in the western parts of Europe, tea was first imported by the Dutch about the beginning of the seventeenth century.

The other papers in this number are strictly pharmaceutical.

NOTES AND QUERIES.

Nitrous Oxide.—Can any of your numerous correspondents inform me whether it is possible to convert nitrous oxide (N_2O) into nitric oxide (NO_2).—INQUIRER.

Improved Bunsen Battery.—Having inquired from one of Professor Bunsen's assistants, I am able to inform J. McCulloch that he has not yet published an account of his improved battery.—E. W. P., Ploeckstrasse, Heidelberg.

Manipulating India-Rubber.—(Reply to F. Bankes.)—You will find the information you desire in Payen's "Précis de Chimie Industrielle," 5th edition, vol. i., pp. 186—235. This work is published by Messrs. L. Hachette and Co. (1867).

Analysis of Iron Ores.—(Reply to L. Peters.)—Consult "Select Methods of Chemical Analysis," by W. Crookes, F.R.S., &c. This work contains a very complete account of the subject you inquire after.

Notched Teeth.—Could any of your readers tell me the cause of notches they would oblige, as I have read somewhere they are a sign of syphilis, and have been told to the contrary.—READER.

Welding Iron and Steel.—I am obliged to "A." for his kind consideration of my question, but I am not satisfied with his reply. I am aware that cast-steel cannot be welded at the heat at which iron is welded to iron—in that case it would, of course, undergo fusion and oxidation; but is there not a possibility of uniting them without raising the heat of the steel to the white heat at which iron is usually welded? Can it not be welded at a red heat with the care referred to in the making of chisels, and the application of some kind of flux (to defend the metal) containing iron or steel in a fine state, and, consequently, easily softened. Could a junction not be made which would give complete satisfaction? Such a process is said to be in actual practice, but perhaps it belongs to the category of processes that cold brazing belongs to.—W. A. MURRAY.

Valuation of Orchil.—(Reply to "Orchil.")—The colorific portion of the lichens may, according to Dr. Schutzenberger, vary from 2½ to 12 per cent. In order to estimate the value of orchil lichens, the author just named recommends a comparative assay, by macerating equal quantities (100 grms.) of the lichens with milk of lime for a few minutes, after which the mass should be filtered and pressed, and the filtrate treated with hydrochloric acid. The white-coloured pasty precipitate thereby produced should be left to drain on a fine silk sieve, and weighed. Dr. J. Stenhouse adds, drop by drop, a titrated solution of hypochlorite of lime to the calcareous extract of the lichens, so as first to call forth colouration; and when this has reached its maximum, more of the hypochlorite solution is added, so as to produce discolouration. By comparing the bulk of hypochlorite liquor required to discolour the calcareous extract of various samples of lichens, an approximative idea of their richness in colouring matter is gained. Lastly, 100 grms. of lichens may be treated with an aqueous alkaline solution, the solution thus obtained evaporated to 100 grms., and next treated with 30 grms. of ammonia, whereby the colour is produced. This having been done, a dycing assay with non-mordanted wool will give some idea of the quantity of colouring matter formed. For a piece of wool 5 centims. long by 2 wide, there is applied from 0·5 to 1·0 grm. of orchil and 300 grms. of water; the dyeing is done on a water-bath, and the process continued at boiling-heat for half an hour.

MEETINGS FOR THE WEEK.

TUESDAY, 16th.—Zoological, 9.
—— Civil Engineers, 8.
—— Royal Institution, 3. Charles Brooke, M.A., F.R.S., "On Force and Energy."
WEDNESDAY, 17th.—Society of Arts, 8.
—— Pharmaceutical, 11. Annual Meeting.
THURSDAY, 18th.—Chemical, 8. R. J. Friswell, "On a New Double Salt of Thallium." Dr. Armstrong, "On a New Benzolic Derivative."
—— Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
FRIDAY, 19th.—Royal Institution, 9. Professor Huxley, F.R.S., "On Bishop Berkeley and the Metaphysics of Sensation."
SATURDAY, 20th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."

TO CORRESPONDENTS.

P.—Consult the index.
The Chemist in the Studio.—This anonymous article is too much like a puff to be inserted in its present form.
Chemiker.—The Patent Office Library, Southampton Buildings, Chancery Lane; admission is free from 10 till 4 daily; you will find most of the works there.
Dr. Isidor Walz.—Received with thanks.

Just published, demy 8vo., 6s., by post 6s. 6d. (vol. i.),

Transactions of the Chemical Society of Newcastle-upon-Tyne.

Andrew Reid, Printing Court Buildings, Newcastle-on-Tyne.

Royal 8vo., pp. 388, price 7s. 6d. Third Edition.

The Patentee's Manual; being a Treatise on the Law and Practice of Letters Patent, especially intended for the use of Patentees and Inventors. By JAMES JOHNSON, Barrister-at-Law; and J HENRY JOHNSON, Assoc. Inst. C.E., Solicitor and Patent Agent.

The call for a Third Edition of this work is conclusive proof that it satisfies a want on the part of Patentees and Inventors, to whom a plain statement of the law bearing upon the subject of Letters Patent for Inventions is obviously a matter of great importance. Whilst the exposition of statutes and judicial decisions is expressed in plain and popular language, no sacrifice has been made of legal accuracy, and it will be found that the work contains a concise but ample and strictly correct enunciation of the law, with an examination of the decided cases to the latest date.

London: Longmans, Green, and Co.

Just published, price One shilling,

A Concise View of the Law connected with Letters Patent for Inventions. By JAMES JOHNSON, of the Middle Temple, Barrister-at-Law, and J. HENRY JOHNSON, Assoc. Inst. C.E., Solicitor and Patent Agent, Lincoln's Inn Fields and Glasgow. Authors of the "Patentees' Manual."

London: Longmans, Green and Co., Paternoster Row.

THE CHEMICAL NEWS.

Vol. XXIII. No. 599.

AN EXPERIMENTAL INQUIRY INTO THE CONSTITUTION OF BLOOD, AND THE NUTRITION OF MUSCULAR TISSUE.*

By WILLIAM MARCET, M.D., F.R.S.,
Senior Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

The results obtained from the inquiry which forms the subject of the paper are as follows:—

(1.) That blood is strictly a colloid fluid.

(2.) That although blood be strictly a colloid, it contains invariably a small proportion of diffusible constituents amounting to nearly 7·3 grms. in 1000 of blood, and 9·25 grms. in an equal volume of serum; these proportions diffusing out of blood in twenty-four hours.

(3.) That the proportion of chlorine contained in blood has a remarkable degree of fixity, and may be considered as amounting to 3 parts (the correct mean being 3·06) in 1000; the proportion of chlorine in a bulk of serum equal to that occupied by 1000 grms. of blood being 3·45, and therefore higher than in blood; and, moreover, that one of the objects of the chlorides, and other diffusible constituents of blood, appears to be to preserve the fluid state of the blood. The substances which yield an alkaline reaction to blood are crystalloid; their being retained in the blood while circulating through the body must be of the highest importance in connexion with the phenomena of oxidation constantly in progress during life.

(4.) That blood contains phosphoric anhydride and iron in a perfect colloid state, or quite undiffusible when submitted to dialysis, the relative proportions appearing to vary from 78·61 per cent of peroxide of iron and 29·39 of phosphoric anhydride, to 76·2 and 23·8 respectively; the proportion of phosphoric anhydride having a tendency to be rather higher.

(5.) That blood contains more phosphoric anhydride and potash, bulk for bulk, than serum. This fact has long been known; but I have shown that the excess of phosphoric anhydride and potash in the blood-corpuscles is greater than can be accounted for by the estimation of the proportions of colloid phosphoric anhydride and potash in blood and serum, consequently there exists in blood-corpuscles a power checking the diffusion of the diffusible substances they contain, and apparently connected with a force peculiar to the corpuscles, as this force ceases to act as soon as the corpuscular form disappears, from admixture with water. This property inherent to blood-corpuscles may cause an accumulation of potash in blood equal to no less than rather more than four times the amount of this substance present in an equal bulk of the serum of the same blood.

(6.) That a mixture of colloid phosphoric anhydride and potash can be prepared artificially by dialysis, and that the colloid mass thus obtained appears to retain the characters of the neutral tribasic phosphate from which it originates; it exhibits an alkaline reaction, yields a yellow precipitate with nitrate of silver, and after complete precipitation the reaction is acid.

(7.) That by dialysing certain proportions of phosphate of sodium and chloride of potassium during a certain time, proportions of phosphoric anhydride, potash, chlorine, and soda are obtained in the colloid fluid very similar to the proportions these same substances bear to each other in serum after twenty-four hours' dialysis.

(8.) That muscular tissue is formed of three different classes of substances; the first including those substances which constitute the tissue proper, or the portion of flesh insoluble in the preparation of the aqueous extract, and consisting of *albumen* and *phosphoric anhydride* with varying proportions of *potash* and *magnesia*; the second class including the same substances as are found in the tissue proper, and in the same proportions relatively to the albumen present in that class, but existing in solution and in the colloid state; the third class including the same substances as are found in the two others, and, moreover, a small quantity of chlorine and soda, which, although relatively minute, is never absent. The constituents of this class are crystalloid, and consequently diffusible, the phosphoric anhydride and potash being present precisely in the proportion required to form a neutral tribasic phosphate, or a pyrophosphate, as the formula $2KOPO_5$ can equally be $2KOHOPO_5$.

Class No. I. of the constituents of muscular tissue is to be considered as the tissue in the complete stage of assimilation.

Class No. II. constitutes the material from the blood on its way to form the Class No. I.

Class No. III. constitutes the material from Class No. I. in the *effete* state, and on its way out of flesh.

(9.) That flesh contains in store a supply of nourishment equal to about one-third more than its requirement for immediate use, this being apparently a provision of nature to allow of muscular exercise during prolonged fasting.

(10.) That the numbers representing the excess of phosphoric anhydride and potash in blood, over the proportion of these substances in an equal volume of serum in the regular normal nutrition of herbivorous animals, appear to bear to each other nearly the same relation as that which exists between the phosphoric anhydride and potash on their way out of muscular tissue, from which result I conclude that the blood-corpuscles have apparently the power of taking up and preparing the material which they themselves supply to muscular tissue for its nutrition.

(11.) That vegetables used as food for man and animals, such as flour, potato, and rice, transform phosphoric anhydride and potash from the crystalloid or diffusible into the colloid or undiffusible state; and it is only after having been thus prepared that these substances appear to be fit to become normal constituents of blood, and contribute to the nutrition of flesh.

A final remark, and one which is worth consideration, is the fact established by the whole of the present investigation, that there is a constant change, as rotation in nature from crystalloids to colloids, and from colloids to crystalloids.

The substances destined to nourish plants being inanimate must be diffusible, otherwise they could not be distributed throughout the mineral kingdom, and brought within the reach of plants. Vegetables transform into colloids the mineral substances destined to the food of animals, and it might be said that the locomotion of animals in some respects acts the same part as the diffusion of mineral substances; for animals move about in search of their colloid food, and crystalloid minerals are displaced by physical diffusion in search of the plants they are to nourish.

The excretory products of animals are crystalloid or diffusible, as far as these soluble constituents are concerned, the solid portions rapidly decomposing in contact of air and moisture into crystalloid compounds. Dead vegetable and animal tissue all return into crystalloids by decomposition, to be distributed afresh either by gaseous or liquid diffusion throughout the whole of the mineral world.

Hence Graham's great discovery of the laws of liquid and gaseous diffusion lifts up the curtain which veils the mysteries of animal life, and throws a flood of light on very many physiological phenomena which had until now remained in darkness.

* Abstract of a paper read before the Royal Society, May, 1871,

PRELIMINARY NOTICE ON THE

CHLORINE SUBSTITUTION COMPOUNDS OF THE ORCINS.

By JOHN STENHOUSE, LL.D., F.R.S., &c.

SCHUNCK, and subsequently the writer of this notice, many years ago studied the action of chlorine upon orcin, and obtained more or less crystalline products, contaminated with a brown resinous matter, from which, however, they did not succeed in separating the crystals in a state of purity. In the year 1864, De Luynes obtained a crystalline substance by acting on orcin with a mixture of potassium chlorate and hydrochloric acid; it had a melting-point of 159° C., and by analysis was found to have the composition of trichlor-orcin, $C_7H_5Cl_3O_2$,

Pentachlor-Orcin.—When a strong solution of orcin is added, with constant agitation, to a mixture of chlorine hydrate and water, care being taken to keep the latter in excess, the chlorine hydrate gradually disappears, and on standing the solution deposits a white crystalline substance, which is nearly pure pentachlor-orcin. In a pure state, it crystallises in large transparent lustrous prisms, which melt at 120·5°, and on analysis were found to have the composition $C_7H_3Cl_5O_2$; by long boiling with water they are decomposed. Aqueous solutions of the alkalies yield humus-like substances. I have also succeeded in isolating pentachlor-orcin from amongst the products of the action of potassium chlorate and hydrochloric acid on orcin.

Action of Hydriodic Acid on Pentachlor-Orcin.—When pure pentachlor-orcin is digested for a sufficient length of time with hydriodic acid and amorphous phosphorus, an oil is obtained which solidifies, on cooling, to a crystalline mass; this is very soluble in alcohol, and can thus be readily separated from the excess of amorphous phosphorus. On adding water to the alcoholic solution, a white precipitate is formed, which can be obtained in a pure state by one or two re-crystallisations from boiling water. It forms long colourless needles, which, on analysis, were found to have the composition $C_7H_5Cl_3O_2$, that of *terchlor-orcin*. They are not, however, identical with the substance described by De Luynes under that name, as they melt at 123°, whereas he gives 159° as the melting-point of his terchlor-orcin. The substance appears to have feeble acid properties, being soluble in dilute alkalies and re-precipitated by acids.

Action of Calcium Hypochlorite on Orcin.—When a solution of orcin in hydrochloric acid is gradually added to a somewhat dilute solution of calcium hypochlorite, a considerable amount of chlorine is evolved, and the solution becomes turbid. After some time, a crystalline substance is deposited, which, when purified and subjected to analysis, was found to have the composition—

$$C_7H_3Cl_5O_2,HClO.$$

I propose for this substance the provisional name of *pentachlor-orcin hypochlorite*. When pure it forms large prismatic crystals, which melt at 140·5°.

Action of Chlorine on Resorcin, Pentachlor-Resorcin.—The action of chlorine hydrate on resorcin does not yield by any means satisfactory results, the products being oily and uncrystallisable. I have, however, succeeded in isolating, from amongst the products of the action of potassium chlorate and hydrochloric acid on resorcin, a crystalline substance which, when purified, forms large colourless prisms very closely resembling pentachlor-orcin. They melt at 92·5°, and the analysis corresponds to the formula C_6HCl_5O. The action of hydriodic acid on pentachlor-resorcin yields a crystalline substance, which appears to be terchlor-resorcin.

I am at present engaged in more fully investigating the compounds above mentioned, and also some of their derivatives, which, from preliminary experiments I have made, appear to be very interesting.

ON THE

MOLYBDATES AND VANADATES OF LEAD,

AND ON A

NEW MINERAL FROM LEADHILLS.*

By Professor Dr. ALBERT SCHRAUF, of Vienna.

IN 1825,† Professor Wöhler published the analysis of a rare variety of pyromorphite from Leadhills, which he describes as an aggregation of very diminutive, orange-red hexagonal crystals, fixed on cerussite. The normal constituents of pyromorphite are mixed in them with small quantities of iron and arsenic. Professor Wöhler having ascertained the existence of vanadine-lead only five years later (1830), and the presence of partly chromium, partly vanadium in Russian specimens of pyromorphite having been stated, it may be supposed that the red pyromorphite from Leadhills owes its characteristic colouration to one or both the elements named before.

An examination of a great number of cabinet specimens from Leadhills gave no satisfying result. I could not find any red pyromorphite, as described by Professor Wöhler, on the specimens at my disposal, and must, consequently, leave unproved the above-stated supposition concerning the colouring substance of red pyromorphite. A cabinet specimen, dating from the years 1820-1825, gave me, however, the proof that the specimens of cerussite found at Leadhills during this period were not completely free from an admixture of molybdenum and vanadium. I succeeded in finding out a specimen of crystallised "vanadine-molybdate of lead" from Leadhills so strikingly different by crystallographical and chemical characters from its next allies, Wulfenite and Descloizite, that it authorises me to consider it as the type of a new species, for which I propose the name of "Eosite," alluding to its saturate aurora-red colour.

1. *Paragenetic Relations of Eosite.*

The cabinet specimen on which I have found the crystals of eosite is in possession of the Imperial Museum since 1828; it must, therefore, as remarked above, date from the years between 1820 and 1825. Its matrix, abundantly beset with crystals of cerussite, is cellular, ochreous galena. The crystals of cerussite, about 3—8 m.m. in size, bear the aspect of plates, and are greenish yellow; some of them completely fill up a deep cavity in the ochreous matrix, others are scattered on the surface of the specimen. This cerussite, as also some places of the matrix, is in several points covered with small moss-like aggregations of delicate and minute acicular yellow crystals. If such a fascicular aggregation is detached, and the concentrically grouped acicular crystals taken away, their nucleus is found to be a very small red crystal fixed on cerussite. When examined with a powerfully magnifying loupe, the specimen shows about twenty such minute red crystals, more or less scattered over the surface of the cerussite, wrapped up, some of them entirely, others only by half, into the above-described fine yellow acicular crystals. As to be subsequently proved by precise determination, the minute red octahedra are eosite, and the yellow acicular crystals are pyromorphite.

These last crystals, being only ½—1 m.m. in length and about 1-10th m.m. in thickness, could only be determined by the aid of the microscope.

They are yellow, pellucid or transparent, very brilliant, with light yellow streaks. The microscope shows prismatical planes without any distinguishable terminal planes. As previously for my investigations on labradorite, I used for the measurement of the angles of the prismatical planes a vertical circle adapted above the horizontal table of the microscope, whose axis bears the object to be measured beneath the microscope's focus; the crystal fixed to the

* Abstract of a paper read before the Royal Society, May, 1871. Translated by Count A. Fr. Marschall.

† *Poggendorff's Annalen*, vol. iv., p. 169.

axis being duly adjusted and centred, the proceedings go on as usual. Two acicular crystals, duly adjusted, gave as results:—

0°,
60°,
120°,
180°,

from which the planes, including them, must be admitted to be those of a regular hexagonal prism.

Their colour, and even their streak, being light yellow, they may be either mimesite or pyromorphite on vanadate of lead. A drop of chlorhydric acid poured on such a crystal makes its tint vanish gradually without bringing to appearance a dark brown nucleus (as is the case with vanadite of lead), and finally produces a pseudomorph of white chloruret of lead. By heating on charcoal a brown globe with facetted surfaces traces of lead are obtained; the smell of arsenic is very insignificant. Smelting with bisulphate of potash in a platinum spoon gives merely a portion of white saline substance. All these tests were indicative of pyromorphite without any admixture of vanadium; the quantities submitted to operation being, however, very minute, a definitive judgment is not to be pronounced.

2. *Chemical Characters of Eosite.*

The crystals of this mineral are all below ½ m.m. in size, and difficult to be made free from the acicular pyromorphite covering them. They are easily detached from the cerussite, on whose surface they are fixed. Their hardness falls between 3 and 4 (Mohs's scale). When compressed, they are crushed into small, irregular granules, showing slight traces of cleavage.

The colour of the eosite is a saturate aurora-red, even deeper than that of the chromate of lead and approaching the tint of red sulphuretted arsenic; the red tint of the urefinite from Ruskberg (Banat) is less intense, that of the Phenixville specimens has more of yellow in it; the tints of Dechenite and Descloizite are varying between brownish carnation and greenish-brown.

The powder resulting from the friction on a hard surface is brownish-orange in eosite, somewhat darker than that from red chromate of lead.

As the powder from red chromate of lead or of red Wulfenite, that from eosite loses its colour and becomes white by contact with chlorhydric acid. This solution, diluted and spread over a glass plate, shows under the microscope the formation of chloruret of lead in white acicular crystals. A minute splinter of eosite, when put on a glass plate and a drop of cold chlorhydric acid poured on it, undergoes, within a quarter of an hour, a partial solution along its margins, pseudomorphising into white chloruret of lead, the interior still remaining partly unaltered. The solution in chlorhydric acid is slightly tinted with yellow; red chromate of lead or vanadinite are more easily solved, and the tints of their solutions are more intense. Wulfenite, even when used in fragments, ten times larger, loses its colour, and becomes white in far shorter time.

If a splinter of eosite is solved in a drop of slightly heated chlorhydric acid, the solution spread on a glass plate, some alcohol added to it, and the whole again heated to evaporation,* a bluish deposit, somewhat verging into greenish-grey, is obtained. With respect to the quantity of eosite experimented upon, and to the extent of the deposit, its tint may be considered as middle-intense; it is bordered on its margin by a delicate green, somewhat acicular precipitate. Fragments of yellow Wulfenite, vanadinite, or red chromate of lead, of equal size and treated in the same way, give different deposits on the glass plate; Wulfenite, a deep blue one (on account of the formation of molybdate of molybdene), vanadinite, a yellowish or bluish-green one, and crocoïte, a feeble yellowish-green one†. The crystals of eosite being of scarce occurrence and of diminutive size, only few experiments could be made concerning the action of the blowpipe on them.

Eosite, when heated in a glass recipient, assumes a darker tint without decrepitating, and, when cooled, takes again its original colour. A splinter of this mineral mixed with three times its volume of bisulphate of potash and smelted on a platinum lamina, gives, under a glowing heat, a transparent, nearly uncoloured saline mass with a very slight light yellow tint. During its cooling, this mass becomes for a moment reddish brown, and, after complete cooling, assumes a light orange-brown tint.

Comparative experiments made with chromate, vanadate, and molybdate of lead, as also with binary mixtures of the powders of these three substances, proved the colour of the eosite salt to approach very nearly the tint of the saline mass obtained by smelting a mixture of two to three parts of molybdate of lead, and one part of vanadate of lead with bisulphate of potash.

The substance obtained by smelting eosite with bisulphate of potash, brought into contact with water in a platinum spoon, gives a solution which, some tinfoil being added to it and the whole being brought to ebullition‡, assumes a faint greenish-blue tint. A second experiment gave the same result, and proved the solution to give, when further evaporated, a yellowish-brown residuum—possibly caused by the presence of vanadium. Wulfenite, treated in the same way, gave a deep blue tint, vanadinite a light yellowish green, and chromate of lead a light yellowish grey of the saline solution heated in contact with tinfoil.

It would be desirable to ascertain experimentally the presence of other metals besides lead, were not the scarcity of the material an obstacle to any further experiments besides those of absolute necessity.

Considering the whole of the experiments above described, the way in which eosite is acted upon by heating in a glass recipient by chlorhydric acid, alcohol, and bisulphate of potash, proves this mineral to be essentially a vanado-molybdate of lead; possibly with an excess of molybdenum, as it may be supposed from the colourations caused by chemical action, the presence of an undoubtedly minute quantity of chrome being concealed by the chemical action of the prevalent constituents.§

The investigations here detailed, having ascertained the presence of lead, molybdenum, and vanadic acid in eosite, it is still to be proved that the chemical actions of vanadate and molybdate of lead are essentially different from those manifested by eosite, and that consequently this mineral is to be identified neither with the red varieties of Wulfenite actually known, nor with the crystals of Dechenite, vanadite, or Descloizite.

3. *Chemical Properties of the Chromo-Wulfenites.*

The late Professor H. Rose (*Poggendorff's Annalen*, vol. xlvi., p. 639) is known to have investigated the red varieties of Wulfenite from Rézbánya, (Banat), and Siberia, and to have ascertained the presence of chrome in them. I have before me cabinet specimens from Rézbánya, Ruskberg (Banat), and Phenixville, which I shall comprehend here under the general denomination of "Chromo-Wulfenites." The specimens from Rézbánya being rather yellowish than pure red, I must confine my investigations to those from Ruskberg and Phenixville.

* It must be remarked that all these experiments have been made under the microscope, or, at least, under a powerful magnifying lens, so that it was impossible to separate the chloride of lead from the solution containing molybdene.

* As M. Gudowicz has stated (*Poggendorff's Annalen*, vol. cxx., p. 17), the action of alcohol on the chlorhydric solution of vanadine gives rise to precipitates, tinted between blue or brown, according to the degree of oxidation. I also obtained at first, by mixing with alcohol a solution of vanadate of lead, precipitates changing from yellowish green into brown, according to the degree of heat. The exact and constant temperature for the evaporation of alcohol was, however, soon ascertained by a series of experiments.

† The splinters of eosite submitted to chemical action being scarcely ⅓ m.m. in size, the chloruret of lead could not be isolated.

‡ The existence of vanadium, as accidentally entering into the composition of the *genuine* Wulfenites, has been ascertained by Professor Wöhler on occasion of his experiments concerning the production of molybdic acid by the treatment of Wulfenites (see Liebig and Kopp, *Ann. d. Chem. und Pharm.*, vol cii., p. 383.

The matrix of the Ruskberg pyromyrphite* is cellular quartz and galena. A good number of rather bright, deep-red octahedral crystals of Wulfenite, 1—2 m.m. in size, are fixed at the surface of the pyromorphite.

The crystals of Phenixville are notably larger (2—4 m.m.), and concreted into a crust on the surface of the matrix (quartz and pyromorphite). Although apparently of even surface, these crystals show merely a peculiar wax-like brightness.

The red of eosite is more saturated than that of krocoïte; it is somewhat more intense than that of the Ruskberg chromo-Wulfenite, and less mixed with yellow than in the Phenixville specimens. The streak is more brownish in eosite than in chromate of lead. The streaks of the Ruskberg crystals and of those from Berezowsk are quite identical; the powder of the Phenixville crystals is light orange, verging into sulphur-yellow.

The Ruskberg and Phenixville chromo-Wulfenites, treated with chlorhydric acid in the way above described, leave both a deep blue precipitate with a yellowish-green margin. Smelted with bisulphide of potash in a platinum spoon, they give both a saline compound, becoming very faintly yellowish-green after cooling. In the beginning of fusion a brownish purple tint appeared, especially on the Phenixville crystals. This circumstance, not remarked on eosite or vanadate of lead, confirms the fact ascertained by Professor Rose, of chrome being a prevailing component of the red Wulfenites of Banat, with which, according to my observations, I rank also the yelowish-red Wulfenites from Phenixville. According to Mr. Smith, these last contain vanadium; however, the specimen before me, when smelted with bisulphate of potash, had certainly manifested the presence of this metal, were it a prevailing constituent, as chrome really is. At all events, the red Wulfenites may possibly contain a certain proportion of vanadium, as may be expected as to nearly all Wulfenites in consequence of Professor Wöhler's statements.

(To be continued).

ON THE

REDUCTION OF CERTAIN METALS FROM THEIR SOLUTION BY METALLIC SULPHIDES,

AND THE RELATION OF THIS TO THE

OCCURRENCE OF SUCH METALS IN A NATIVE STATE.†

By WILLIAM SKEY,
Analyst to the Geological Survey of New Zealand.

In a paper read by Mr. C. Wilkinson, before the Royal Society of Victoria, "On the Formation of Gold Nuggets," publicity is given to the fact, first observed by Mr. Daintree, that gold, when placed in a solution of its chloride undergoing decomposition by contact with organic matter, determines the deposit of much or all the liberated gold upon itself.

In the same paper, the author states that he finds copper, iron, and arsenical pyrites, galena, zinc blende, stibnite, wolfram, and molybdenite also act as nuclei for gold thus reduced, but that brown iron ore and quartz do not.

The general correctness of these statements has been verified by the results of a critical enquiry, conducted by Mr. Cosmo Newberry, Analyst to the Geological Survey of Victoria; but I notice there has not been any attempt on the part of either of these authors to explain these very singular phenomena.

That gold itself should be nuclear to gold slowly precipitating from its solution is by no means abnormal or unrelated to other phenomena; it is, in fact, just what we should expect from a consideration of the mode of deposit of numerous other substances from their solutions. Whether such deposits are crystalline or amorphous, they generally tend to arrange themselves round a nucleus of their own substance; but, that substances so chemically dissimilar as those specified by Mr. Wilkinson, both as compared among themselves and to gold, should also be nuclear under these circumstances, appeared altogether so strange and anomalous, that I was induced to make further researches into this suhject, for the purpose of ascertaining what other substances (if any) could be added to this list of nuclei rendered, and whether any of them could be nuclear for other metals during their slow precipitation, so that from an enquiry thus extended some general principle might be discovered regulating and explaining such deposits.

The results of these I now beg to lay before this Society. They have been so singular and unexpected, and have taken a direction so different to that contemplated for them, that I have had to change the title of this paper, as first adopted and worked to, into the one now assigned to it.

I found, in the first place, on repeating the experiment adverted to, with certain modifications, that in the case of wolfram the tendency of gold to deposit thereon might properly be referred to the well-known reducing power of soluble proto-salts of iron upon salts of gold, since protoxide of iron forms a considerable portion of this mineral, and is actually dissolved away from it to a small extent by dilute acids at common temperatures—at least I found it so in the case of a specimen of it I had from the Museum here. The case, therefore, so far as these results of Mr. Wilkinson's are concerned, is reduced to one in which there only remains to consider the metallic sulphides and arsenides, a set of metals both chemically and mineralogically related to each other.

Now, in respect to these sulphides, it is distinctly stated in Mr. Newberry's paper that, in even "weak solutions" of terchloride of gold (the salt used in his experiments), "they decompose, but so slowly as not to interfere with the deposit taking place regularly." Having corroborated the correctness of this statement, and also proved that the arsenides are similarly affected, it occurred to me that (though hitherto quite unsuspected) this decomposition was very intimately connected with the first deposit of gold upon these sulphides: that, in reality, the commencement of metallic deposit was effected, not by the interaction of organic matter, as supposed, but by that of the several nuclei themselves with the salt of gold.

I therefore agitated a little finely-powdered galena (sulphide of lead) with a weak solution of terchloride of gold, omitting the addition of organic matter, and taking every precaution against its presence accidentally, when I found, after a little while, the gold solution had become quite colourless, and on testing it not a trace of this metal could be found. It had evidently been absorbed, as it were, by the galena; and, in fact, a careful inspection of this mineral showed it to be feebly gilded.

Small cubes of galena, simply immersed for a few hours in a strong solution of the gold salt without organic matter, were so thoroughly gilded over the greater part of their surfaces that in certain positions they could not be distinguished from gold by the eye.

Chloride of gold was also found to be reduced by contact with the following sulphides (they include, as will be observed, all those mentioned by Mr. Wilkinson):—Sulphides of iron (proto- and bi-sulphide); sulphides of copper (ferro-sulphide and sub-sulphide); the sulphides of zinc, tin, molybdenum, lead, mercury, silver, antimony, bismuth, arsenic, platinum, and gold; and among the arsenides, mispickel and arsenide of silver. Cubical iron pyrites is rather slow in its action upon this solution of gold, while sulphide of antimony scarcely affects it at first, but after some hours' contact with it, reduction goes on rapidly, perhaps by aid of some galvanic action. All these

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* Ruskberg lies in Austrian Banat, next to the threefold frontier between Banat, Transsylvania, and Wallachia.

† Read before the Wellington Philosophical Society.

effects were produced at common temperatures, with the exception of sulphide of bismuth, while other experiments with iron and copper pyrites prove similar effects are produced when all kind of light is excluded, so there is no reason to suppose that light has been concerned in any of these reactions.

I should state that, in the case of some of the highly-coloured minerals, such as cinnabar and arsenide of silver, for instance, it is necessary to operate upon their streak in order to obtain a visible deposit upon them at common temperatures within a reasonable time. With hot solutions, however, even these are well gilded in an hour or two.

A portion of the metal of the sulphide was uniformly found in the solution afterwards, and also sulphuric acid; the mode, therefore, in which these effects were produced was evidently by the oxidation of both the constituents of the nucleus employed.

Thus we have removed this singular anomaly of gold in the act of precipitating, selecting as nuclei substances so diverse as those cited, and while refusing others, which, in reality, differ no more from such non-metallic nuclei than do these from gold; as it certainly appears from these results that, whatever gold has been reduced by organic matter in the experiments quoted, would never deposit on these non-metallic nuclei *surface to surface*, but only upon *gold* already occupying such surfaces, reduced by the exercise of affinities far superior and swifter in their action than those involved in the decay of wood or other organic structures employed by Mr. Wilkinson in the experiments I have alluded to. Being so, therefore, the question next arose—Is organic matter, unnecessary as it certainly is for the commencement of gold deposits on these nuclei, is it necessary to continue them, and to give them that close coherent form obtained when such matters were administered?

Experimentally, it appeared quite unnecessary for this, too, as it was found that the solution, after depositing a certain time, often gave films towards the angles of the nuclei, which were of some thickness, and which under the microscope appeared quite continuous.

The reducing effects of metallic sulphides and arsenides upon salts of gold being thus manifested, it became of some interest to enquire whether or no any other metallic salts were capable of being reduced by these substances.

This part of the subject I have only slightly investigated, but enough has been discovered to show that silver and platinum, and possibly most or all the metals of the platinum series, are reducible in this way from their solutions in acids.

Thus silver is reduced from either its nitrate or acetate very readily by galena, copper pyrites, and the inferior sulphides of iron and copper. From ammoniacal solutions, however, it is not reduced by any of these sulphides, not even when heated with them, excepting by sub-sulphide of copper.

As precipitated by galena, the silver selects some angular portion of it to deposit on, and sometimes strikes off from this in beautiful arborescent form and minutely crystalline filaments, exactly like those which the metal generally assumes in its native state.

Cubic iron pyrites, also stibnite, has little or no effect upon silver salts, not even when heated with them; arsenide of silver has, however, a feeble effect.

The metal platinum is very slowly removed from its bichloride solution by galena and grey copper ore; also by iron pyrites, but at a still slower speed. These were the only sulphides tried.

Mercury does not appear to be reduced to the metallic state by any of the sulphides enumerated from its bichloride solution, but most of them reduce it to the subchloride. Sulphide of gold even thus affects this mercurial salt, the sulphur being oxidised and the gold set free.

Neither the sulphate nor the acetate of copper are affected by these sulphides; but perchloride of iron is reduced to the protochloride by galena and grey copper ore.

Before I proceed to the next part of the subject, I had better state here the results I obtained when other gold solutions were administered to these sulphides, the one hitherto employed in the experiments described being, as will be remembered, the chloride.

When, in the place of this salt, I applied the *oxide* (of gold) in solution of either potash, bicarbonate of soda, or an alkaline silicate, the same reduction of the metal followed—at least, this was the case for galena and the inferior sulphides of iron and copper. If the oxide is dissolved in ammonia, such results require for their production an elevation of the temperature of the solution to about 200° F.

In the case of the sulphide of gold, however, whether dissolved in any of these salts or alkalies, it was found this compound could not be reduced by contact with any of these sulphides, either at common temperatures or at the boiling-point of the particular solution employed, nor yet when strong deoxidising agents, such as tannic or gallic acids, were used along with such gold solutions.

All this tends to show, I think, that in the case where Mr. Newberry obtained the reduction of gold upon iron pyrites from a solution of its sulphide in bicarbonate of soda mixed with organic matter, the gold had, prior to the act of reduction, become in some way divested of its sulphur, and had taken up oxygen in exchange, thus passing to a salt readily reducible by deoxidising agents.

During the period of time which had to be allowed by Mr. Newberry for his experiment the oxygen of the air might have oxidised the sulphur of the auriferous sulphide, as it does that of several other metallic sulphides, and so rendered him a gold compound amenable to the influence of deoxidising substances; any way, it is inconceivable how organic matter and metallic sulphides, singly or conjointly, can desulphurise a gold sulphide. The effect of organic matter in a state of decay is rather to generate sulphides than to decompose them, thus retarding in the place of promoting the reduction of such compounds of gold. It is hard to suppose there could have been any chemical interchange effected by the mere addition of bicarbonate of soda to the gold sulphide, since gold has far more affinity for sulphur than for oxygen—it could hardly pass to an oxy-salt, therefore, in this manner; besides, if it could, then reduction should have proceeded as well with this kind of solution as with the gold oxide in solution of bicarbonate of soda, which, as before stated, I found it did not.

Further experiments in this direction are, however, absolutely necessitated by the importance of ascertaining positively whether there is any solution of gold (likely to occur naturally) able to resist the reducing power of either metallic sulphides or organic matter in a state of decay.

The several results arrived at in these investigations are now stated; it only remains, therefore, to point out, or, rather, to refer, to the very obvious relation they appear to sustain to the manner in which certain of our native metals are frequently associated.

The great deoxidising power of sulphides generally upon most gold, silver, and platinum salts, as manifested by the experiments just described, renders them so absorbent, as it were, of these metals, that any such solutions traversing a very thin vein even or reef of the mixed metallic sulphides would, in all probability, be completely divested of these metals.

Solutions of silver, however, would be little, if any, affected in traversing a reef of either iron pyrites or stibnite; but if, in addition to silver, the solution contained gold, it is very probable a certain portion of the silver would precipitate along with the gold as the result of a secondary action, that is, by a simple chemical substitution. On the other hand, silver would readily be absorbed by veins of galena or inferior sulphides.

All this certainly agrees very well with what we find on examination of these sulphides; indeed, the facts just

stated seem to explain very simply the constant or the frequent association of gold and silver with certain of our metallic sulphides, and their absence or comparative scarcity in others. Whether or no this is the reason of such association, the knowledge that certain metallic sulphides and arsenides are capable of reducing these metals from their solutions should, I think, be taken into careful consideration when we attempt to explain the origin of these metals in the forms they have taken and in the rocks or veins they have selected.

As yet, every theory of any general acceptance which has been broached to explain the occurrence of such metallic deposits in these matrices is based upon the reducing action of organic matter, when the fact is that most of the common sulphides are much superior in this respect to such matters, being far more rapid in their effects, and capable of reducing, weight for weight, more metal—a single grain of iron pyrites being sufficient to reduce $8\frac{1}{6}$ grains of gold.

While, therefore, allowing that organic matter has had a share in the reduction of gold and other metals, I cannot but think that by far the greater portion of our gold and silver deposits, especially those situated in the deeper-seated rocks and lodes removed from carboniferous strata, have been wholly due to the deoxidising effects of pyritous minerals.

Of course both theories require, as a common ground, that the metal shall first be in such a state of combination as to be soluble in water, a condition which is generally conceded to it, and which I have, therefore, taken all along as actually obtaining.

I cannot take leave of this subject without adverting to and commenting upon some of the singular chemical reactions which these researches have opened upon.

In the first place, it has appeared that aqueous solutions of oxy-salts of gold, among which we must include the chlorides, for obvious reasons, possess oxidising power to an extent not hitherto contemplated for them, great as this may have been acknowledged. I think it is a little superior to that of chromic acid, since it attacks cubical pyrites pretty readily in the cold, while chromic acid scarcely affects it.

Further, these salts of gold not only oxidise the metals of these sulphides, but their sulphur too, although I find, from direct experiment, neither common (sublimed) sulphur, nor yet sulphur precipitated from solution in alkalies are the least affected by them, not even when these auriferous solutions are raised to their boiling-points.*

This vulnerability of combined sulphur, as against the invulnerability of the same substance when free, suggests the greatness of the change which sulphur in itself undergoes when it passes to a state of combination.

Possibly some voltaic action facilitating this change may be set up by these several substances among themselves, in which the gold already reduced would form the negative element, but this does not, of course, explain how the oxidation commenced.

DETERMINATION OF ALKALIES IN MINERALS, &c., BY IGNITION WITH CARBONATE OF LIME AND SAL-AMMONIAC.†

By J. LAWRENCE SMITH.

(Concluded from page 223.)

12. *Method of Analysis.*—We have now the pure carbonate of lime, granular sal-ammoniac, and the proper crucible, The silicate is to be well pulverised in an agate mortar;‡ for the analysis I take ½ grm. or 1 grm., the former is most commonly used, as being sufficient, and best manipulated in the crucible used; a gramme, however, may be conveniently employed. The weighed mineral is placed in a large agate mortar, or better in a glazed porcelain mortar, of ½ to 1 pint capacity. Weigh out an equal quantity of the granular sal-ammoniac (a centigramme more or less is of no consequence), put it in the mortar with the mineral, rub the two together intimately; after which add eight parts of carbonate of lime in three or four portions, and mix intimately after each addition; empty the contents of the mortar completely upon a piece of glazed paper, that ought always to be under the mortar, and introduce into the crucible. The crucible is tapped gently upon the table and the contents settled down.

13. It is then clasped by a metallic clamp in an inclined position, or it is placed in the support referred to in the latter part of this article, leaving outside about ½ or ¾-inch; a small Bunsen burner is now placed beneath the crucible, and the heat brought to bear just about the top of the mixture, and gradually carried toward the lower part, until the sal-ammoniac is completely decomposed, which takes place about four or five minutes; heat is then applied in the manner suggested, either with the blast or with the burners referred to, acting by its own draught, and the whole kept up to a bright red heat, for about from forty to sixty minutes. It is well to avoid too intense a heat, as it may vitrify the mass too much.

14. The crucible is now allowed to cool, and, when cold, the contents will be found to be more or less agglomerated in the form of a semi-fused mass; a glass rod or blunt steel point will most commonly detach the mass, which is to be dropped into a platinum or porcelain capsule of about 150 c.c. capacity and 60 or 80 c.c. of distilled water is added. In the course of a longer or shorter space of time, the mass will slake and crumble after the manner of lime; still better, this may be hastened by bringing the contents of the capsule to the boiling-point, either over a lamp or water-bath. At the same time, water is put into the crucible to slake out any small particles that may adhere to it, and subsequently this is added to that in the capsule, *washing off the cover of the crucible also.*

15. After the mass is completely slaked, the analysis may be proceeded with, although, as a general thing, I prefer to allow the digestion to continue six or eight hours, which, however, is not necessary. If the contents of the crucible are not easily detached, do not use unnecessary force, as the crucible may be injured by it, but fill the crucible to about two-thirds of its capacity with water, bring almost to the boiling-point, and lay it in the capsule, with the upper portion resting on the edge; the lime will slake in the crucible, and then may be washed thoroughly into the dish, and, as before, the cover is to be washed off.

16. We have now by this treatment with water the excess of lime slaked into a hydrate, and some of the lime combined with the silica and other ingredients of the silicate, in an impalpable form; in solution there is the excess of the chloride of calcium formed in the operation, and all the alkalies originally contained in the mineral as chlorides, and all that now remains to be done is to filter, separate the lime as carbonate, and we have nothing left but the chlorides of the alkalies. To do this, I proceed as follows:—

17. Throw the contents of the capsule on a filter (the size preferred for the quantity above specified is one 3 to 3½ inches in diameter), wash well, to do which requires about 200 c.c. of water; the washing is executed rapidly. The contents of the filter (except in those cases where the amount of the mineral is very small, and there is no more for the estimation of the other constituents) is of no use, unless it be desired to heat again, first adding a little sal-ammoniac to see if any alkali still remains in it, a precaution I find unnecessary. The filtrate contains in solu-

* Chromic acid and *cold* nitric acid have the same effect upon sulphide of gold, but have none upon these forms of sulphur.

† From "Select Methods in Chemical Analysis," by W. Crookes, F.R.S., &c. London: Longmans.

‡ While in all mineral analyses thorough pulverisation of the mineral is usually essential, still it is a singular fact that very good analyses can be made with this method, even when the powder is tolerably coarse, and in some experiments with lepidolite, powder was used with much of it in particles from 1-40th to 1-30th of an inch in size, giving good results. Notwithstanding this, thorough trituration of the mineral is recommended.

tion all the alkalies of the mineral, together with some chloride of calcium and caustic lime; to this solution, after it has been placed in a platinum or porcelain capsule is added a solution of pure carbonate of ammonia; (equal to about 1½ grms. is required).

18. This precipitates all the lime as carbonate; it is not, however, filtered immediately, but is evaporated over a water-bath to about 40 c.c., and to this we add again a little carbonate of ammonia and a few drops of caustic ammonia to precipitate a little lime that is re-dissolved by the action of the sal-ammoniac on the carbonate of lime. Filter on a small filter (2 inches), which is readily and thoroughly washed with but a little water, and the filtrate allowed to run into a small beaker glass. In this filtrate are all the alkalies as chlorides and a little sal-ammoniac; add a drop of a solution of carbonate of ammonia, to make sure that no lime is present. Evaporate over a water-bath in a tared platinum dish, in which the alkalies are to be weighed; the capsule used is about from 30 to 60 c.c. capacity, and during the evaporation is never filled to more than two-thirds its capacity.

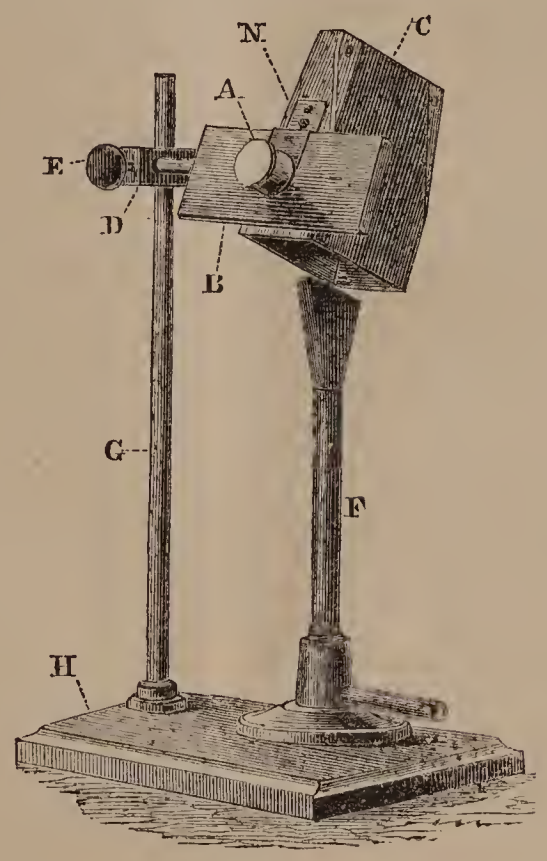
19. After the filtrate has been evaporated over the water-bath to dryness, the bottom of the dish is dried, and on a proper support heated *very gently* by a Bunsen flame to drive off the little sal-ammoniac. It is well to cover the capsule with a piece of thin platinum to prevent any possible loss by the spitting of the salt after the sal-ammoniac has been driven off. Gradually increasing the heat, the temperature of the dish is brought up to a point a little below redness, the cover being off (the cover can be cleansed from any sal-ammoniac that may have condensed by heating it over the lamp). The capsule is again covered, and, when sufficiently cooled, before becoming fully cold, is placed on the balance and weighed. This weight gives as chlorides the amount of alkalies contained in the mineral.

20. If chloride of lithium be present, it is necessary to weigh quickly, for this salt being very deliquescent attracts moisture rapidly.

It not unfrequently occurs that the chlorides at the end of the analysis are more or less coloured with a small amount of carbon, arising from certain constituents in carbonate of ammonia; the quantity is usually very minute, and in no way affects the accuracy of the analysis. In selecting pure carbonate of ammonia for analytical purposes, it is well to select specimens that are not coloured by the action of the light.

21. It only now remains to separate the alkali by the known methods. Under this head I have made several observations that will be published as soon as they are considered satisfactory.

22. *A Special Arrangement for Heating the Crucibles by Gas.*—The support and burner, where gas is to be had (as it is in almost all analytical laboratories) are simple in their character, and have been arrived at after a great variety of experiments with gas furnaces. The figure here given illustrates the stand, burner, crucible, &c.,* and is about one-sixth the natural size. H is the stand with its rod, G. D is a brass clamp, with two holes at right angle to each other; having two binding screws, it slides on the rod, G; the second hole is for a round arm attached to B, the binding screw, E, fixing it in any position. B is a plate of cast-iron, 5 to 6 m.m. thick, 10 to 11 centims. long, and 4½ centims. broad, having a hole in its centre large enough to admit the crucible to within about 15 m.m. of the cover without binding. A is the crucible already referred to, which is made to incline a few degrees downwards, by turning the plate of iron that supports it. C is a chimney of sheet-iron, 8 to 9 centims. long, 10 centims. high, the width at the bottom being about 4 centims. at one end, and about 3 centims. at the other end. It is made with the sides straight for about 4 centims., then inclines towards the top, so as leave the width of the opening at the top about 1 centim. A piece is cut out of the front of the chimney of the width of the diameter of the hole in the iron support, and about 4 centims. in length, being semicircular at the top, fitting over the platinum crucible. Just above this part of the chimney is rivetted a piece of sheet-iron in the form of a flattened hook, N, which holds the chimney in place by being slipped over the top of the crucible support; it serves as a protection to the crucible against the cooling effects of the currents of air. F is the burner, which has been described in an article on flame heat in the *American Journal of Science*, November, 1870, p. 341, the upper opening of which is a slit from 1½ to 2 m.m. in width, and from 3 to 4 centims. long, and when used is brought within about 2 centims. of the lowest point of the crucible, the end of the flame just playing around the lower end of the crucible. The gas enters the lower part of the burner by two small holes of 1-16th of an inch, furnishing at 1-inch pressure about 5½



cubic feet of gas per hour. The precaution must be observed, already referred to, in heating the crucible a first gently (13).

23. It is surprising to see the effect produced by this simple burner as here used; 8 grms. of precipitated carbonate of lime can be decomposed to within 2 or 3 per cent in one hour, and when mixed with silica or a silicate, in a very much shorter space of time, although in my analyses I employ one hour, as it requires no attention after the operation is once started. This form of furnace and crucible is found to be convenient for other operations.

24. Although the details given here are lengthy, the time occupied in the analysis is short and the precautions necessary are of a simple character, so much so that results obtained by students on beginning chemical analysis have been found by me reliable, and less variable on the alkalies of the silicates than of any of the other constituents of this class of bodies. I have often made good alkali determinations in three hours from the commencemnt of the operations, hastening the evaporations by more direct application of heat, which of course, requires close watching.

25. It is a common practice, when a silicate comes under my examination that it is not easily made out by its physical properties, to make at once an alkali determination, which often indicates at once what it is if it be a known silicate. If not, the alkali determination serves as one step in its examination.

* These burners and stands, as well as the platinum crucibles, made according to Professor Lawrence Smith's pattern, can be obtained of Messrs. Johnson and Matthey, Hatton Garden.

St. Mary's Hospital.—We are glad to learn that Dr. C. R. A. Wright has been appointed Lecturer on Chemistry and Practical Chemistry *vice* Dr. Russell, who is now the Professor of Chemistry at St. Bartholomew's Hospital.

ON SOME OF THE

CONDITIONS OF LOSS BY VOLATILISATION IN CERTAIN METALLURGICAL OPERATIONS.*

By CHARLES P. WILLIAMS,
Professor of Chemistry, &c., Delaware College.

IT is a well recognised fact, pointed out by an expensive experience, that in all methods of lead smelting and in oxidising and chloridising roastings generally, a very considerable loss of metals results. This loss is brought about either by the formation of more or less volatile compounds, or by the draught of the furnace carrying, mechanically suspended, particles of chemically changed ore, with the products of combustion, or, in the case of muffle furnaces, with the atmosphere sweeping over the ore therein treated. The problem of devising an economical and complete method of collecting these metallic vapours or fumes is one of very considerable difficulty, and it may be fairly questioned if it has yet received a satisfactory solution. In this country, where rich lead ores are abundant, and where all the elements, except labour, entering into the metallurgical treatment of such ores are comparatively cheap; this matter of condensation and collection has not generally forced itself upon the attention of practical metallurgists, but it is, nevertheless, of considerable significance.

In the treatment of silver-bearing ores by smelting operations, or where a previous chloridising roasting is necessary, the loss of silver by volatilisation must be of significant importance, and will yet, if it has not already, give rise to a study of the means adapted to its prevention. Various European experimenters have given different figures for the loss of silver by volatilisation in different methods of its extraction, but these figures may safely be averaged as ranging between 0·5 and 4 per cent of the content of the original ore. With regard to chloride of silver it is well established that its volatility is greatly increased by the presence of other chlorides. Plattner's experiments on the Augustin method of silver extraction† show that a mixture of 10 parts of oxide of copper, 3 of chloride of lead, and 0·6 of fused and finely-divided chloride of silver, lost, on roasting in a current of air, 6·61 per cent of the original weight by volatilisation. The sublimate, on quantitative examination, gave 63·8 per cent chloride and oxide of lead (containing 54·8 of metallic lead), 32·8 per cent of cuprous chloride (containing 21 of copper), and 3·4 of argentic chloride (containing 2·6 of silver). These figures would give 5·09 as the percentage of the loss of the chloride of silver contained in the original mixture, partly due to the volatility of chloride of silver itself, but chiefly brought about, however, by the presence of other chlorides. In the oxidising roasting of sulphuretted compounds of silver, the loss is also great, through the volatilisation of the sulphate, &c., of themselves, or increased by the presence of compounds of other metals, or through the mechanical movement of fine particles of the ore by the draught through the furnaces.

In the treatment of galena for the extraction of lead, whether by the process of reaction or by other means, a large amount of the metal is lost, chiefly in the forms of sulphate or of oxide. This loss, though admittedly large, is not fully appreciated, and is, without doubt, due, not merely to the volatility of the oxide of lead (this being partially changed into sulphate), but also to the facts that sulphate of lead is volatile, and that sulphide of lead is readily and rapidly volatilised at elevated temperatures and is subsequently converted into sulphate either through the action of the oxygen of the air or through that of sulphuric acid. This latter production of sulphate is well established by the researches of Plattner,‡ and his observation of pseudomorphous crystals of plumbic sulphate after sulphide of lead, which had formed after the condensation of the latter crystals from a vaporous condition. The writer of this communication has invariably observed during repeated exhaustive analyses of the so-called Bartlett white-lead, and of oxide of zinc produced at the Keystone Zinc Works, the presence of sulphide of lead. Both of these substances are essentially mixtures of oxide of zinc and plumbic sulphate obtained by the treatment of mixed galena and blende by the well-known process for the manufacture of zinc-white directly from the ores. On treatment with hyposulphite of soda, the sulphate of lead is readily dissolved, whilst a subsequent addition of dilute acetic acid will dissolve the remaining ingredients, leaving a small amount of dark metallic particles, which, on chemical and microscopic examination, are found to be minute crystals of sulphide of lead.

Mr. G. T. Lewis, a well-known manufacturer of Newark, Del., has repeatedly completely volatilised several hundred pounds of the purest galena obtained in the Wisconsin lead region. The products of these treatments (which were chiefly conducted in the Wetherill furnace for the manufacture of zinc-white), were collected by the "bag process," and have been analysed by the author. They are mixtures of sulphate of lead (commonly about 60 per cent), with some oxide of lead and zinc, and carbonate and sulphite of lead, and invariably contain small and variable amounts of sulphide of lead.* From these experiments the high degree of volatility of this last-named compound must be admitted, unless it be claimed that it is first converted into sulphate, and that this salt is even more volatile than the sulphide. The author has made efforts to establish the degree of volatility of the sulphide of lead by strongly heating the artificially prepared salt in currents of nitrogen and hydro-sulphuric acid, but has found so many difficulties in the preparation of a perfectly dry and unaltered compound, that he cannot, as yet, regard his results as removed beyond sources of fallacy,

Plumbic sulphate is also to a slight degree volatile. Some of the perfectly dry and pure artificially prepared salt was strongly heated for one hour, in a porcelain tube through which a current of dry air freed from carbonic acid was drawn. Thus treated, 1·4082 grms. of the sulphate lost in weight 0·0019 grms., equivalent to 0·134 per cent. A second experiment with 4·2761 grms., but heated for upwards of two hours, showed a loss of 8·5 milligrammes, or a little less than 0·2 per cent. Other trials showed variable losses, but in no instance was the percentage amount greater than that obtained in the second experiment. Examined with a glass, indications of fusion of the sulphate were noticed. That this method of producing sulphate of lead in lead smoke or fumes is of much moment compared with that of the volatilisation and subsequent oxidation of the sulphide, can hardly be claimed, though it, doubtless, is one of the many causes of the loss of lead in metallurgical operations.

Another cause of loss is the mechanical one brought about by the currents of air, or of the products of combustion sweeping through the furnaces. In furnaces of that description, where the fuel is intermingled with the ore under treatment, or where the gases, vapours, &c., from the fuel in the fire-box pass over and in contact with the charge, this mechanical loss is, doubtless, increased by the presence of fuliginous particles of finely-divided carbon, which enclose, and, as it were, buoy up, the denser particles of metallic compounds. Where the metallurgist has to deal with lead ores which contain zinc compounds —by no means an unusual condition of affairs—it will be admitted that the formation of zinc vapours and their subsequent conversion into the specifically light oxide of zinc, will add greatly to the loss of lead as sulphide or as oxide by assisting mechanically in carrying these compounds from the furnace in a manner similar to that in

* From advance-sheets of the *Journal of the Franklin Intsitute.* Communicated by the Editors.

† *Berg-und Huttenmänische Zeitung e*, 1854, p. 125 *et. seq.*

‡ *Procedes Metallurgiques de Grillag e*, p. 207 *et seq.*

* The method of analysis followed in these investigations was essentially that described by the author in the *Journal of the Franklin Institute*, for March, 1869.

which the sooty particles of carbon may act. The volatility of silver as oxide or as sulphate may be fairly assumed as increased in this same manner. The experiments of Malaguti and Durocher,* made with argentiferous zinc blende, indicate that the loss of silver in the presence of zinc compounds may be very considerable, reaching as much even as 70 per cent of the original content of the ore, but ranging usually according to the richness of the ore in silver and in zinc, and according to the management of the furnaces, between 15 and 60 per cent.† A small proportion of this loss is unquestionably due to the volatility of the sulphate of silver, but, as in the case of ores not carrying zinc, and not highly siliceous, the loss is less than 10 per cent,‡ it must be admitted that zinc compounds dispose silver to volatilise.

The ores mined in the Silurian lime-stones of Sinking Valley are chiefly intimate mixtures of zinc blende and galena (the latter forming usually about 20 per cent of the mixture), and contain on the average, by the assays of the author, about 5 ounces of silver to the ton of 2000 lbs., though Ashmead finds|| by his assays 8·5 and 9 ounces. These ores are treated, at the Keystone Zinc Works, for the manufacture of zinc-white by the ordinary process, and in the residues on the perforated grate bars of the furnace, the writer has never been able to find weighable quantities of silver, though operating repeatedly on amounts of 58½ grms. By dissolving a weighed amount of the so-called "oxide" made from this ore and collected in the "bags" in nitric acid, evaporating to dryness, adding hydrochloric acid, extracting with water, and assaying the residue in the usual manner, 0·014 per cent of silver may be found, corresponding to 4·2 ounces to the ton of "oxide." The "Bartlett white-lead," prepared in a similar manner from mixed blende and gelena, mined in Davidson county, North Carolina, yields a somewhat smaller percentage of silver (0·0087 per cent) though the original ore is more highly argentiferous than that utilised in Pennsylvania. This discrepancy may be accounted for, however, by the fact that the North Carolina ore is roasted (with the addition of salt?) at the mine before shipment to New Jersey for its conversion into the basis of a pigment.

The following analyses will exhibit the composition of lead fumes by various operations:—

	I.	II.	III.	IV.	V.	VI.
ZnO	72·083	73·426	49·50	13·80	25·70	9·23
PbO	0·274	—	27·90	10·20	48·30	13·21
SbO_3	trace	—	AsO_3 2·10	—	3·90	—
Fe_2O_3	trace	—	—	3·40	—	trace
$PbOSO_3$ ..	23·968	25·084	13·00	66·60	14·40	74·05
$ZnOSO_3$ ..	0·810	0·574	—	PbS 1·40	—	trace
ZnCl	0·839	—	—	—	—	—
Fe_2Cl_3	0·071	—	—	—	—	—
$SbCl_3$	trace	—	—	—	—	—
CdCl	0·256	—	—	—	—	—
$CdOSO_3$ ..	—	0·187	—	—	—	—
$CaOCO_2$ ..	—and loss	0·729	CO_2-7·00	—	4·50	3·27
Clay	—	—	—	5·60	—	SO_2 0·84
Per cent Pb. in above ..	16·624	17·132	34·778	56·169	54·571	62·840
Ag.	0·0087	0·014	—	—	—	0·0019

Analyses I., II., and VI. are by the author; III., IV., and V. are from Watts's "Dictionary of Chemistry," Art. "Lead." No. I. is the so-called Bartlett White Lead (*Journal of the Franklin Institute*, March, 1869); II. is the zinc-white from the ores of Sinking Valley, Pennsylvania; III., fumes from blast furnaces at Freiberg; IV., fumes from reverberatory furnace at Alston Moor; V., from refinery at Freiberg; VI., from Wisconsin ores treated at Birmingham, Pennsylvania, in the Wetherill furnace for the manufacture of zinc-white; Nos. I. and II. were also obtained by treatment in the same furnace. The three analysed by the author all contained sulphide of lead, but the amounts were small and were not estimated.

Estimates based upon the treatment of a large amount of ores have given the following figures, making evident the loss in the treatment of lead ores by conversion into merchantable lead, though unfortunately no descriptions of the characters of the ores are furnished. A blast hearth furnace treating 267,008 pounds, yielding by assay 75·75 per cent, or 202,258 pounds, gave 178,895 pounds or 67·00 per cent, exhibiting, therefore, a loss of 23,363 pounds. The refining process gave a further loss of 13·40 per cent, whilst the reduction of the dross from the refinery added a still further loss of 3·60 per cent, giving an aggregate of more than 28½ per cent of the original content of lead.*

If considerations of the health and property of a neighbourhood are only of subordinate importance, such figures must make evident the necessity of connecting with lead establishments, suitable apparatus for the condensation and saving of this fume. The process of straining the fumes through muslin or fine canvass, which gives such successful results in the collection of zinc fume or oxide as exemplified by the well-known "bag process," has not received from metallurgists the attention its simplicity and completeness merit. From experiments made on a large scale on galena ores, both nearly pure and containing a large amount of blende, the writer is satisfied that its application to the condensation of lead and other metallic fumes would leave little to be desired. It seems strange that it has not been applied to purposes other than the mere saving of oxide of zinc, especially when the necessity of a thorough collection of metallic fume is considered. Its satisfactory use in the treatment of sulphuretted ores, it is true, would be somewhat lessened by the fact that the fabric of the bags would be rapidly destroyed by sulphuric acid, &c.; but even this difficulty might be obviated by washing the mixed gases and metallic vapours by conducting them into a brick chamber through which water in the form of rain is falling. The water from this chamber might be run off through caustic lime to remove soluble metallic salts. The ordinary blower working in the flues between the zinc furnaces and the bags would prevent any injury to the draught of the furnace.

A discussion of the various mechanical devices and combinations already in use in England and elsewhere, for preventing or reducing the loss by volatilisation at lead furnaces, is not deemed necessary in this connection. The results of Von Patera's experiments† made on a small scale, and having in view the saving of argentiferous fume through chemical reagents and reactions, may yet be found to contain the germs of important technical applications. Especially may this be true of those obtained by the use of sulphydric acid, whereby waste sulphurous acid may be converted into sulphur, and a complete saving of volatilised silver and lead compounds be effected.

* *Annales des Mines*, xvii., 1850.
† Plattner, *loc. cit.*, p. 118.
‡ Kerl's *Handbuch der Huttenkunde*, i., 2nd edition, p. 89.
|| *American Chemist*, vol. i., No. 4, p. 134.

* These figures are condensed from the elaborate tables in Watts's "Dictionary of Chemistry."
† *Zeitschrift für Berg und Hüttenwesen*, 1854, p. 33.

CORRESPONDENCE.

PRODUCTIVE POWERS OF SOILS IN RELATION TO THE LOSS OF PLANT FOOD BY DRAINAGE.

To the Editor of the Chemical News.

Sir,—The few remarks I made on the subject of Dr. Voelcker's lecture at the Chemical Society have been, to some extent, imperfectly reported in your Journal. With regard to sewage-irrigation, I did not argue against its utility, but against the probability of the immense

return in produce from its use which some authorities lead us to expect; and with respect to the retention of nitrates by soil, I did not believe there was any evidence that nitrates were well retained by soil, but simply that some facts seemed to show the existence of a small power of retention, which had hitherto been overlooked.—I am, &c.,

R. WARINGTON.

MISCELLANEOUS.

A Manual of Organic Chemistry, by Dr. Henry E. Armstrong, F.C.S., Professor of Chemistry in the London Institution, is advertised by Messrs. Longmans and Co., as being in preparation for their series of Text-books of Science.

Alexander's Air Purifier.—This is an ingenious arrangement for disinfecting sick chambers, hospital wards, &c., with carbolic acid, an alcoholic solution of sulphurous acid, or disinfecting powder. During illness and infectious diseases, the purifier is hung in each room, passage, cupboard, and wardrobe, and a regulator is opened so as to allow the odour of the disinfectant to be distinctly perceived.

Pharmaceutical Conversazione.—The annual conversazione of the Pharmaceutical Society was held on Wednesday evening last at the South Kensington Museum. About 3000 visitors were present. In addition to the attractions of the museum, a selection of instrumental music was performed by the band of the Grenadier Guards, and the Orpheus Glee Union, under the direction of Mr. Fielding, contributed to the pleasure of the evening by singing several glees.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Annalen der Physik und Chemie, von Poggendorff, No. 2, 1871.

This number contains the following original papers and essays more strictly belonging to chemical and collateral sciences:—

Optico-Experimental Researches.—Dr. G. Quincke.—The continuation of a lengthy memoir. This part contains the following chapters:—On the change of the phase during the refraction of the waves of light; on the change of the phase during the reflection of the waves of light; reflection in glass. The first portion of this monograph, not yet concluded, was published in the above-named periodical in 1866.

Power of Electro-Magnets to Carry Weights.—Prof. A. von Waltenhofen.—The contents of this paper cannot be well understood without the reproduction of the accompanying engravings.

A Curious Fountain.—M. H. Vogelsang.—This lengthy paper contains the record of a curious occurrence which took place near Delft (Holland). It appears that, in order to obtain fresh water for domestic purposes, a so-called Norton pump-tube was being rammed down in the alluvial soil (peat-bog chiefly). When at a depth of 17·5 metres below the surface, and very nearly as much below sea level at that locality, the ram-block was suddenly lifted by a mass of gas issuing from the opening of the iron tube, immediately followed by water, which rose to a height of 14 metres, and continued to do so for some fourteen hours; after that lapse of time, the fountain became for a few weeks intermittent. The temperature of the water was 13°. The composition of the gas, in 100 parts, by bulk, was found, by Dr. A. C. Oudemans, jun., to consist of 91·2 of marsh gas and 8·2 of carbonic acid, with traces only of air, while carbonic oxide and heavy hydrocarbons were proved to be absent. The phenomena alluded to ceased when, after some weeks, the iron pump-tube was forced down to a depth of 25 metres.

Theory of Marine Currents.—Dr. von Witte.

Researches on Alcoholic Fermentation, and on the Nutrition of the Saccharomyces cerevisiæ.—Dr. A. Mayer.—This paper contains the results of a series of physiologico-chemical researches on the development, functions, growth, and composition of a peculiar kind of cryptogamic vegetable (*Saccharomyces cerevisiæ*), which the author considers to be the cause of fermentation.

Combination of Acetate of Soda with Water.—Dr. E. Zettnow.—The main result of the author's researches is that acetate of soda is capable of combining with very variable quantities of water, viz:—

$C_4Na_2H_6O_4+6H_2O$ = 39·70 per cent of water.
$C_4Na_2H_6O_4+5H_2O$ = 35·40 „ „
$C_4Na_2H_6O_4+4H_2O$ = 30·51 „ „
$C_4Na_2H_6O_4+3H_2O$ = 24·77 „ „
$C_4Na_2H_6O_4+2H_2O$ = 18·00 „ „
$C_4Na_2H_6O_4+1H_2O$ = 9·89 „ „

Barometer without Mercury.—A. Heller.—Without the reproduction of the engravings added to this lengthy essay, it is not possible to give an account of the complex apparatus here alluded to.

Reversion of the Sodium Line.—M. A. Weinhold.

Observations on the Asteric Figures met with on Certain Crystals.—Dr. H. Baumhauer.

Critical Review of a Paper ("Sur les Crystallites") by Dr. Vogelsang.—Dr. E. Weiss.—Illustrated with engravings.

Pharmaceutische Zeitschrift für Russland, No. 1, 1871.

This number contains only the following original paper bearing upon chemistry:—

Nutritive Value of Extract of Meat.—Dr. J. von Liebig.—This paper has been written by the author specially as a reply to a short note on extract of meat, published in a German newspaper by a Russian, Dr. Beljawsky, who placed extract of meat in the category of various nostrums, to which applies the "*mundus vult decipi.*" The eminent author vindicates his character, and gives a well-deserved rebuke to the party just named. The paper does not, however, contain anything new on the subject.

Nos. 2, 3, and 4, 1871,

Do not contain any original papers relating to chemistry.

No. 5, 1871.

This number contains the following original paper:—

Quantitative Estimation of Arsenious Acid by means of Marsh's Apparatus.—Dr. J. Trapp.—This paper strictly treats on a method by means of which it is possible to exhibit in courts of law the presence of arsenic in cases where the detection of this substance may be required for forensic purposes; while the author sets also forth how, approximatively, some idea may be given of the quantity of arsenic which was present in either stomach, liver, spleen, or other organ. The contents of the paper bear, however, entirely upon the criminal mode of procedure in Russia.

Neues Jahrbuch für Pharmacie, von Dr. F. Vorwerk, March, 1871.

This number contains the following original papers more strictly relating to chemistry:—

Simple Method for the Preparation of Mercurous Chloride (Calomel).—Dr. Uloth.—This paper records a series of experiments made by the author for the purpose of testing the fitness of the preparation of calomel by the wet way, by causing oxalic acid to act upon mercuric chloride (corrosive sublimate), whereby, provided the operation is assisted by good sunlight, there is formed carbonic and hydrochloric acids and mercurous chloride, according to the formula $2HgCl+C_2O_3,HO=2CO_2+Hg_2Cl+HCl$. The conclusion come to by the author is that this method—although attended with some loss, because, practically, there is not obtained as much calomel as there should be according to theory—is yet available in practice, since, with due care, the resulting calomel is very pure, while a real loss of mercury is not actually sustained, some of it being recovered as oxide.

Journal für Gasbeleuchtung und Wasserversorgung, Nos. 6 and 7, 1871.

These numbers do not contain any other papers than those strictly bearing upon gas and waterworks engineering.

Zeitschrift für Chemie von Beilstein, No. 4, 1871.

Some of the Derivatives of Camphoric Acid.—Dr. F. Wreden.—This paper treats on the following substances:—Anhydride of bromcamphoric acid, $C_{10}H_{16}O_4+Br_2=C_{10}H_{13}BrO_3+HBr+H_2O$. The body thus obtained is more difficultly soluble in ether than camphoric acid anhydride; it crystallises from its alcoholic solution in needle-shaped crystals, is readily soluble in chloroform, and becomes blackened when heated to 100°. Oxycamphoric acid anhydride, $C_{10}H_{14}O_4$, obtained from the above-mentioned body by boiling with water, fuses at 201°, is soluble in ether, alcohol, and water, and forms salts with bases, most of which are crystalline; the cadmium salt, $Cd(C_{10}H_{13}O_4)_2.3H_2O$, yields prismatically-shaped crystals, and loses at 100° all the water it contains. The ether of this acid, $C_{10}H_{13}(C_2H_5)O_4$, obtained by saturating the solution of the anhydride in absolute alcohol with HCl, is

crystalline solid body, fusing at 63°, and soluble in alcohol, ether, chloroform, and sulphide of carbon. A hydrocarbon, C_8H_{14}, obtained from the anhydrous acid by distillation with lime, was found to have a vapour density=3·75, a sp. gr.=0·814 at 0°, and to exhibit a smell of oil of turpentine and camphor, mixed.

Nitrited Ortho-Toluidine.—Dr. F. Beilstein and A. Kuhlberg.—Nitrited ortho-acetoluid, $C_7H_6(NO_2).NH(C_2H_3O)$, is obtained by treating ortho-acetoluid with very strong nitric acid kept in a freezing mixture. This body crystallises in rhombic-shaped prisms, fuses at between 101° and 102°, is readily soluble in boiling alcohol, almost insoluble in boiling water, and yields, by being decomposed with the requisite quantity of alcoholic potassa solution, nitro-ortho-toluidine, $C_7H_6(NO_2).NH_2$, soluble in water, fusing at 134°, readily soluble in acids, and re-precipitated from these solutions by ammonia.

The Cerite Metals.—Dr. C. Erk.—This very lengthy monograph, illustrated by some woodcuts relating to the spectrum analysis phenomena of the metals alluded to, is divided into the following chapters:—Separation of cerium from lanthanum and didymium; separation of lanthanum from didymium; absorption-spectrum of didymium solutions; separation of yttria from the oxides of lanthanum and didymium; composition of the oxides of cerium; cero-ceric hydroxide; on the quantity of water contained in oxalate of cerium; composition of cero-ceric sulphate; basic salts; basic cero-ceric sulphate; basic cero-ceric nitrate; basic cero-ceric acetate; electrolytic experiments made with fused cero-chloride; concentrated cero-sulphate solution; neutral cero-nitrate solntion; basic cero-ceric acctate solution.

Preliminary Notice on a Crystallised Xylol, Dimethyl-Benzol.—P. Jannasch.—The author prepared perfectly pure dimethylbenzol from perfectly pure crystalline bromotoluol, iodmethyl, and sodium, obtaining a body crystallised at 135°, which, on being cooled, and afterwards purified, yielded a solid crystalline substance, boiling at 136°, fusing at 15°, and found to consist, on being analysed, of C_8H_{10}. The author is engaged with further researches on this compound and its derivatives.

Bulletin de l'Academie Royale des Sciences, des Lettres et des Beaux Arts de Belgique, No. 2, 1871.

The only paper relating to physico-chemical sciences in this number is the following:—

Note on the Quality of our Ancient Limestones, as regards their Fitness for Building Purposes.—M. J. J. Omalius d'Halloy. The main gist of this paper is, that it is not so much the texture of the limestones which has to be taken into consideration, since the structure and texture of these stones may vary immensely, and yet they may all be suited for building purposes, provided the layers or beds have not been, as very frequently is the case, dislocated by geological upheavings, whereby many of these kinds of stone become foliated, and do not then withstand wind and weather for any length of time without crumbling to pieces.

No. 3, 1871.

Contains—

Note on the Aurora Borealis observed at Brussels on the 12th of February last.—M. A. Quetelet.

Observations on the Action of Iodate of Potassium upon Animals.—Dr. Melsens.—The record of some experiments upon dogs, to which this salt was given along with their food. The author concludes, from his experiments, that iodate of potassium is a violent poison; but this paper is only a preliminary notice, it being the author's intention to publish an exhaustive account of his experiments, and of the effects of this salt on the blood and internal organs of the animals experimented with.

NOTES AND QUERIES.

Nitrous Oxide.—(Reply to "Inquirer.")—You cannot convert nitrous oxide into nitric oxide. The latter may be converted into the former by placing it in contact with moist iron filings, zinc filings, or soluble sulphides, but the nitrous oxide so obtained is frequently contaminated with ammonia.—A. A.

Notched Teeth.—(Reply to "Reader.")—Your query is one strictly belonging to the domain of physiology, and we advise you to read the excellent article on "Teeth" in the "Cyclopœdia of Anatomy and Physiology," edited by Dr. R. Todd, vol. iv., part II., pp. 864—935 inclusive. From this it appears that there is no connection whatever between notched teeth and syphilis, the notches being due to the peculiar lamellated structure of the teeth, which by constant mastication become smooth.—X. Y. Z.

Cellulose.—(Reply to "Papyrus.")—Your best plan is to exhaust the substances in question, first with boiling water, next with boiling alcohol, then with boiling ether; then treat the residue, first with weak aqueous caustic potassa solution (1 of solid potassa in 100 of water), and, after thorough digestion and repeated washing with boiling distilled water, treat the residue with dilute sulphuric acid (1 to 7 or 8); wash again thoroughly with boiling water, dry, and treat once more with alcohol. What remains after this treatment you may consider to be cellulose, which, however, to become quite white, requires bleaching with chlorine.

Electrical Properties of Aluminium and Magnesium.—A correspondent asks about the electrical properties of aluminium. As a positive, I find it about equal to iron; and as a negative, inferior, if the iron be skilfully used. For if iron be skilfully used it is nearly equal to copper; used as a negative it should always be in such a way as to form a *black oxide* on the surface. I have lately been trying experiments with magnesium as a positive; the great evil of it is the rapid local action which takes place, which, however, by a peculiar arrangement, I have succeeded in entirely overcoming. The following results as to its properties may be of interest to your readers:—On the galvanometer I used, which presented considerable resistance, an ordinary Daniell's battery stood at 38°. With magnesium for the positive, and copper and sulphate of copper for the negative, the current rose to 55°. With magnesium as positive, and zinc for negative, I got a current of 35°; with iron for negative, 45°; the first being nearly equal to Daniell's battery, and the latter superior. With a different electrolyte, I got between zinc and copper, 46°; and magnesium and copper, 60°. With another specimen of magnesium, procured fresh this morning from Solomon's, the current rose to 67°. I then took another galvanometer showing smaller numbers, with the following results:—

Zinc and copper, in an arrangement of my own	15°
Magnesium and copper	26°
One of Grove's cells	22°
One of Bunsen's cells	26°
Magnesium, with carbon and nitric acid for negative	30°
With platinum and nitric acid	29°
Magnesium with bichromate of potass and carbon for negative	30°

Thus, magnesium with platinum and carbon is to zinc as about the tangents of 29° and 30°, respectively, to tangents of 22° and 26°; but, with iron and copper for negatives, it is to zinc nearly as 7 to 4. I think 22° for Grove's cell was rather too low, as the porous cell was new and not thoroughly soaked with the acids, and it was some time before I could get any current at all through it. Thus, magnesium and copper was equal to a Bunsen's and superior to a Grove's cell. The magnesium, with carbon and nitric acid or bichromate for a negative, is, I think, the most powerful *practical* arrangement on record, and is very constant for a long time. Sodium or potassium are, no doubt, stronger, but are difficult in the use. Probably lithium would be very powerful. If any of your readers will furnish me with a piece sufficient to try with, I will test it in my arrangement.—H. HIGHTON, Putney.

MEETINGS FOR THE WEEK.

MONDAY, 22nd.—Royal Geographical, 1. Anniversary.
TUESDAY, 23rd.—Royal Institution, 3. Rev. Prof. Haughton, "On the Principle of Least Action in Nature."
—— Civil Engineers, 8.
—— Ethnological, 4. Anniversary.
WEDNESDAY, 24th.—Society of Arts, 8.
—— Geological, 8.
THURSDAY, 25th.—Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
—— Royal, 8½.
FRIDAY, 26th.—Royal Institution, 9. Prof. Rankine, "On Sea Waves."
SATURDAY, 27th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."

TO CORRESPONDENTS.

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ERRATUM.—In Mr. H. A. Smith's paper in our last issue, p. 221, the last three lines in Table III. should read thus:—

100 tons of hard Norwegian pyrites make 140·875 tons of H_2SO_4, containing	1·051
140·875 tons of sulphuric acid make 104·9 tons of HCl, containing	0·691
140·875 tons of sulphuric acid make 204·12 tons of Na_2SO_4, containing	0·029

A. R. H.—A new edition of Mr. Sutton's "Volumetric Analysis" will shortly appear. The Editor is not engaged on a translation of Dr Mohr's work.

Merrick and Gray.—Received.

Dr. B. H. Moore.—Your letter and enclosure came duly to hand.

C. N.—"Food and its Adulterations," by Dr. Hassall, will meet your requirements; the price, we believe, is 10s. 6d.

NOTICE TO AMERICAN SUBSCRIBERS.

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THE CHEMICAL NEWS.

VOL. XXIII. No. 600.

ON THE

NITROGEN SUPPLIED TO THE SOIL IN MANURE, AND NOT RECOVERED IN THE INCREASE OF CROP.

By J. B. LAWES, F.R.S., F.C.S., and Dr. J. H. GILBERT, F.R.S., F.C.S.

REFERRING to that part of Dr. Voelcker's lecture "On the Productive Powers of Soils in relation to the Loss of Plant-Food by Drainage" in which he gave the results of his elaborate investigation into the composition of drainage-waters from differently manured experimental plots at Rothamsted, it has occurred to us that a *resumé* of some points of our own part of the enquiry might be of interest to the readers of the CHEMICAL NEWS at the present time. It is proposed, therefore, to give a brief account, chiefly in the form of quotations from, or reference to, former papers, of those of our own results and conclusions which relate to the question of the amounts of the nitrogen supplied in manure found to be recovered and not recovered in the increase of crop obtained.

In the early years of our experiments—indeed more than twenty years ago—we directed attention to the fact that when, especially in the case of graminaceous crops, nitrogen was supplied to the soil in the form of ammonia salts, or other concentrated manures, a large proportion remained unrecovered in the increase of produce obtained. It would, of course, not be expected that the whole of the nitrogen, or, indeed, of any other constituent of manure, would be gathered up by the crop which immediately succeeded its application. But it was found, on calculation, that when a given amount of nitrogen was supplied year after year, and the same description of crop was grown for a series of years in succession, generally less than half that which had been supplied was recovered in the increase of crop; whilst, if the application were discontinued, but a very small proportion of the missing amount of nitrogen would be recovered each year in the succeeding crops.

At first we were disposed to consider that this loss of nitrogen might, in part at least, be explained by reference to the vital actions of the plant itself; it having been supposed by several experimenters that plants evolved nitrogen from their leaves during growth. Thus De Saussure,* Daubeny,† and Draper‡ concluded, from experiments with plants, that such an evolution did take place. Mülder, again, from purely chemical considerations, supposed that there was a constant evolution of nitrogen during the growth of plants, and that the source of it was the compounds containing it already existing in the plants. For a fuller account of our treatment of this subject at the time we must refer to our early papers, and especially to one published in the *Journal of the Horticultural Society of London*, vol. v., part 1, 1850.‖

After showing, in a paper in the *Philosophical Transactions*, part ii., 1861,§ the relation of the increased yield of nitrogen to the amount supplied in manure in some particular cases, we submitted the following questions:—

"Is the unrecovered amount of supplied nitrogen, or at any rate a considerable proportion of it, drained away and lost?

"Are the nitrogenous compounds transformed within the soil, and their nitrogen, in some form, evaporated?

"Does the missing amount for the most part remain in some fixed combination in the soil, only to be yielded up, if ever, in the course of a long series of years?

"Is ammonia itself, or nitrogen in the free state, or in some other form of combination than ammonia, given off from the surface of the growing plant? Or, lastly—

"When nitrogen is supplied within the soil for the increased growth of the graminaceous crop, is there simply an unfavourable distribution of it, considered in relation to the distribution of the underground feeders of the crop?—the leguminous crop, which alternates with it, gathering from a more extended range of soil, and leaving a residue of assimilable nitrogen within the range of collection of a next succeeding cereal one?"

Briefly enumerated, the three main sources of loss of nitrogen here suggested are, then—*drainage*, *accumulation within the soil in a state of combination or of distribution unfavourable for being taken up by immediately succeeding crops*, or *evolution in some form from the surface of the growing plant*.

From some of the results reported upon in the same paper, and also from other considerations, we were led to conclude, in opposition to the view we had previously entertained, that the last-named of these, that is, *evolution from the plant*, did not take place.

With regard to *drainage*, the previous results of Professor Way,* and especially the subsequent ones of the experiments conducted at Rugby under our superintendence, for the Royal Sewage Commission,† led us to attribute great importance to that part of the subject. In the course of that enquiry, we arranged for the collection of sixty-two samples of drainage-water, the partial analysis of which was conducted by Professor Way; and, comparing the results with those on the corresponding samples of sewage, it was concluded that but a small proportion of the nitrogen of the sewage which was not obtained in the increase of produce was recovered in the drainage-water in the form of ammonia. We therefore arranged for the collection of some special samples for complete analysis, and especially for the determination of the nitric acid, if any, in both sewage and drainage-water. The results showed considerably more nitrogen in the drainage in the form of nitric acid than in that of ammonia. Indeed, it was obvious that a large proportion of that important manurial constituent of the sewage was drained away and lost. Satisfied for the time with this indication, it was not contemplated to follow up that part of our own general inquiry until the question of the accumulation of nitrogen within the soil itself had first been investigated.

When, among other field experiments made at Rothamsted, wheat had been grown, year after year, on the same land for more than twenty years, on some portions without any manure, and on others with farm-yard manure, or with various descriptions of manure, and the results obtained in the field during that period had been published, the question of the varying composition of the crop according to season and manure was resumed, and that of the accumulation of the nitrogen of manure in the soil was taken up; and the results of the latter investigation were given at the meeting of the British Association for the Advancement of Science, in 1866. After considering the difficulties of sampling, preparing for analysis, and analysing soils in such a manner as to yield results applicable to the purposes of the enquiry, and describing the

* "Recherches Chimiques sur la Végétation," 1804.

† Memoir "On the Action of Light upon Plants, and of Plants upon the Atmosphere" (*Phil. Trans.*, part i., 1836).

‡ "Chemistry of Plants," 1844, pp. 184-5 and context.

‖ "Experimental Investigation into the Amount of Water given off by Plants during their Growth, especially in relation to the Fixation and Source of their Various Constituents."

§ "On the Sources of the Nitrogen of Vegetation, with special reference to the question whether Plants Assimilate Free or Uncombined Nitrogen," by Lawes, Gilbert, and Pugh.

* "On the Composition of the Waters of Land Drainage and of Rain" (*Journ. Roy. Ag. Soc. England*, vol. xvii., part i.)

† "On the Sewage of Towns" (Third Report and Appendices 1, 2, and 3, of the Royal Commission, 1865). "On the Composition, Value, and Utilisation of Town Sewage (*Journ. Chem. Soc.*, new series, vol. iv.; entire series, vol. xix., 1866.)

methods adopted, the results themselves, arranged in tables, were discussed. The percentage and calculated acreage amounts of nitrogen, existing in such condition as to be determinable by burning with soda-lime, were given, for the soil of the first, of the second, and of the third 9 inches in depth, of eleven differently manured plots, showing, therefore, both percentage and actual amounts to the depth of 27 inches in all.

In the short abstract only that was published, the results and conclusions were summarised as follows :*—

"The accumulation of nitrogen from the residue of manuring was found to be, in some cases, very considerable; but, even with equal amounts supplied, it varied, both in total amount and in distribution, according to circumstances, the depth to which the unused supply had penetrated being apparently influenced by the character and amount of the asssociated manurial constituents. The general result was, that although a considerable amount of the nitrogen supplied in manure, which had not been recovered as increase of crop was shown to remain in the soil, still a larger amount was as yet unaccounted for. Initiative results indicated that some existed as nitric acid in the soil, but it was believed that the amount so existing would prove to be but small. In fact, it was concluded that a considerably larger proportion would remain entirely unacccounted for within the soil to the depth under examination than was there traceable, and the probability was, that at any rate some of this had passed off into the drains, and some into the lower strata of the soil. Finally, it was shown, by reference to field results, that there was not more than 1 or 2 bushels of increase in the wheat crop per acre per annum, due to the large accumulated residue of nitrogen in the soil, notwithstanding its amount was many times greater than that which would yield an increase of 20 bushels or more, if applied afresh to soil otherwise in the same condition. On the other hand, it was shown that the effect of an accumulated residue of certain mineral constituents was not only very considerable in degree, but very lasting."

Thus, then, it was established, that there was a considerable accumulation within the soil, of nitrogen supplied in manure and not recovered in the increase of crop, but that there remained a considerable quantity not so accounted for; and it was concluded that some of this had passed off into the drains, and some into the lower strata of the soil. Being fully occupied at the time with other subjects, and finding that Dr. Voelcker was desirous to investigate the question of land drainage, we gladly provided him with samples of the drainage water from the differently manured plots in the experimental wheat-field, and also with full particulars of their history for the purposes of inquiry. The results of the seventy complete analyses of drainage water of accurately known history, which he has published in his lecture will, we doubt not, prove a most valuable contribution to our knowledge, and an important aid to the future study of the subject, not only in its agricultural bearings, but also in relation to the question of the influence of the sources of potable and other waters upon their composition and quality.

Referring to Dr. Voelcker's own paper for any account of his results and conclusions, we will only further observe that, it being established beyond all question that land-drainage may carry off as nitrites and nitrates large quantities of the nitrogen supplied as manure, it becomes a matter of great practical importance to consider the power of different descriptions of soil to retain the nitrogen supplied to them, to estimate approximately the probable average proportion of the rainfall which may pass from them as drainage water, and to determine, accordingly, the best modes, and the best periods of the year, for the application of nitrogenous manures. In a recent paper† we have directed attention to such evidence as is at command relating to the proportion of the drainage to rainfall in some special cases, and this subject, as well as that of the best modes, and the best periods of the year, for the application of nitrogenous manures, is now receiving attention at Rothamsted.

Rothamsted, May 18, 1871.

* *British Association Report* for 1866. See also CHEMICAL NEWS, vol. xiv., p. 122; and *Agricultural Gazette*, September 8, 1866.

† Effects of the Drought of 1870 on some of the Experimental Crops at Rothamsted. (*Journ. Roy. Ag. Soc. Eng.*, vol. vii., S.S. Part I., 1871).

NOTE ON THE SPECIFIC GRAVITY OF AQUEOUS HYDROBROMIC ACID.*

By Dr. C. R. A. WRIGHT.

THE writer having recently had occasion to prepare some little quantity of pure aqueous hydrobromic acid, advantage was taken of the circumstance to examine the relation subsisting between the specific gravity and percentage of HBr in specimens of acid of different strengths.

The acid was obtained by the action of sulphuretted hydrogen on excess of bromine in presence of water, and subsequent repeated rectification over pulverised potassium bromide; it was free from sulphuric acid and other sulphur compounds, and contained no free bromine. The percentage of HBr was determined by a standard alkaline solution, and the specific gravity by weighing a known volume of the liquid. The results, which do not pretend to very rigorous accuracy, are as follows :—

Percentage of HBr.	Sp. gr. at 15° C.
10·4	1·080
23·5	1·190
30·0	1·248
40·8	1·385
48·5	1·475
49·8	1·515

By graphical representation, it was found that these numbers agree tolerably together, yielding a *mean curve* from which the following table is constructed.

Percentage of HBr.	Sp. gr. at 15° C.	Difference for 5 per cent.	Difference in 1 per cent.
nil.	1·000	—	—
5	1·038	0·038	0·0076
10	1·077	0·039	0·0078
15	1·117	0·040	0·0080
20	1·159	0·042	0·0084
25	1·204	0·045	0·0090
30	1·252	0·048	0·0096
35	1·305	0·053	0·0106
40	1·365	0·060	0·0120
45	1·435	0·070	0·0140
50	1·515	0·080	0·0160

From these numbers it appears that the increase in specific gravity is not proportional to the increase in percentage of HBr, showing, in this respect a difference between hydrobromic and hydrochloric acid.

The numbers in the foregoing table agree tolerably with rough determinations given by other experimenters; thus C. G. Williams (*Jahresberichte*, 1858, p. 438) finds acid of 37 per cent HBr has sp. gr. 1·32: that deduced from the Table for this strength is $1·305 + 2 \times 0·0120 = 1·329$.

In Miller's "Chemistry," 4th edition, vol. ii., p. 146, acid of *about* 47 per cent HBr is said to have sp. gr. 1·486 (authority not stated); the sp. gr. deduced from the Table is $1·435 + 2 \times 0·0160 = 1·467$, the sp. gr. 1·486 representing 48·2 per cent HBr.

In many text-books, however (Watts's "Dictionary," vol. i., p. 673; Gmelin's "Handbook," vol. ii., p. 281;

* Read before the Newcastle-upon-Tyne Chemical Society, January 26th, 1871.

Brande's "Chemistry," &c.), the specific gravity of the densest aqueous hydrobromic acid is given, on the authority of Löwig, as 1·29; if this number be correct, the solubility curve must be a very abnormal one, the curvature up to 50 per cent HBr being *convex* towards the horizontal base line, while at some higher percentage a point of contrary flexure will be found, after which the curve becomes *concave* towards the horizontal line, and approaches this line instead of receding from it. It would appear much more probable that the acid employed by Löwig contained a large quantity of hydrochloric acid, a saturated aqueous solution of this acid having only a sp. gr. of about 1·2. As yet, stronger acid than 50 per cent has not been examined, but it is proposed to determine the form of the solubility curve for higher percentages of HBr.

The foregoing experiments were made in the laboratory of Messrs. Bell, Brothers, Washington.

ON BRITTLE SILVER.

By A. H. CHURCH, M.A.

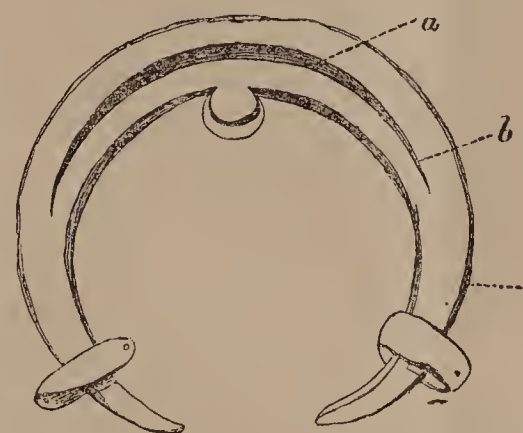
The examination in my laboratory of some ancient silver ornaments has led to a curious result, which may prove of interest to some of the readers of the Chemical News.

The silver objects referred to were obtained lately by General de Cesnola in the island of Cyprus. He opened several hundreds of tombs, specially at Dali, the site of the ancient city of Idalium, and obtained large quantities of specimens of antique glass and metal-work. I believe the silver specimens with which the present note is concerned cannot have an antiquity of less than 1500 years, but the date of their manufacture may prove to have been even more remote. It is necessary to say a few words concerning the form of the particular object of silver which I have specially examined. It is, or rather was, a fibula, or, at least, was shaped like a fibula. It resembled a crescent in that the ends of the bow were small and pointed and its middle much stouter; sections throughout were nearly cylindrical, and in the widest part about ¼-inch in diameter. The specimen was nearly covered with a dark sub-metallic crust, and underneath this a soft powdery grey substance was observable. Both these layers, which together presented an average thickness of 1-30th of an inch, could be readily removed by the finger-nail. Below, the substance appeared white, metallic, and uniform, but yet was remarkable for its extreme brittleness, the object being easily snapped in numerous pieces by a very slight pressure. If the brittleness had pertained to the outer crust or layer, obviously altered as it was, it would not have been surprising. But to find that an apparently continuous metallic substance of great lustre, and, certainly once, at the time of its manufacture, in a perfectly ductile condition (for portions of it had been twisted into fillets), to find this metallic substance as brittle as a stick of chalk was certainly difficult to explain. And a further difficulty was offered by the observation that in the thicker part of the crescent, and only there, a kind of core existed similar in colour to the main mass of the object, but possessing great tenacity and ductility. This core could not be broken short off, but might be wrenched out of the surrounding brittle mass, though with some difficulty. It tapered off towards the narrower ends of the crescent, and terminated where those parts did not exceed ⅛th of an inch in thickness. A physical and chemical examination of the materials enables me to offer a reasonable explanation of the observed appearances.

The outer crust consisted of the corroded and chemically altered silver-alloy. It was made up of finely-divided metallic silver, with traces of the sulphide and chloride of that metal, and it gave also a faint trace of iodide; copper, apparently in the form of a basic carbonate, was likewise present, with a trace of gold. The main part of the specimen consisted of the second and extremely brittle layer referred to before. It gave on analysis the following composition:—

	Per cent.
Silver	94·69
Gold	0·41
Copper	3·43
Lead	0·28
Antimony with trace of arsenic and bismuth	1·21
	100·02

The central ductile and malleable core had *precisely* the same composition as this brittle layer. To what, then, are the physical differences between them to be attributed? Not to chemical differences or chemical changes, but to a *molecular change* which, in the course of ages, has occurred throughout all the thinner parts of the piece of silver, but had left unchanged in the thickest part a central fusiform core. A crypto-crystalline structure had been produced in the previously homogeneous alloy, which caused the peculiarly easy fracture of the metal. A smart blow with a hammer was capable of shattering it to powder, while by rolling or gentle hammering, the brittle mass could be restored readily to its pristine condition. The density of



Section (in the plane of the paper) of the silver object referred to.

the brittle silver was 9·06, but, by rolling, it became 10·20. There was not, therefore, a chemical change, but a physical one. However produced, whether by minute and long-continued alterations of temperature, or by other agencies, this molecular change of silver may possibly throw some light upon the similar change observed in the case of iron.

ON THE REVIVED THEORY OF PHLOGISTON.*

By WILLIAM ODLING, M.B., F.R.S.
Fullerian Professor of Chemistry, Royal Institution.

Observationem, quam produco, bono jure mihi vindico. . . Materia hæc ignescens, in omnibus tribus regnis, una eademque existit. Unde, ut e vegetabili in animale, abundantissime transmigrat, ita ex utrolibet horum, in mineralia et ipsa metalla, promptissime omnium transfertur.—*Stahlii*, Experimenta, Observationes, Animadversiones, CCC, Numero.

In 1781—83, Cavendish showed that when inflammable air or hydrogen, and dephlogisticated air or oxygen, are exploded together in certain proportions, "almost the whole of the inflammable and dephlogisticated air is converted into pure water," or, as he elsewhere expresses it, "is turned into water."

On June 24, 1783, the experiment of Cavendish was repeated on a larger scale and in a somewhat different form by Lavoisier, who not only confirmed the synthesis of the English chemist, but drew from it the conclusion—at first strongly contested, then rapidly acknowledged, and since never called into question—"that water consists of in-

* A Lecture delivered at the Royal Institution of Great Britain, April 28th, 1871.

flammable air united to dephlogistigated air," or that it is a compound of hydrogen and oxygen.

This conclusion, so opposite to his own preconception on the matter, Lavoisier subsequently confirmed by an analysis of water. He found that iron, heated to redness and exposed to the action of water-vapour, became changed by an abstraction of oxygen from the water, into the self-same oxide of iron procurable by burning the metal in oxygen gas,—the other constituent of the water, namely, its hydrogen, being freely liberated.

With the demonstration by Lavoisier of the composition of water began the triumph of that antiphlogistic theory, which he had conceived, in a necessarily imperfect form, so far back as 1772, or before the discovery of oxygen, and had brought to completion by the aid of every successive step in pneumatic chemistry, achieved by himself or by others.

In 1785, the relationship to one another of hydrogen and water being then conclusively established, Berthollet declared himself a convert to the new theory of combustion put forward by his countryman. Fourcroy next gave in his adhesion; and soon afterwards De Morveau, invited to Paris expressly to be reasoned with by Lavoisier, succumbed to the reasons set before him. The four chemists then associated themselves together, and in spite of a strong though short-lived opposition both in England and Germany, succeeded in obtaining for *La Chimie Française* an all but universal recognition.

The principal articles of the new or antiphlogistic theory of combustion propounded by Lavoisier are as follows:—That combustible bodies in burning yield products of various kinds—solid in the case of phosphorus and the metals, liquid in the case of hydrogen, gaseous in the case of carbon and sulphur. That in every case the weight of the products formed by the burning is greater than the weight of the combustible burnt. That the increase of weight is due to an addition of matter furnished to the combustible by the air in which its burning takes place. That bodies of which the weights are made up of the weights of two or more distinct kinds of matter are of necessity compound; whereas bodies of which the weights cannot be shown to be made up of the weights of two or more distinct kind of matter are in effect simple or elementary. That inasmuch as the weights of the products furnished by the burning of different combustibles are made up of the weights of the combustible burnt and of the oxygen consumed in the burning, these products are compound bodies—oxides, in fact, of the substances burnt. That inasmuch as given weights of many combustibles, as of hydrogen, sulphur, phosphorus, carbon, and the metals, are not apparently made up of the weights of two or more distinct kinds of matter, these particular combustibles are in effect elementary; as for the same reason is the oxygen with which in the act of burning they enter into combination. And, lastly, that combustion or burning consists in nothing else than in the union of combustible matter, simple or compound, with the empyreal matter, oxygen—the act of union being somehow attended by an evolution of light and heat. And except that it would be necessary nowadays to explain how, in certain cases of combustion, the combustible enters into union, not with oxygen, but with some analogue of oxygen, the above precise statement might equally well have been made by Lavoisier in 1785, or be made by one of ourselves at the present day.

Lavoisier's theory of combustion being known as the antiphlogistic theory, the question arises, What was the phlogistic theory to which it was opposed, and which it succeeded so completely in displacing? This phlogistic theory was founded and elaborated at the close of the seventeenth century by two German physicians, Beccher and Stahl. Having exercised a scarcely disputed authority over men's minds until the notorious defection in 1785, it preserved for some years longer a resolute though tortuous existence, and was to the last defended and approved by our own Priestley and Cavendish—who died, the former in 1804, and the latter in 1810.

The importance attached to the refutation of this theory may be judged of from the circumstance, that after the early experiments of Lavoisier on the composition and decomposition of water had been successfully repeated by a committee of the French Academy in 1790, a congratulatory meeting was held in Paris, at which Madame Lavoisier, attired as a priestess, burned on an altar Stahl's celebrated *Fundamenta Chemiæ Dogmaticæ et Experimentalis*, solemn music playing a requiem the while. And the sort of estimation in which the Stahlian doctrines have since been held by chemists is fairly illustrated by a criticism of Sir J. Herschel, who, speaking of the phlogistic theory of chemistry, says that it "impeded the progress of the science, as far as a science of experiment can be impeded by a false theory, ... by involving the subject in a mist of visionary and hypothetical causes in place of the true acting principles." Possibly, however, this much-abused theory may yet prove to contain an element of permanent vitality and truth; anyhow the study of this earliest and most enduring of chemical theories can never be wholly devoid of interest to chemists.

To appreciate the merit of the phlogistic theory it is necessary to bear in mind the period of its announcement. Its originator, Beccher, was born in 1625, and died a middle-aged but worn-out man in 1682, a few years before the publication of the "Principia." His more fortunate disciple, Stahl, who was born in 1660, and died in 1734, in his seventy-fifth year, though afforded a possibility of knowing, seems equally with Beccher to have remained throughout his long career indifferent to the Newtonian principle that the weight of a body is proportionate to its quantity of matter—that loss of weight implies of necessity abstraction of matter, and increase of weight addition of matter. Whether or not the founders of the phlogistic theory conceived that change of matter in the way of kind, might equally with its change in point of quantity, be associated with an alteration in weight—and it must not be forgotten what pains Newton thought it necessary to take in order to show the contrary—certain it is they attached very little importance to the changes of weight manifested by bodies undergoing the metamorphosis of combustion. It might be that when combustible charcoal was burned the weight of incombustible residue was less than the original weight of charcoal—it might be that when combustible lead was burned the weight of incombustible residue was greater than the original weight of metal—this was far too trifling an unlikeness to stand in the way of the paramount likeness presented by the two bodies. For the lead and charcoal had the common property of manifesting the wonderful energy of fire; they could alike suffer a loss of light and heat—that is, of phlogiston—by the deprivation of which they were alike changed into greater or less weights of inert incombustible residue.

And not only were these primitive students of the philosophy of combustion unconscious of the fact and meaning of the relationship in weight subsisting between the consuming and the consumed body, but they were altogether ignorant of the part played by the air in the phenomena which they so boldly and successfully attempted to explain. Torricelli's invention of the barometer and Guericke's invention of the air-pump were both, indeed, made during Beccher's early boyhood; but years had to elapse before the consequent idea of the materiality of air could be domiciled, as it were, in human understandings. And not until more than a century after Torricelli's discovery of the weight of air,—not, indeed, until the time of the great pneumatic chemists, Black, and Cavendish, and Priestley, and Scheele, was it ever imagined that the aërial state, like the solid or liquid state, was a state common to many distinct kinds of matter; and that the weight or substance of a rigid solid might be largely contributed to by the weight or substance of some constituent having its independent existence in the aërial or gaseous form. The notion that 100 lbs. of smithy-scales might consist of 73 lbs. of iron, and 27 lbs. of a particular kind of air, and that

100 lbs. of marble might consist of 56 lbs. of lime, and 44 lbs. of another kind of air, was a notion utterly foreign to the older philosophy. Air, it was allowed, might be rendered mephitic by one kind of contamination, and sulphurous by another, and inflammable by a third; it might even be absorbed in, and so add to the weight of, a porous solid, as water is absorbable by sand; but still air was ever indisputably air, essentially alike and unalterable in its mechanical and chemical oneness. This familiar conception had to be overcome, and the utterly strange notion of the largely aërial constitution of solid matter to be established in its stead, by the early pneumatic chemists, Black, and Cavendish, and Bergmann, before the deficiencies rather than positive errors of the phlogistic theory could be perceived.

But long ere the foundation of modern chemistry had thus been laid, in 1756, by Black's discovery of fixed air or carbonic acid as a constituent of mild alkalies and limestone, those old German doctors, Beccher and Stahl, though ignorant of the nature of air and neglectful of the import of gravity, had yet found something to say about the chemistry of combustion worthy of being defended a century afterwards by men like Priestley and Cavendish, —worthy, it is believed, of being recognised nearly two centuries afterwards as the expression of a fundamental doctrine in chemical and cosmical philosophy. They pointed out, for example, that the different and seemingly unlike processes of burning, smouldering, calcining, rusting, and decaying, by which combustible is changed into incombustible matter, have a community of character; that combustible bodies possess in common a power or energy capable of being elicited and used, whereas incombustible bodies are devoid of any such energy or power; and lastly, that the energy pertaining to combustible bodies is the same in all of them, and capable of being transferred from the combustible body which has it to an incombustible body which has it not, rendering the body that was energetic and combustible inert and incombustible, and the body that was inert and incombustible energetic and combustible—and further rendering some particular body combustible over and over again. That this is a fair representation of the views held by phlogistic chemists is readily recognisable by a study of chemical works written before the outbreak of the antiphlogistic revolution. After Lavoisier's challenge, the advocates of phlogiston, striving to make it account for a novel order of facts with which it had little or nothing to do, were driven to the most incongruous of positions; for while Priestley wrote of inert nitrogen as phlogisticated air, Kirwan and others regarded inflammable hydrogen as being phlogiston itself in the isolated state.

(To be continued.)

ON THE DURATION OF FLASHES OF LIGHTNING.

By O. N. ROOD.

After the completion of my first set of experiments on the duration of the discharge of a Leyden jar, I became anxious to make some measurements of the duration of a flash of ordinary lightning, which may be considered as equivalent to the discharge of an immense jar with an enormous striking-distance. The results of Feddersen have shown that the duration of the discharge is increased by an addition to the size of the jar, as well as by augmentation of the striking-distance, and as both these quantities are so large with a flash of lightning, it was reasonable to expect that the duration of its discharge would be prolonged in some corresponding ratio. During the violent thunder-storm of last August, which occurred in the evening, I happened to be at a house commanding an unobstructed view of the horizon, and this circumstance taken in connection with the frequency and proximity of the electrical discharges, induced me, although entirely unprovided with apparatus, to attempt a measurement of their duration. A circular disc, five inches in diameter, was hastily cut from white cardboard, while a steel shawl-pin served as an axis, on which it was made to revolve by constantly striking its edge tangentially with the right hand, the pin being held in the left. The *maximum* velocity attainable in this way was always employed. The general indications at the time were that the rate thus obtained was considerably more uniform than might have been expected, and subsequent quantitative experiments have confirmed this idea. The first experiments were made by observing black figures traced near the circumference of the disc, which was illuminated solely by the rapidly recurring flashes, and it often happened that the figures, with their details, were seen quite as clear and sharply as though the disc had been stationary; on the other hand, sometimes the edges seemed blurred, as though the disc had moved through a few degrees during the act of discharge. The result being doubtful, the mode of experimenting was quickly changed; about fifteen narrow radial apertures were made near the circumference of the disc, and the flashes and illuminated clouds were observed through these openings, the disc being made to revolve as before. The distance of the eye from the apparatus was about 8 inches, and it was of course adjusted so as to obtain distinct vision of the disc. The result was that sometimes the openings were seen quite unchanged in appearance, but more frequently they were most distinctly elongated into well-defined streaks some degrees in length. They were observed often and without difficulty, but as farther confirmation I may add that I requested Professor Joy, who was ignorant of the actual form of the aperture, to state his opinion of their apparent shape while the disc was in rotation. The reply was that they resembled Prince Rupert's drops—a not unfair description of the phenomena in question. Repeated estimates of their size were then made with paper and pencil. Some time afterward I measured the velocity which I could communicate to this disc in the manner above described, by attaching to it a small hollow axis through which the steel pin passed, the disc being then caused to wind up a thread stretched by a small weight. The rate of rotation thus attainable was found to be about twelve revolutions per second, which is a little more than I had anticipated. The average size of the streaks was 9°, corresponding to a duration of 1-480th of a second. It hence results that the duration of the flashes of lightning on the occasion referred to was in round numbers about 1-500th of a second, some of them, however, seeming to be confined to smaller limits.

I know of only a single circumstance which might militate against the correctness of the above conclusion, and it is but fair to give it such weight as it may carry. Becquerel has succeeded with some difficulty in observing a faint phosphorescence when an electric discharge is passed through rarefied air, and it is not absolutely impossible that the effects observed by me were due to a cause of this kind.

This point can hereafter readily be decided by observing with a revolving disc, not the distant clouds, but a sheet of white paper, placed so as to receive the light from the electrical flashes.—*American Journal of Science.*

ON THE REDUCTION OF SULPHURIC ACID BY ZINC-AMALGAM.*

By Dr. I. WALZ.

Although, as is well known, a number of oxidising agents are capable of oxidising sulphuretted hydrogen to sulphuric acid, the reverse reaction, viz., the reduction of

* Communicated by the Author. Read before the Lyceum of Natural History, New York.

sulphuric acid to sulphuretted hydrogen, has hitherto been observed only in a few cases, and even then only to a very limited extent. Messrs. Calvert and Johnson published, in 1866, some observations upon the action of zinc upon the various hydrates of sulphuric acid, in which they remark that, at a high temperature, the mono-, bi-, and ter-hydrates of sulphuric acid, when acting upon zinc, evolve principally sulphurous acid, while the fourth, fifth, and sixth hydrates, are acted upon (in the words of the *Jahresberichte*) "with a preponderating evolution of sulphuretted hydrogen," an observation which I can confirm if it is intended to say that more H_2S is formed than SO_2, but less than H, as on repeating the experiment I found that but very little H_2S was formed. According to Messrs. Calvert and Johnson, sulphuric acid which is diluted with more than six equivalents of water evolves pure hydrogen, an observation which was disproved by Kolbe, who showed that, when even chemically pure dilute sulphuric acid acts upon chemically pure zinc, a trace of sulphuretted hydrogen is always formed. In the majority of reactions, however, in which a reduction of sulphuric acid takes place, it stops at sulphurous acid, as when mercury, copper, carbon, &c., act upon the acid. Oppenheim has shown that when concentrated sulphuric acid and phosphorus act upon each other the result is phosphorous and anhydrous sulphurous acid, according to the formula $3SO_4H_2+2P=2PO_3H_3+3SO_2$.

I have lately studied the action of zinc and sodium amalgam on concentrated sulphuric acid, and have made the—in view of the foregoing remarks—somewhat unexpected observation, that in this case a considerable amount of sulphuretted hydrogen is formed. With sodium amalgam, the action is almost instantaneous. A small quantity of sodium amalgam is covered in a test-tube with an equal amount of chemically pure sulphuric acid, sp. gr. 1·84; the smell of sulphuretted hydrogen is at once perceived, and on pouring water into the test-tube the solution appears milky, from the supension of finely-divided sulphur. But the course of the reaction can be better studied by employing zinc instead of sodium amalgam, as the action proceeds slower and more regularly in this case. (In preparing the zinc amalgam, whether the zinc be used in little sticks or in filings, I find it convenient to cover the mercury and zinc with water, rendered *alkaline* by a drop of soda, potash, or ammonia. The amalgamation takes place very rapidly, and at a lower temperature, and the operator is not exposed to the noxious vapour of mercury. I prefer alkalies to acids in amalgamating zinc.)

Zinc-amalgam is placed in a small H_2S apparatus, and covered with about an equal amount of chemically pure sulphuric acid (sp. gr., 1·84), and the gases evolved are passed into a solution of acetate of lead. The first few moments only hydrogen seems to be formed, but presently the reaction becomes intense, and sulphuretted hydrogen is given off in abundance. The liquid in the apparatus becomes white, which is caused partly by the formation of anhydrous sulphate of zinc, and partly by the deposition of sulphur. There appears to be a period at which H_2S and SO_2 are formed simultaneously, and the two, reacting upon each other, give rise to an abundant deposit of sulphur. In the latter stage of the reaction sulphurous acid alone is formed. At last, the generating flask contains only mercury and white dry sulphate of zinc, that is to say, if too much sulphuric acid is not taken. (An excess of sulphuric acid should be avoided from the beginning, as the reaction towards the end generally becomes very violent in this case, volatilising mercury and sulphur, and forcing the liquid out of the flask.) If we pour water into the generating flask, the sulphate of zinc is dissolved, and the quantity of sulphur formed becomes then apparent. The mercury, immediately after the reaction is over, generally presents a bright or but slightly tarnished surface, but, on remaining in contact with the liquid, is soon covered with a black coating of sulphide. No mercury enters into solution during the reaction.

I believe that the reduction of sulphuric acid in the beginning is not gradual and successive, but is explained by the simple formula—

$$SO_4H_2+4(H_2)=4H_2O+H_2S.$$

Laboratory, 18, Exchange Place, New York.

ON THE

MOLYBDATES AND VANADATES OF LEAD,

AND ON A

NEW MINERAL FROM LEADHILLS.*

By Professor Dr. ALBERT SCHRAUF, of Vienna.

(Concluded from p. 232).

4. *Chemical Properties of the Vanadates of Lead (Dechenite, Descloizite, and Vanadite).*

EOSITE as containing vanadic acid, stands next to the just-mentioned vanadites of lead. Dechenite was first found in 1851, by M. Krantz, near Schlettenbach; and M. Bergemann, neglecting a notable proportion of zinc† contained in this mineral, has stated for it the chemical formulæ VO_3PbO. Vanadite, first found at Kappel (Carinthia), received its specified name by the late Prof. Zippe, and is, according to M. Tschermak, $PbOVO_3$. Descloizite was stated by Mr. Damour (1854) to correspond to the formulæ $2PbO,VO_3$. I have, as early as 1861,‡ emitted the opinion that these three species, concordant in a great number of properties, may be considered as being specifically identical. M. Czudnowicz (*l. c.*), in his paper on vanadite, has proffered several arguments against the vanadium supposed to enter into the composition of these minerals so near related to each other, that they must be submitted altogether to comparative investigation. The Peruvian Descloizite is of so rare occurrence, that I could not detach any particle of the small group of crystals at my disposal for the purpose of chemical investigation, being thus obliged to confine them to two varieties of vanadite from Kappel, and to a specimen of Dechenite. On two specimens of vanadite from Kappel, vanadite appears in the form of crystals intimately connected into a crust, the single crystals not exceeding the size of ½ to ¾ m.m. On one specimen (variety A), the greater number of the larger crystals is of a dark greenish-brown colour (fluorescence-colour), and without translucidity; the smaller ones are reddish brown and translucid; the streak is brownish orange. This variety agrees, in all its external characters, with the specimen of Peruvian Descloizite now before me; the crystals of it are reddish to greenish brown, translucid, and their streak is brownish orange.‖

The crystals of the second specimen (variety B) form a thin crust over limestone; they are ½—1 m.m. in size, rather translucid, of lighter carnation tint, and less bright than those of variety A. The crystals of variety A show a vivid metallic brightness, while the light carnation ones of variety B scarcely offer a faint vitreous brightness, very easily altered by the contact with moist air; even the moisture contained in the air breathed out is sufficient to provoke a superficial decomposition of the crystals of variety B. Their brightness is immediately tarnished, and in a few minutes very diminutive greyish-white globules cover, like Mucedineæ, the surface of the

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* Abstract of a paper read before the Royal Society, May, 1871 Translated by Count A. Fr. Marschall.

† See Czudnowicz in *Poggendorff's Annalen*, vol. cxx.

‡ See Schrauf, *Poggendorff's Annalen*, vol. cxvi. I must here remark expressely, in order to avoid misunderstandings, that in the course of this paper I have purposely quoted the formulæ of precedent authors with the (older) symbols used by *them* to express vanadic acid. I do not intend to discuss the results of former analyses, because Professor Roscoe, in the course of his ingenious researches conerning vanadium, has expressly (See *Proceedings of the Royal Society*, vol. xvi., p. 226) said, "It is the author's intention to investigate the composition of the vanadates at a future time."

‖ See Schrauf "On the Identity of Vanadite and Descloizite," in *Poggendorff's Annalen*, vol. cxvi. (1861).

crystal. Some time after these globules assume a slight yellowish colour, but spread no further over the specimen as soon as it is kept in dry air. The supposition of this alteration to be caused by the presence of arsenic is contradicted by the results of subsequent investigation; proving arsenic, if it exists at all, to exist in such diminutive proportion that visible marks of its presence could not be obtained by heating the mineral on charcoal or in a glass recipient.* Notwithstanding the differences detailed above, the actions of chlorhydric acid, of alcohol, and of bisulphate of potash give evidence of the notable difference between eosite, Descloizite, and vanadite, and of the nearly absolute identity of the two varieties of vanadite from Kappel.

Small splinters of both these varieties, moistened with chlorhydric acid on glass plate, become yellowish grey on their margins, the internal portion assuming a dark reddish-brown tint. Under the protracted action of the acid this nucleus becomes darker and lessens in size, till at last it disappears, leaving a portion of uniformly greyish white substance. The nucleus of the carnation variety, B, shows a somewhat lighter tint in the beginning; this difference, however, vanishes more and more under the lengthened action of acid; alcohol added to the cold solution of both varieties, provokes the secretion of a yellowish green gelatinous substance, which, being heated to evaporation, leaves on the glass plate a precipitate of fine green colour.

These alterations are common to both varieties, as also those provoked by fusion with bisulphate of potash. The saline substance thus obtained is yellow during fusion; it becomes reddish during cooling, and deep reddish orange after complete refrigeration. This last colouration may exceed two or three times in intensity the colouration obtained by treating eosite with bisulphate of potash.

These experiments, merely tried with some few minute fragments, although anything but decisive, are sufficient for ascertaining the difference between eosite and the vanadates of lead.

As the dark variety (A) resembles Peruvian eosite, the light variety (B) bears some resemblance with the Dechenite from Schlettenbach.

This Dechenite is likewise of carnation colour, somewhat fainter on the surface than on the recent fracture. The cabinet specimen before me shows isolated crusts, transversely concreted, with globular external surface, offering a black berry-like aspect. A fracture shows these crusts to be concentric agglomerations of minute crystals (1 m.m. in size), externally exhibiting only one or two of their planes. Isolated margins may be found out by careful inquiry, but I could not succeed in obtaining any measurable angle. The crystaline crust includes a number of spheroidal cavities (similiar to those of pisolite), around which the aggregations of crystallised Dechenite are concentrically disposed. Greenish pyromorphite appears on the inferior surface of the specimen in question.

Like the variety β of vanadite, this specimen is very easily affected by moisture. Even when a sheet is held before the mouth, the moisture of the air breathed out is sufficient to provoke globular yellowish-grey efflorescences. The streak is orange, verging into reddish-brown.

The action of chlorhydric acid and bisulphate of potash is nearly the same with that exercised on vanadite, as already detailed.

A splinter moistened with chlorhydric acid shows a yellow margin and a darker nucleus, in which the vanadium is concentrated. The colour of this nucleus, instead of being very intensely brownish red, is of a rather darker brown than the unaltered mineral. The action of the acid manifested by the progressive darkening and lessening of the nucleus is, however, less prompt and less evident than when vanadite is acted upon, and resembles rather the alterations undergone by eosite. The difference lies, however, in the yellowish colouration of the solution around the crystal lying on the glass plate. Alcohol gives rise to the secretion of a gelatinous yellowish-green liquid, which, evaporated by heat, assumes a fine green or bluish-green tint, nearly identical to the tint assumed by vanadite under the same circumstances.

Dechenite, melted with bisulphate of potash, gives a deep brownish-yellow substance, reddening by cooling out, and finally becoming of a deep orange. This salt, heated in a platinum spoon with laminated tin, gives a faintly greyish-green solution.

The analogy between the vanadate of lead from Schlettenbach (Dechenite) and the vanadate from Windisch-Kappel (vanadite) goes still beyond their chemical characters. I submitted both these minerals to the action of the blowpipe, in order to find out, independently from my comparative studies concerning eosite, the cause of the prompt decomposition of the cabinet specimens from both these localities.

A splinter of Dechenite from Schlettenbach, heated in a glass recipient, emits, previously to its melting, a greenish-white vapour, not condensing into water drops nor in any other precipitate, upon the cooler portions of the recipient. The presence of arsenic is manifested neither by its characteristic smell nor by any specular sublimate. The mineral takes a darker tint when heated, and assumes again its original tint by cooling. It melts in the glass recipient, without previous decrepitation, into a brownish-yellow substance. Heated on charcoal, Dechenite melts easily, and with ebullition into a hollow dark steel-brown globule, giving at the same time a slight orange aureola and a whitish slag, including metallic granules. Melting with soda gives rise to a yellowish slag, including granules of metallic lead, and to graphitic vanadium imbedded in the charcoal. The slag imbibed with cobalt solution and heated to redness, assumes an unclean greenish tint and is surrounded by a greenish-yellow aureola, thus betraying the presence of a rather notable quantity of zinc, not mentioned in M. Bergemann's account of his analysis of Dechenite.

Among the vanadites from Kappel, only the carnation variety, B, seems to contain a notable proportion of zinc. Its tint darkens transitorily during heating, emits a faint vapour (of water) when heated in a glass recipient, without manifesting the presence of arsenic, and melts on charcoal into a dark greyish-brown, steel-bright globule, proving hollow when the flame of the blowpipe touches it. A notable aureola of oxidated lead appears, together with a small quantity of a dark slag, including globules of lead and graphitic vanadium, and assuming a green tint in contact with nitrate of cobalt or cobalt nitrate.

The darker variety, A, exhibits the same phenomena when heated in the glass recipient or by the blowpipe-flame, only the remaining slag is dark coloured, and only in one case among repeated experiments I could perceive on it some faint traces of green colouration. Possibly this variety may contain but a small proportion of zinc, as M. Damour has found only 2½ per cent of this metal in the Peruvian Descloizite.

The result of the before-detailed investigations, leaving previously aside the subsequent crystallographical facts, may be reassumed as follows:—Of both the varieties of vanadite from Kappel, the darker one (A) contains but a small proportion of zinc, and may be assimilated to the Peruvian Descloizite; the lighter one (B) is far richer in this metal, and may be identified with the Dechenite from Schlettenbach. Eosite differs from both these minerals as to its chemical characters.

Discussion of Results.

The morphological properties run parallel to the chemical characters of the minerals here in question. The results obtained may be concisely enounced thus:—

The crystalline form of eosite differs from those of Descloizite and Wulfenite, having the terminal lateral

* Several experiments intended for ascertaining directly the chemical character of these efflorescences remained without any satisfactory result.

angles of Descloizite united with crystallographical system of Wulfenite.

Eosite is a vanado-molybdate of lead.

The red varieties of Wulfenite have an admixture of chrome, but are not crystallographically different from the other varieties.

Descloizite is isomorphous to Anglesite; thus the results of the analysis of Descloizite seem to be questionable.

The dark variety of vanadite (A) from Kappel is identical to Peruvian Descloizite; the light variety (B) from the same locality is nearly identical in chemical characters to the Dechenite of Schlettenbach, and, as to the form of its crystals, to Descloizite.

These appear to me to be the result of the present investigations. They are still incomplete as to the crystalline forms of Dechenite. I would, however, congratulate myself if the hints given in their exposition could provoke some more attention for finding out distinctly crystallised groups of Dechenite.

TECHNICAL EDUCATION FOR THE SONS OF FARMERS.

MR. LITTLE has forwarded us a letter on this subject written to the members of the "Lincolnshire Farmers' Association," and other Agricultural Associations in Great Britain. In it he says—

It is indeed surprising that in a country where the practice of agriculture is carried to such a high degree of perfection, and where it is one of the chief sources of wealth, and is besides the means of employing such an immense amount of labour and capital, so little should be done towards the scientific education of those engaged in its pursuit. As a rule, agriculture is practised almost exclusively under the guidance of a slowly-earned experience, and mere traditional principles and habitual routine, without those engaged in it having any appreciation of the phenomena and natural laws which govern the growth and production of animal and vegetable food, the first necessities of man.

The success we have already arrived to in agriculture is, I believe, more mechanical than scientific. Drainage, steam culture, and a liberal use of capital and labour, are amongst the chief causes; but now that chemistry in its relation to artificial manures is taking such a prominent position, it is of the first importance that the future generation of farmers should have such a general knowledge of science as will enable them to correctly appreciate the value and properties of the various compounds offered by the too numerous chemical manure makers.

I cannot imagine a more dangerous, unfortunate, and lamentable position for any person to be in, whilst in the practice of his business, by which he hopes to gain his bread for himself and family, than being entirely dependent on the scientific skill and integrity of another man; or that his capital, time, labour, care, and hopes should be in many cases completely out of his own control. Such a state of things cannot—must not—last.

What I propose to do to correct this state of things is this—at a very moderate cost to give to an agricultural pupil a six or twelve months' course of scientific education and practical laboratory teaching after he has left his regular school; and I will engage that any boy of average ability, at the expiration of this time, shall have such a knowledge of all the materials employed in the compounding of chemical manures as will enable him to dictate what should be used without the interested interference of the manure maker. He shall, besides, have such an insight into the science and laws of chemistry as to make the reading and future study of scientific agriculture not only perfectly easy, but a delightful and intellectual employment.

To carry out this important object three conditions are necessary—a good qualified teacher, a laboratory, and pupils. The first could be had for a very moderate salary; the laboratory, with the necessary instruments and materials, would involve no serious outlay; the third condition, the pupils, gives rise to this question—would farmers send their sons to supplement their previous education by a six or twelve months' practical scientific instruction? The want of such knowledge amongst the present generation must be so strongly felt that I believe they would be too glad to have the opportunity, especially if they knew that probably a moderate fee would more than pay the costs.

The site of the laboratory might be anywhere—a small country village would in many respects offer advantages superior to a market town. The cost of erecting or hiring a building suitable for a laboratory, together with the instruments and materials, should be raised by subscription. The pupils' fees would, I have no doubt, pay all other charges, so that when once established it would ever after be self-supporting.

I am aware that certain schools exist where agricultural science is professedly taught, but I consider such a combination as almost waste time, the two kinds of teaching cannot be well carried on together, and what is most important, the mind must arrive to a certain maturity before it can grasp with sufficient reasoning power, the beautiful and wonderful phenomena arising out of laboratory practice.

If the foregoing observations should be thought worthy of the serious consideration of persons interested in agriculture, I shall indeed be glad to receive any communications or suggestions in promotion of the object of this letter; and if our members will support this object by the subscription of a sum equal to only a tenth of one year's savings effected through the agency of our association, I will devote myself most earnestly to establish a school laboratory in this village that I trust may serve as a model and example to be followed by many other localities in Great Britain.

I shall certainly not ask a single member of our Association to do that which I am not prepared to do myself. My consumption of phosphate manure—of course, I buy no mixtures or nostrums—is about twenty tons a year; and previous to the formation of our Association I paid to the most respectable makers £6 10s. per ton for manure containing 25 *per cent of water*. The percentage of soluble phosphate was entirely a matter of speculation; according to an elaborate report of Anderson and Way, from an analysis of 171 samples, the average percentage of soluble phosphate at that time was 15 per cent, and a ton of this manure was valued by these chemists at £7 5s. per ton. Therefore, if a 15 per cent manure was worth £7 5s., a 26 per cent manure would be worth £12 10s.; but our Association price for the 26 per cent manure is now £3 18s. per ton in bulk, delivered free at any station in Lincolnshire, with a further advantage of a watchful system of analysis, free of any cost, to ensure quality and dry condition.

My saving on these calculations would be £8 12s. per ton, or a total of £172 on a consumption of twenty tons yearly. I state facts just as they are recorded by the most eminent chemists, and every farmer will believe me when I say that a very large proportion of the manure sold at that time had little or no value whatever, consisting as it did of *dried mud and road scrapings*, flavoured with a little gas water just to flatter the olfactory nerves of the wise and cautious farmer of that period. A tenth of £172 would be £17 4s. for my contribution, but my requirements are much more modest. I think, therefore, I had better leave every member of our Association to form his own estimate of what he should contribute, suffice it to say that in the first instance the only contribution I ask for is a free and unprejudiced opinion as to the necessity and desirability of what I have in view—viz., the formation of a laboratory, in which agricultural

chemistry shall be taught at small cost, in a short time and in a practical way, to pupils who have received an ordinary education.

I have only alluded thus far to the material advantage to be derived from a brief course of scientific instruction. Allow me, in conclusion, to quote the language of one of the best and most highly gifted of our chemical philosophers as to its moral influence. The late Dr. Faraday says—"I do think that the study of natural science is so glorious a school that with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter, there cannot be a better school for the mind." Vain and foolish ideas, the fruit of ignorance, cannot be uprooted and destroyed by violence, the natural and more gentle method must be adopted, what in chemistry is called the law of substitution; the mind must be fertilised by knowledge, then truth and useful ideas will take the place of error and ignorant conceits. It is the absence of the exercise of the higher and intellectual faculties that leads so often to vacuity of the human mind, and the consequent indulgence in grosser and more material excitements, injurious alike to body and mind. A better form of education would eradicate the greatest of all human enemies—Ignorance and Intemperance.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

May 18th, 1871.

Professor Frankland, F.R.S., President, in the Chair.

Messrs. T. Greenish, and J. E. Mayall were elected Fellows.

The following papers were read:—

"*On a New Double Salt of Thallium*," by R. J. Friswell.

The author, wishing to prepare thallic platinocyanide, mixed hot solutions of thallic carbonate and potassic platinocyanide, and obtained, on leaving the mixture to cool, masses of splendid crystals, which appeared by transmitted light of a magnificent crimson-red, whilst their reflected colour was a bronzy green of strong metallic lustre. On treating these crystals with acids, they effervesced, evolving carbonic acid, and leaving a pale pink residue; thus showing that the salt was not the desired double cyanide. The thallic platinocyanide is obtained by decomposing baric platinocyanide with an equivalent quantity of thallous sulphate; it is yellowish-white and completely destitute of dichroic tints. The analysis of the first-mentioned salt suggests it to be a compound of thallic carbonate and thallic platinocyanide,—

$$Tl_2PtCy_4,COTlO_2.$$

When the residue, obtained after treating the salt with nitric acid, was washed, dried, and analysed, it gave figures corresponding to those required by theory for thallic platinocyanide. The precise form of the crystals has not yet been determined, but Mr. Friswell, believing the thallic platino-cyano carbonate to be a true double salt, supposes that it belongs to the trimetric system, since thallic carbonate and thallic platinocyanide belong to that system, and it is generally assumed that a double salt is isomorphous with its constituent salts. Mr. Friswell uses the term true double salt to denote a molecular combination and not a body of a more complicated nature like an alum.

In the following discussion the President remarked that he considered the new salt as an atomic combination, the platinum affording the link between the thallic cyanide and the thallic carbonate.

Professor Williamson, too, regarded it most probable that so great a number of atoms were bound together by atomic forces.

Mr. Spiller inquired whether the thallium was disguised in the salt, or whether it gave the usual reactions? which question the author answered affirmatively in the latter sense—it gave all the known thallic reactions.

The next paper read was "*On the Action of Nitric Acid on Dichlorophenol Sulphuric Acid*," by Dr. Armstrong.

CORRESPONDENCE.

ATOMIC BONDS.

To the Editor of the Chemical News.

Sir,—With reference to the discussion raised at the last meeting of the Chemical Society on the above subject, may I be allowed to adduce the instance of the use of lines, to mark direction and intensity of force, in that department of science which treats of mechanics?

The ease with which, by means of these symbols we are enabled to solve such problems, for example, as the composition and resolution of forces, is an argument for the use of similar symbols in giving graphic representations of the forces acting between atoms.

No one supposes that when two or more forces undergo composition or resolution, a visible, tangible, parallelogram, with its diagonal, exists, except upon paper; nor need we fear that the young chemist, unless of unusual dullness, will attach any more reality to the "bonds" which express the attraction of atoms.—I am, &c.,

B. J. G.

May 22, 1871.

GRAPHIC FORMULÆ.

To the Editor of the Chemical News.

Sir,—In the conversation which took place at the Chemical Society on Thursday last after the reading of Dr. Armstrong's paper, a discussion on graphic formulæ arose.

Dr. Williamson objected strongly to the materialistic idea which he asserted was induced by the use of "bonds" between symbols in a graphic formula. He made use of an alternative method and took for his example benzole, C_6H_6, which he represented thus—

H | H | H
C C C
C C C
H | H | H

This is supposed to be represented in space instead of on a plane surface, and to be a closed chain or triangular prism.

The objection to the bonds I think applies equally to this for if the bond produces the idea of a "strap or wire," surely the lines between the hydrogen atoms suggest a "wall" around or between them.

Either of these ideas, if they were produced, would be equally objectionable, and the only remedy would be to reject alike bonds and walls, and write graphic formulæ without either.

Any one who has had any experience in teaching can at once see what confusion this would entail. Even in memoirs and lectures it would be almost impossible to convey a real and definite idea of the constitution of many bodies, and, as a result, the present well-defined theory of "atomicity" or equivalence would give way before a sort of polyatomic theory in which every atom could combine with an unlimited number of like or unlike atoms, a result

which one cannot but feel would be attended by disastrous consequences in all attempts at teaching.

If, as I think is the case, wall or bond must be used, bond is certainly the best, for it represents a positive proposition, *i.e.*, that no combination exists.

I would respectfully submit to Dr. Williamson that a positive proposition is always to be preferred to a negative, —at any rate in teaching; firstly, because in the majority of cases it is more important as a fact; secondly, because a positive proposition is always more easily grasped and retained by the mind upon which it is impressed.—I am, &c.,

R. J. F.

PS. Is not the representation of a graphic formula in space making an assumption as to its form, and therefore more materialistic than merely writing down symbols on a plane surface?—R. J. F.

INTERMITTENT SPRINGS.

To the Editor of the Chemical News.

Sir,—Being at Buxton a few days ago, an excursion party was formed to visit the Peak Cavern, at Castleton. On our way thither, the "driver" suddenly pulled up and called our attention to a small "pond" by the roadside, into which was running a clear stream of water, which came out of a hill of considerable altitude, along the side of which hill the road ran.

Being thus admonished we all alighted, and were then informed by the "driver" "that the water of the spring intermitted, and that it was considered one of the 'wonders' of Derbyshire; that just as we arrived, he observed the stream had begun to flow; he added that he often brought visitors to see it, who had sometimes (like visitors to the Great Geyser in Iceland) to wait several hours to witness the flow and intermission of the stream."

On looking at the water, it at first seemed as if the stream was constantly running into the pond, and out at the other end, by a channel under the carriage road, till I observed that the stones in the pond began to be covered with the water, just like it is when the tide on the sea shore comes in. This showed that the "water level" of the pond was rising.

I then went to the other side of the pond and observed that the water came out of the hill on, or just below, the level of the surface of the pond, and a good deal seemed to rise from somewhat further below the surface.

The pond now became much fuller, and I should judge that the flow of water, while we watched it, was from 500 to 1000 gallons. The "driver" then informed us that the stream from the hill was about to stop, which, to our great surprise, it certainly did, and the surplus water draining out of the pond, the stones again appeared above the surface of the water, and all was still.

On considering the matter, the first impression on the mind was that some one high up the hill had opened a "flood gate" to let water run down to the pond, and then had suddenly shut it, but on looking at the hill no such hydraulic apparatus was visible.

The hill is composed of mountain limestone, and on attentively considering the matter, it occurred to me that the explanation of the phenomena is as follows:—

The limestone is full of cracks and hollows: when the rain falls on the hill it drains down in many channels, which all meet at a large hollow cavity, high up the hill. This cavity being cup-shaped, and impervious to water at the bottom, would accumulate in its interior a large body of water.

We have only then to imagine a channel rising up from the pond up the interior of the hill, which channel would be formed in the rock, going over the highest level of the water in the cavity, and the opening of the channel descending below the level of the water to the bottom of the cavity, exactly on the principle of the syphon, and the whole matter is explained.

Thus, suppose the cavity is empty—after a heavy shower of rain it slowly falls; if the supply continues, the water will rise "above" the level of the bridge of the syphon, and if there is sufficient water overflowing to perfectly fill the "longer leg" which goes down to the pond, the pressure of the atmosphere on the surface of the water will push all the water out of the cavity down the syphon, which water will then appear as a good stream running into the pond. When all the water has run out of the cavity, the stream of course ceases.

In very dry weather, if the supply of water was but just sufficient to keep the cavity full, and not enough to perfectly fill the "longer leg," the surplus water would slowly trickle over the bridge, without starting the syphon, and the stream being very small would not be observed; hence visitors may go to see the spring commence running and be disappointed. On the other hand, if it is very wet weather, there may be sufficient water to fill the syphon, and thus keep the spring constantly running without emptying the upper cavity.

The most favourable time to see the commencement and intermission of the stream should be in moderately dry weather, when there is sufficient water in the cavity for the syphon to draw off two or three times a day.

It requires some little "tact" to make a syphon which shall start itself, and it is certainly very remarkable, that by the natural crevices, channels, and cavities, contained in the mountain limestone a self-acting syphon should be formed on a somewhat grand scale.

How long this intermittent spring has been running can only be guessed at, but I observed that there are in the pond some "worked stones" which form a sort of sunk trough into which the spring runs out of the hill; this trough looks very old, and has no doubt been there for ages.

There is a "tumulus" on a neighbouring hill, and the ancient military earthwork on "Mam Tor" is not far distant.—I am, &c.,

Alfred Bird, F.C.S.

Birmingham, May 18, 1871.

ATOMIC VOLUMES.

To the Editor of the Chemical News.

Sir,—It is generally assumed in modern chemical works, that the weights of the atomic volumes of the elements, with certain rare exceptions, such as arsenic and phosphorus, are the same respectively as their combining weights. Thus, for instance, Dr. George Wilson, in his useful little treatise on "Inorganic Chemistry," observes that "an atom of hydrogen, or 1, taken as unity, will occupy, a volume or measure represented by a single square; an atom of chlorine, or 35·5, will be exactly enclosed in the same measure or square; an atom of oxygen, or 16, will only fill the same volume or square; and so in the case of the atom of nitrogen, 14, and the atom of carbon, 12."

When, however, we consider attentively the constitution of compound gases, we shall find that many difficulties are involved in the theory thus enunciated. Let us take, for example, hydrochloric acid, HCl, formed, without condensation, of one atomic volume of hydrogen, 1, and one atomic volume of chlorine, 35·5. Now, as each of the two volumes thus produced must contain both hydrogen and chlorine, it follows that both the single volumes of these gases are in the combination halved, or equally divided, otherwise an uniform double compound volume could not result.

Again, in the case of water, H_2O, we have two atomic, volumes of hydrogen, 1, combined with one atomic volume of oxygen, 16; forming two volumes of aqueous vapour. Here the single atomic volume of oxygen, like the H or Cl in HCl, must have been divided into two to become a constituent of the normal product volume represented by H_2O.

As another simple illustration, I may refer to ammonia, H_3N, formed of three atomic volumes of hydrogen, 1, and one atomic volume of nitrogen, 14, condensed into two compound volumes. Applying similar reasoning, we infer that the single atomic volume of nitrogen thus combined must also be halved or divided, both the volumes of H_3N containing this element. Indeed, we might even conceive the atomic volume of nitrogen in H_3N to be divided into three parts, inasmuch as it is chemically and equally associated with each of the three atomic volumes of hydrogen. But, at least, we seem, as I have said, to be justified in regarding the N atomic volume as divided into two in the compound double volume H_3N.

These considerations naturally lead to the conclusion that the so-called atomic volumes are strictly multiples of the true combining elementary volumes; in other words, that in order to form the double standard compound volume, the atomic volumes must, generally speaking, be halved or divided, and that, consequently, the numbers representing the weights of such combining elementary volumes should be really the *halves* of those that now represent the atomic volume weights or the respective combining weights.

This theory does not in the least affect the proportions by weight in which the volumes, erroneously according to it termed *atomic*, unite, seeing that in the case of hydrochloric acid, for example, we have only to consider the present atomic volume weights of the hydrogen, 1, and of the chlorine, 35·5, as denoting double the elementary or *true* atomic volume weights.

Hoping that these few and imperfect observations may lead to further inquiries concerning that important and interesting subject, the combination of gases—I am, &c.,

W. H. O.

Mildmay Road, Stoke Newington, May 19, 1871.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

American Journal of Pharmacy, May, 1871.

This number being the first published under the editorship of Dr. J. M. Maisch, who has succeeded Mr. Procter, we must not omit to compliment the new editor, and express our sincere wish that he may earn as great a success as his excellent predecessor, and, like him, hold this important post for a long series of years. In addition to a series of papers, the contents of which relate strictly to pharmacy, we meet in this number with the following original papers and memoirs bearing upon chemistry:—

Preservation of Pharmaceutical Apparatus from Breakage by Change of Temperature.—R. Simpson.—The contents of this paper, which contains the detailed account of a series of experiments made by the author, may be briefly summarised as follows:—Every one is aware that the pouring of hot liquids into thick glass vessels is frequently attended with the breakage of the same. The author states that, if metallic rods are so placed in these vessels that the rods do not touch any part of the sides thereof, and the hot liquid poured alongside the rods, no breakage takes place, owing to the absorption of heat from the liquid by the rod, which, to ensure success, should be placed on the centre of the bottom of the vessel and be held perpendicularly. The thickness of the rods should be about $\frac{1}{4}$ of an inch, and the metal they are made of may vary to suit the quality of the liquid—iron, brass, copper, glass, but the latter must be used with great care, on account of their inferior absorbing power.

Preservation of the Oils of Oranges and Lemons.—C. Fruh.—The author adds to every pound of oil 1 oz. of pure alcohol, which, having been thoroughly mixed with the oil, there is added to it 1 oz. of water, which again withdraws the alcohol from the oil, and collects at the bottom of the bottle as dilute alcohol. The *rationale* of this process appears to be that the alcohol, by removing foreign resinous and other matters present in the oil, retards the oxidation of the oil by the atmosphere; but the author is engaged in further elucidating this point by continuing a series of experiments on this subject.

Uva Ursi.—Dr. J. Jungmann.—This essay contains an account of the author's researches on this well-known material, the leaves of the *Arctostaphylos uva ursi*, the bear-berry. After reviewing the botanical description of the tree which yields this drug, and other matters strictly belonging to *materia medica*, the author reviews the analyses made since 1809, and then exhaustively relates his own experiments, and the mode of preparation and properties, first of arbutine, discovered in 1852 by Dr. Kawalier. The most prominent feature in relation to this glucoside is a remarkable test the author discovered for its detection. When a solution of arbutine in water is rendered alkaline by ammonia or any other caustic or carbonated alkali, and then phospho-molybdic acid is added, a blue colour is produced. In strong solutions, the colouration is of a deep azure blue, but the bluish hue can be observed even in very dilute solutions (1 in 140,000). This reaction does not occur with molybdate of ammonia, nor does it take place when phosphoric or phospho-molybdic acid is acted upon by an alkali alone. From the mother-liquor of arbutine, when heated with a dilute acid (sulphuric or hydrochloric), a resinous body separates, ericolin; also a glucoside, which, when treated with a dilute acid, splits into grape sugar and an odorous substance having the character of a volatile oil. Ericolin is a dark brown resin, becoming somewhat lighter when dried and rubbed to powder; its chemical composition is $C_{68}H_{56}O_{42}$; its decomposition into ericinol is exhibited by—

$$\underbrace{C_{68}H_{56}O_{42}}_{\text{Ericolin.}} + 8HO = \underbrace{C_{20}H_{16}O_2}_{\text{Ericinol.}} + \underbrace{4C_{12}H_{12}O_{12}}_{\text{Grape sugar.}}.$$

The folia *Uvæ ursi* are found to contain the following organic constituents:—Arbutine, decomposable into hydrokinone, ericolin, ericinol, urson, tannic, gallic, and malic acids, a small quantity of volatile oil, fatty matter, wax, gum, sugar, albumen, colouring matter, and cellulose.

Crab-Orchard Salt.—J. T. Viley.—This salt is obtained from the waters near Crab-Orchard, a small town in Lincoln County, Kentucky. The most interesting part of this lengthy paper are the results of the analysis, viz.:—Sulphate of magnesia, 44·778; sulphate of lime, 1·277; sulphate of potassa, 1·871; sulphate of soda, 6·483; chloride of lithium, 0·049; chloride of sodium, 0·412; water, 44·655; insoluble matter and ferric oxide, 0·520. Dried at 120°, the salt contains—Water of crystallisation, 18·933; sulphate of magnesia, 65·463; sulphate of potassa, 2·735; sulphate of soda, 9·480; chloride of sodium, 0·602; chloride of lithium, 0·072.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 6, 1871.

This number contains the following original papers and memoirs:—

Hydrate of Sulphide of Carbon.—Dr. Ballo.—This paper is a refutation of the assertions made by M. V. Wartha against the correctness of this author's researches; but it does not contain anything new on the above-named subject, the author's further researches in that direction not having been completed.

New Series of Aromatic Hydrocarbons.—Dr. T. Zincke.—After first referring, at some length, to a series of investigations made some years ago on the action of finely-divided copper upon benzyl-chloride, the author in this paper states that, by treating benzol-chloride and benzol, first with finely-divided zinc, and next employing fractional distillation, he obtained a new hydrocarbon, exhibiting an orange-like smell, consisting of $C_{13}H_{12}$, crystallising; fusing at 25°; boiling and distilling over, without decomposition, at 262°; readily soluble in alcohol, ether, and chloroform. With bromine it yields a substitution compound, while it yields a nitro compound with strong nitric acid, and on being oxidised, which it does with great difficulty, it yields, along with some benzoic acid, a substance of the same composition as benzo-phenon. Toluol and benzyl-chloride yield, in the same manner, a hydrocarbon boiling at 277°. This new body is, as regards its properties, very similar to that just alluded to, but it is a fluid (sp. gr. 0·995 at 17°). The structural formula of the first-named hydrocarbon is $C_6H_5--CH_2--C_6H_5$, while the structural formula of the second is $C_6H_5--CH_2--C_6H_4--CH_3$.

Constitution of Alizarine and Naphthazarine.—M. T. Petersen.—Notwithstanding the high scientific value of this essay, it is not possible to give any further notice of it here, because for the due understanding of the contents it would be requisite to reproduce a lengthy and very complex series of formulæ, which our space forbids us to do.

Heat of Neutralisation of the Inorganic and Organic Bases Soluble in Water.—Dr. J. Thomsen.—Among the results obtained by the researches described in this lengthy essay we quote the following:—Lithium, sodium, potassium, and thallium, when dissolved in water in the state of hydrates, yield, when neutralised with sulphuric acid, almost identically the same quantity of caloric, the average number for the four being 31,130 c., and no greater difference exists between the values of the four figures than 5 per mille. As regards the alkaline earths—baryta, strontia, lime, magnesia—the average number is 31,140 c., and the difference between the four figures is even less than in the former case. Consequently, the author draws the following conclusion:—That the molecule of sulphuric acid yields, by neutralisation with the alkalies and alkaline earths, a quantity of caloric which is almost precisely the same, and amounts, on an average, to 31,134 c. The paper further treats on ammonia and compound ammonias, in relation to the quantity of heat they yield by neutralisation with acids.

Caloric Action of Water.—Dr. F. Mohr.—The main object of this exhaustive memoir is to prove that what the author has set forth as the mechanical theory of chemical affinity holds good, and is applicable for the explanation, also, of the phenomena observed when salts are dissolved in water, that is, according to the author, have combined with water. When water, while combining with bodies, obtains a higher temperature, the boiling-point of the water has risen; but when water, by combining with a body, has become lowered in temperature, the freezing-point of water has been lowered. The author speaks in this paper of a large work on the subject here referred to, and published by him, but he does not quote the title.

Contribution to the History of the Benzol-Sulpho Acid.—M. Ascher and V. Meyer.—This paper treats on what is here termed Schmitt's benzol-sulpho acid, obtained from the diazo derivative of sulphanilic acid, according to the following formula:—

$$C_6H_4\left\{\begin{matrix}SO_3\\ \\ N\equiv N\end{matrix}\right\}+C_2H_6O=C_2H_4O+N_2+C_6H_5SO_3H.$$

The authors, having prepared the benzol-sulpho acid referred to from the diazo-derivative of the sulphanilic acid, proceed to record, in this paper, a series of results of experiments, all of which tend to refute the opinion expressed by M. Schmitt, that the benzol-sulpho acid obtained by him, and above referred to, should not be identical with the ordinary benzol-sulpho acid. This identity is proved by the composition of the potassa salt, the chloride, the amide, the anilide yielded by the acid under investigation, and lastly, also, by the solubility in water, both benzol-sulpho acids dissolving at 16 in 1000 parts of water to the same extent.

Application of the Spectroscope for the Quantitative Estimation of Colouring Matters.—M. K. Vierordt.

On Hæmatoxyline.—Dr. F. Reim.—From this lengthy monograph we quote the following main results of the author's researches on this subject:—Hæmatoxyline is not a body constituted as phloroglucin, but it is a derivative of the so-called aromatic series. Towards weak oxidising agents it behaves as a hydrochinon, since the hæmatine which is formed stands to the hæmatoxyline in the same relation as the chinon does to hydrochinon. Lastly, hæmatoxyline contains six hydroxyl groups. The structural formula for hæmatoxyline is—

$$\begin{matrix}C_6H_2(OH)_3\\ |\\ \cdot\\ |\\ C_4H_4\\ |\\ \cdot\\ |\\ C_6H_2(OH)_3\end{matrix}$$

and the similar formula for hæmatine is—

$$\begin{matrix}C_6H_2(OH)_2O\\ |\\ \cdot\\ |\\ C_4H_4\\ |\\ \cdot\\ |\\ C_6H_2(OH)_2O\end{matrix}$$

A Solvent for Indigo-Blue.—Dr. V. Wartha.—The author states that Venetian turpentine, heated to its incipient boiling-point, is a solvent for indigotine, which, after cooling, is readily purified by the aid of ether or alcohol. Boiling paraffin also dissolves indigo-blue (indigotine), and the paraffin can be removed, after cooling, by means of benzol. Spermaceti, stearic acid, and chloroform, all at a high temperature, are stated to be more or less good solvents for indigo.

On Benzoine.—Drs. Limpricht and Schwanert.—The contents of this paper records a series of experiments on the action of alcoholic potassa solution upon benzoine, and the formation of benzilic acid as an effect of the simultaneous action of the atmosphere, since in sealed tubes only benzil is formed. The formation of ethyl-dibenzoine and acetyl-ethyl-dibenzoine from benzoine is elucidated by a series of lengthy and complex formulæ; while, lastly, the generation of oxylepiden from benzoin by the action thereupon of hydrochloric acid in sealed tubes is explained.

Lecture-Room Apparatus.—J. Y. Buchanan.—And on the—

Formation and Decomposition of some Chlorited (Chlorirten) Acids—J. Y. Buchanan—Are papers which cannot be well abstracted, since to both are added diagrams and woodcuts.

Chemical and Mechanical Change of the Haloid Salts of Silver under the Influence of Light.—C. Schultz-Sellack.—The author communicates a series of experiments made with the object of proving that, while the chloride, bromide, and iodide of silver are acted upon by light, and altered in a well-known manner, this action is due to a phenomenon of dissociation, called forth by the light; but when these salts are put in sealed tubes, and care taken to have an excess of the haloids in free state present, and the tubes exposed to light, a mechanical change is effected, since the previously crystalline salts—obtained in that state from the ammoniacal solutions of the chloride and bromide and hydriodic acid solution of the iodide of silver—become at first turbid, and next crumble to powder. This mechanical change of the haloid salts of silver is greatest when the chemical change is smallest.

Formation of Lactic Acid from Sugar without Fermentation.—F. H. Seyler.—After first referring to the action of caustic alkalies, aided by heat, upon grape sugar, cane sugar, milk sugar, and cellulose, the author states that when 1 lb. of grape sugar is placed, along with half a litre of caustic soda solution (1·34 sp. gr.) in a capacious retort, an equal volume of water added, and the retort heated in a water-bath, a very strong reaction sets in at 96°, the temperature rises to 116°, the mixture boils violently, and, though no gas is evolved, a pleasant smell ensues. If, after the exact neutralisation of the soda by means of sulphuric acid, the liquid is concentrated by evaporation, and next treated with ether, that fluid takes up, among other substances, lactic acid, which is removed from the ether by shaking it up along with water and carbonate of baryta, and purified by being converted into lactate of zinc.

Review and Critical Observations on the Essay "On the Products of Distillation of a Mixture of Butyrate and Acetate of Lime," published by Dr. F. Grimm.—Dr. A. Ladenburg.

Contribution to the Question on the System of the Elements.—Dr. D. Mendelejeff.—A review of the labours of Dr. Odling, MM. Gerstl, Blomstrand, L. Meyer, and others on this subject, as compared with suggestions and papers published thereupon by the author in Russian.

Phosphorus Compounds.—Drs. E. Drechsel and Finkenstein.—The authors describe at length the mode of preparation and properties of phosphide of zinc, obtained from an etherial solution of zinc-ethyl by the action of phosphuretted hydrogen. The body in question is exceedingly prone to decomposition, and can only be kept at a low temperature and in hermetically-closed bottles; it is a solid substance, and has been used by the authors for the purpose of preparing various organic phosphorus compounds, by means of double decomposition with chloride of acetyl, iodide of ethyl, iodide of methyl, and triethyl-phosphonium zinc iodide.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin, March, 1871.

This number contains, in addition to some papers by Dr. A. W. Hofmann, the contents of which we have abstracted from other German periodicals wherein these papers have been also published, the following original papers bearing upon physico-chemical sciences:—

Action of the Secondary Currents (Nebenströme) of the Electric Battery on the Main Current and on each other.—Dr. Riess.—This memoir is divided into the following sections:—Counter action (*rückwirkung*) of a secondary current on the main current; counter action of two secondary currents upon the main current; action of two secondary currents upon each other.

Behaviour of Agate in the Magnetic Field, between the Poles of a Powerful Electro-Magnet.—Prof. Dove.—The author describes, in this paper, a series of experiments made with so-called mountain quartz and polished plates cut from agate, with the view to ascertain the diamagnetic properties of these minerals. It appears that all varieties of quartz exhibit unmistakably diamagnetic properties.

NOTES AND QUERIES.

Removing Sulphur from Coke.—Can any of your readers inform me of a process for the removal of sulphur from coke by steeping it in a solution of brine before ignition in the furnace, also giving the changes thereby produced.—R.

The New Solvent for Indigotine.—The use of aniline as a solvent for indigotine, as quoted in the CHEMICAL NEWS, vol. xxiii., p. 214, and in the *Journal of the Chemical Society* for the present month, from the *Ann. Ch. Pharm.*, is by no means a new discovery on the part of Drs. A. A. de Aguiar and A. Bayer. So far back as 1861 an editorial article in the CHEMICAL NEWS (vol. iii., p. 255) contains the following passage:—"Hot aniline dissolves indigo, forming a solution having almost exactly the tint of the solution of azuline in methylated spirit. On cooling, the indigo crystallises out in beautiful coppery spangles. This method of crystallising indigo from a solution in boiling aniline does not appear to be generally known to chemists." I have a strong objection to English discoveries being re-discovered on the Continent, and that is my only plea for encroaching on your valuable space.—R. MELDOLA.

MEETINGS FOR THE WEEK.

TUESDAY, 30th.—Royal Institution, 3. Rev. Prof. Haughton, "On the Principle of Least Action in Nature."
—— Civil Engineers, 8.
WEDNESDAY, 31st.—Society of Arts, 8.
THURSDAY, June 1st.—Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
—— Chemical, 8. Dr. Debus, "On Ozone."
FRIDAY, 2nd.—Royal Institution, 9. Prof. Andrews, on "Gaseous and
—— Liquid States of Matter."
—— Geologists' Association, 8.
SATURDAY, 3rd.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."

NOTICE TO AMERICAN SUBSCRIBERS.

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THE CHEMICAL NEWS.

VOL. XXIII. No. 601.

FURTHER REMARKS ON BRITTLE SILVER.

By A. H. CHURCH, M.A.

I AM indebted to Mr. R. Warington for a reference to a note by his father on this subject. An analysis of a portion of a (Romano-British ?) silver urn (found near Bow, and weighing about 40 ounces) gave results which closely resembled those described by me in last week's CHEMICAL NEWS. The metal was, however, so thin that the whole substance of it had suffered a molecular change, and become crystalline and brittle. The late Mr. Warington interpreted the experimental results which he obtained exactly in the same manner as I do, attributing the brittleness of the silver to molecular disturbance, not to chemical change. He, however, did not separate the crust from the rest of the metal, and so obtained less conclusive proof of his view. If we translate his analytical numbers (*Mem. Chem. Soc.*, ii., p. 47, 1843) into percentages, after deduction of the silver chloride and ferric oxide which constituted the outer film of the object, they show the true metallic character of the brittle material.

Analysis of Brittle Silver, by the late Mr. R. Warington, F.R.S.

	Metal with crust. Per cent.	Metal crust deducted. Per cent.
Silver	90·01	96·74
Copper	2·83	3·03
Gold and loss	0·33	0·23
Silver chloride ..	6·12	—
Ferric oxide	0·71	—
	100·00	100·00

RESEARCHES ON THE HYDROCARBONS OF THE SERIES C_nH_{2n+2}.*

By C. SCHORLEMMER.

IN a former communication I have shown that the paraffins, the constitution of which is known, may be arranged in four groups. The first group, which I called *normal paraffins*, contain the carbon atoms linked together in a single chain. Of these I have obtained some new ones, which I shall describe more fully in a further communication. The normal paraffins which I have so far studied are given, together with their boiling-points, in the following table :—

	From petroleum.	From the acids of the series $C_nH_{2n-2}O_4$.	So-called alcohol radicals.	
C_5H_{12}	37°— 39°	—	—	
			Dipropyl.	From mannite.
C_6H_{14}	69— 70	69·5°	69°—70°	71·5°
C_7H_{16}	98— 99	100·5	—	—
				From methyl-hexylcarbinol.
			Dibutyl.	
C_8H_{16}	123—124	123—124	123—124	124

That these paraffins have really the constitution which I have ascribed to them follows partly from their mode of formation; thus dipropyl was obtained from the normal propyl iodide, and dibutyl from normal butyl iodide. The constitution of the others was determined by converting them into alcohols and studying the oxidation products of the latter; thus the hexyl hydride from petroleum, as well as that obtained from mannite, was transformed into secondary hexyl alcohol, which on oxidation yielded acetic acid and *normal* butyric acid.

In the communication above referred to, I placed the hydrocarbon C_8H_{18} from methyl-hexyl carbinol amongst another group; but I have found now that this body is identical with dibutyl and also with the hydrocarbon, which Zincke obtained from primary octyl alcohol. This chemist prepared also dioctyl, $C_{16}H_{34}$, which consequently is a normal paraffin; and it appears probable that dihexyl, which Brazier and Gosleth obtained by the electrolysis of œnanthylic acid, belongs to this group too.

We are now acquainted with the following normal paraffins:—

	Boiling-points. Found (mean).	Calculated.	Difference.
CH_4	—	—	
C_2H_6	—	—	
C_3H_8	—	—	
C_4H_{10}	1°	1°	
C_5H_{12}	38	38 ..	37°
C_6H_{14}	70	71 ..	33
C_7H_{16}	99	100 ..	29
C_8H_{18}	124	125 ..	25
$C_{12}H_{26}$	202	207 ..	4×19
$C_{16}H_{34}$	278	278 ..	4×19

From this it appears that the boiling-point is not raised 31° for each addition of CH_2, as I formerly assumed, but that, as the calculated numbers show, the difference between the boiling-points of the lower members decreases regularly by four until it becomes the well-known difference of 19°.

* Read before the Royal Society, May 19, 1871.

ON THE SPECIFIC GRAVITY OF AQUEOUS HYDRIODIC ACID.*

By Dr. C. R. A. WRIGHT.

A SHORT time ago a note on the density of aqueous solutions of hydrobromic acid was communicated by the writer to this Society (CHEMICAL NEWS, vol. xxiii., p. 242): it appeared to be of interest to determine also the relation between the specific gravity and strength of hydriodic acid, so as to compare the three curves got with these two acids and also with hydrochloric acid.

Diluted hydriodic acid was got, in the first instance, by the action of sulphuretted hydrogen on iodine in presence of water; in this acid iodine was dissolved, and the whole digested at a very gentle heat with phosphorus; finally, the colourless liquid was distilled twice or thrice successively, the observations being made with the fresh distillate containing no traces of free iodine. The strength was found by a standard alkaline solution, and the specific gravity by weighing a known volume. The results, which only pretend to a moderate degree of accuracy, are as follow:—

Percentage of HI.	Sp. gr. at 15° C.
51·9	1·708
47·2	1·551
39·2	1·442
30·3	1·297
18·5	1·175
5·9	1·053

By graphical representation these numbers are found to agree tolerably together, yielding a *mean curve*, from which the following table is constructed;—

* Read before the Newcastle-upon-Tyne Chemical Society, March 23, 1871.

Percentage of HI.	Sp. gr. at 15°.	Difference for 5 per cent.	Difference for 1 per cent.
0	1·000	—	—
5	1·045	0·045	0·0090
10	1·091	0·046	0·0092
15	1·138	0·047	0·0094
20	1·187	0·049	0·0098
25	1·239	0·052	0·0104
30	1·296	0·057	0·0114
35	1·361	0·065	0·0130
40	1·438	0·077	0·0154
45	1·533	0·095	0·0190
50	1·650	0·117	0·0234
52	1·700	—	0·0250

From these numbers it appears, firstly, that the increase in sp. gr., as with hydrobromic acid, is not proportionate to the increase in strength; and, secondly, that the density of aqueous acid containing a given percentage, x, of HI is, as might be expected, greater than that of aqueous hydrobromic acid containing x per cent of HBr; this, again, being greater than that of hydrochloric acid containing x per cent of HCl.

The following table shows, comparatively, the sp. grs. of the three acids of different strengths, along with the values for hydrobromic acid calculated from the preceding formula:—

Percentage of Anhydrous Acid.	Specific gravity.			
	Hydrochloric.	Hydriodic.	Hydrobromic (found).	Hydrobromic (calculated).
5	1·0244	1·045	1·038	1·039
10	1·0488	1·091	1·077	1·078
15	1·0734	1·138	1·117	1·119
20	1·0981	1·187	1·159	1·159
25	1·1233	1·239	1·204	1·203
30	1·1484	1·296	1·252	1·249
35	1·1738	1·361	1·305	1·300
40	1·1966	1·438	1·365	1·356
45	—	1·533	1·435	—
50	—	1·650	1·515	—

It might, perhaps, be expected that the percentages of anhydrous acids, for the same sp. gr., would be in the inverse proportion of the atomic weights of the anhydrous acids, viz.:—

$$HCl = \frac{1}{36{\cdot}5} \qquad HBr = \frac{1}{81} \qquad HI = \frac{1}{128}$$

or taking the atomic weight of HCl as under:—

$$HCl = 1 \qquad HBr = 0{\cdot}450 \qquad HI = 0{\cdot}285$$

this, however, is not the case: thus the sp. gr. 1·050 corresponds to 10·2 per cent HCl, 6·5 per cent HBr, and 5·5 per cent HI, or HCl = 1, HBr = 0·64, HI = 0.54.

Similarly the sp. grs. 1·100, 1·150, 1·200 give the following ratios:—

Sp. gr.	Percentage of HCl.	Relative Percentage of HBr.	Relative Percentage of HI.
1·050	1·000	0·64	0·54
1·100	1·000	0·63	0·54
1·150	1·000	0·62	0·53
1·200	1·000	0·60	0·52

The preceding experiments were performed in the laboratory of Messrs. Bell Brothers, Washington.

ON SOME NEW DERIVATIVES OF ALBUMEN.*

By O. LOEW.

IN regard to those derivatives or products of substitution of albumen, which stand nearly related to albumen, our knowledge is as yet very imperfect, and the principal radical complexes of the proximate constituents of albumen are not known at all. The difficulty of obtaining some insight into the composition of albumen is chiefly due to the facility with which albumen is split up, by different chemical agencies, into many simpler substances, as tyrosine, leucine, benzoic and caproic acids, or even uncrystallisable so-called extractive matters, of which it is difficult to obtain an exact chemical formula.

Some time ago, I endeavoured to obtain products of substitution of albumen, and at last succeeded in forming several derivatives, among them a nitro-product hitherto unknown. As regards the action of fuming nitric acid upon albumen, we only know that the latter is oxidised to an acid, called by Mulder xanthoproteic acid, for which, however, no exact formula has been given. In my experiments, a mixture of one volume of fuming nitric and three volumes concentrated sulphuric acid were employed. If less of the latter was used, an oxidation, in addition to the substitution, would occur.

30 grms. of finely-powdered albumen were gradually added to a cool mixture of 90 c.c. fuming nitric acid, and 270 c.c. concentrated sulphuric acid, shaking well after each addition, and taking care that the mixture remained cool. The albumen was slowly dissolved to a clear solution without any disengagement of nitrous acid fumes, and the solution, contained in a vessel surrounded by cold water, was, after a lapse of ten hours, poured into about fifteen times its volume of water. The voluminous flocculent precipitate resulting was immediately filtered, washed, lastly with hot water, and dried with care. The quantity obtained amounted to nearly two-thirds of the albumen employed; another part of the albumen is converted into a soluble product, which, after neutralisation of the acid filtrate, gives a precipitate with tannin.

The powder thus obtained is of a light yellow colour and a slightly bitter taste. It is insoluble in water, dilute acids, and alcohol, but is soluble in dilute alkalies, forming a reddish solution. On heating these alkaline solutions a decomposition begins and new products are formed, which I have not thus far examined. On heating the powder in a dry test-tube, it behaves like the original albumen, empyreumatic vapours being developed, accompanied by cyanogen, and carbon remaining behind.

On analysis, this new derivative yielded the following figures:—

I. 0·680 grms. gave 0·144 barium sulphate (determined by Kolbe's method) = 2·92 per cent sulphur.
II. 0·912 grms. gave 0·200 barium sulphate = 3·03 per cent sulphur.
I. 0·269 grms. gave 39·30 c.c. nitrogen at 741·5 m.m. barometer, and 9° C. = 17·20 per cent nitrogen.
II. 0·268 grms. gave 38·80 c.c. nitrogen at 742 m.m. barometer, and 9° C. = 17·00 per cent nitrogen.
I. 0·366 grms. gave 0·184 grms. water and 0·594 grms. carbonic acid = 5·57 per cent hydrogen and 44·26 per cent carbon.
II. 0·380 grms. gave 0·184 grms. water and 0·614 grms. carbonic acid = 5·58 per cent hydrogen and 44·01 per cent carbon.

These figures correspond to an albumen in which 6 atoms of hydrogen are replaced by 6 atoms of the nitro-group, and 1 atom of hydrogen by 1 atom of the sulphoxyl group. The generally adopted formula of albumen is

$$C_{72}H_{108}N_{18}SO_{22}.$$

(Lieberkuhn has in his formula four more atoms of hydrogen.) The formula of the new derivatives is therefore:—

$$C_{72}\left\{\begin{matrix}H_{101}\\(NO_2)_6\\SO_2OH\end{matrix}\right\}N_{18}SO_{22}$$

		Theory.	Experiment. I.	Experiment. II.
C_{72}	864	44·13	44·26	44·01
H_{102}	102	5·21	5·57	5·58
S_2	64	3·27	2·92	3·03
N_{24}	336	17·16	17·20	17·00

* Read before the Lyceum of Natural History of New York.

The rational name of this compound is, therefore, hexanitro-albumen-sulphonic acid. We know two different classes of nitro-bodies; the one class yields, by treatment with reducing agents, the original substance, as nitro-cellulose and nitro-glycerine: the other class yields amidated products, as in the case of nitro-benzoic acid. The cause of this difference in behaviour is, that in the first case the hydrogen of an hydroxyl is replaced by the nitro-group, while in the second case the hydrogen in combination with the carbon is replaced by the nitro-group. To decide to which class the hexanitro-albumen-sulphonic acid belongs, it was treated with sulphide of ammonium. After many attempts to isolate the product of reduction, I adopted the following as the best method: hexanitro-albumen-sulphonic acid is dissolved in not too great an excess of ammonia and a current of sulphuretted hydrogen gas passed through it for some time. The solution, saturated with sulphuretted hydrogen, is heated in a flask over a water-bath, till all sulphide of ammonium is volatilised (I cannot recommend evaporating in a porcelain dish, because the free access of air would effect an oxidation of the newly-formed product). The fluid is then filtered from the deposit of sulphur, and to the filtrate acetic acid is added till the formation of a precipitate ceases; this precipitate is then well washed with water and alcohol, and carefully dried.

Analysis of this precipitate resulted as follows:—

0·743 grms. gave 0·211 barium sulphate = 3·92 per cent sulphur.

0·248 grms. gave 44·13 c.c. nitrogen, at 14° C. and 742 m.m. barometer = 19·02 per cent nitrogen.

0·331 grms. gave 0·584 grms. carbonic acid and 0·203 grms. water = 48·12 per cent carbon, and 6·87 per cent hydrogen.

We have, therefore, all the (NO_2) here replaced by (NH_2), thus forming the corresponding hexamido-albumen-sulphonic acid—

$$C_{72}\begin{Bmatrix} H_{101} \\ (NH_2)6 \\ SO_2OH \end{Bmatrix} N_{18}SO_{22}$$

	Theory.		Experiment.
C	48·593	....	48·12
H	6·412	....	6·87
N	18·897	....	19·02
S	3·599	....	3·92
O	22.497	....	—

This compound is a yellowish-brown powder, almost tasteless, insoluble in dilute acids, soluble in concentrated hydrochloric acid; alkalies dissolve it readily, but on the application of heat a decomposition takes place, with development of ammonia. Nitric acid attacks it with energy and dissolves it, with disengagement of nitrous fumes. I made many attempts to obtain crystalline combinations of this substance, but in vain.

Another Derivative of Albumen with Concentrated Sulphuric Acid.—It is well known that albumen swells up considerably in concentrated sulphuric acid, but no investigation has been made as to what this product really is. I mixed in a mortar 1 part of albumen gradually with 10-15 parts of concentrated sulphuric acid, and allowed it to stand in the cold for one day; the mass was then treated with cold water till the free SO_4H_2 was all removed; the flocculent mass was placed in a dilute potassa solution (1 to 6—8), stirred sometimes, allowed to stand in the cold for one day, filtered, precipitated by acetic acid, the precipitate washed and dried. Analysis showed that it is albumen-sulphonic acid. It contains twice as much sulphur as the albumen itself, is insoluble in water, alcohol, and dilute acids, but soluble in alkalies, wherein it swells up considerably before the real solution slowly takes place. On boiling these solutions, decomposition takes place. I shall make further investigations on this subject.

ON THE ELECTROMOTIVE POWER OF METALLIC SULPHIDES.*

By WILLIAM SKEY,
Analyst to Geological Survey of New Zealand.

In this paper the author shows by a series of experiments that several of those sulphides which are conductors of electricity have the capacity of generating strong electric currents in those solutions frequently having contact with them in a natural way.

These currents when generated from a single pair, even of dissimilar sulphides, have an intensity so great that when allowed to pass into certain metallic solutions these solutions are decomposed, the metal being deposited in a coherent form.

This was demonstrated before the Society in the following manner:—

A mass of iron pyrites and another of galena were partially immersed in a vessel charged with sea water. To each of these minerals copper wires were attached at a distance of about 3 inches from the water-line, the intervening surface of the minerals being kept dry. That wire connected with the iron pyrites was placed in a solution of sulphate of copper contained in a separate vessel—the other wire was fastened to a thin piece of platinum foil—which foil was also set in the metallic solution facing the copper wire.

In a short time, the platinum foil was shown to be electrotyped with a regular film of copper wherever it had contact with the liquid, clearly showing that voltaic action had been developed by these sulphides, the galena forming the positive element of the pair.

A series of such pairs the author thought would properly be named a sulphide battery in accordance with the custom hitherto obtaining by which new forms of batteries are designated after some distinguishing or novel feature in them.

Gold was found to be reduced from its chloride by several of the metallic sulphides; both the electrodes in these cases were of platinum. Certain silver and platinum solutions were also effected in this manner.

Other sulphides manifest the same phenomena when paired among themselves.

The annexed column represents those which have been tested to this date, the arrangement of which is from positive to negative; the exciting solution is sea water.

Any two of these connected as described will be found positive and negative repectively; that occupying relatively the highest position in the column being always the positive one.

Protosulphide of iron.
Protosulphide of manganese (manganese blende).
Protosulphide of zinc (as zinc blende).
Bisulphide of tin.
Protosulphide of mercury.
Protosulphide of silver, chemically prepared.
Protosulphide of lead (as galena).
Subsulphide of copper (as copper glance).
Ferrosulphide of copper (as copper pyrites).
Bisulphide of iron (as cubical pyrites).
Sulphide of antimony (as stibnite).
Sulphide of gold.
Sulphide of platinum.
Sulpho-arsenide of iron (as mispickel).
Carbon (graphite) sulphurised.

In connection with this table it was found that in the same solution zinc was positive to protosulphide of iron, while silver places itself between the sulphides of silver and lead; platinum between sulphide of platinum and mispickel; and, finally, carbon places itself below sulphurised carbon, to which it is, therefore, negative.

* Abstract of a Paper read before the Wellington Philosophical Society, January 29th, 1871.

The author on commenting upon what this table discloses, alludes to the singular circumstance that galena should prove to be more easily affected by such saline solutions than copper pyrites, as under ordinary circumstances it appears unalterable, copper pyrites, on the other hand, very soon discolouring.

As to the intensity of the electric current which some of these sulphides are capable of generating, the author thinks it little, if anything, inferior to that produced by zinc in similar solutions, and while its development is slower, he assumes the absolute quantity generated by the complete oxidation of iron pyrites would be about four times that yielded by the oxidation of this metal.

These results lead the author to believe that much of our native silver and gold has been electro-deposited from saline solutions by voltaic action set up by the contact of dissimilar sulphides, or of sulphides with more negative substances, such as hæmatite, magnetite, or ferruginous rocks.

They further lead him to think that those traces of electric currents discovered about forty years ago by Mr. Fox to occur in mineral lodes generally are produced by the oxidation of these sulphides, and so do not appear to be either the results of magnetic induction or of changes or differences in the temperature of adjoining rock masses, to either of which these currents have hitherto been generally attributed.

The author concludes his paper by offering the suggestion that "possibly a table like the one here constructed, when amended and enlarged by the results of further and more extensive researches upon pure, mixed, and native sulphides in this direction, may be useful to chemical geology in its attempts to trace back the history of our metalliferous deposits, since it shows (by the use of a thoroughly reliable and simple process) the relative tendency of various sulphides to decompose in those kinds of solutions most likely to have contact with them in the situations where they naturally occur."

ON THE REVIVED THEORY OF PHLOGISTON.*

By WILLIAM ODLING, M.B., F.R.S.
Fullerian Professor of Chemistry, Royal Institution.

(Concluded from page 245.)

Very different is the view of phlogiston to be gathered from the writings of Dr. Watson, for example, who was appointed Professor of Chemistry at Cambridge in 1764, became Regius Professor of Divinity in 1771, and Bishop of Llandaff in 1782. This cultivated divine, indifferent, it is true, to the novel questions by which in less placid regions men's minds were so deeply stirred, amused the leisure of his dignified University life by writing scholarly accounts of the chemistry it had formerly been his province to teach; and in the first volume of his well-known "Chemical Essays," published in 1781, the following excellent account of phlogiston is to be found:—

"Notwithstanding all that perhaps can be said upon this subject, I am sensible the reader will still be ready to ask, *What is phlogiston?* You do not surely expect that chemistry should be able to present you with a handful of phlogiston, separated from an inflammable body; you may just as reasonably demand a handful of magnetism, gravity, or electricity to be extracted from a magnetic, weighty, or electric body. There are powers in nature which cannot otherwise become the objects of sense, than by the effects they produce; and of this kind is phlogiston. But the following experiments will tend to render this perplexed subject somewhat more clear.

"If you take a piece of *sulphur* and set it on fire, it will burn entirely away, without leaving any ashes or yielding any soot. During the burning of the sulphur, a copious vapour, powerfully affecting the organs of sight and smell, is dispersed. Means have been invented for collecting this vapour, and it is found to be a very strong acid. The acid thus procured from the burning of sulphur, is incapable of being either burned by itself, or of contributing towards the support of fire in other bodies; the sulphur from which it was procured was capable of both: there is a remarkable difference, then, between the acid procured from the sulphur and the sulphur itself. The acid cannot be the only constituent part of sulphur; it is evident that *something* else must have entered into its composition, by which it was rendered capable of combustion. This something is, from its most remarkable property, that of rendering a body combustible, properly enough denominated the food of fire, the *inflammable principle, the phlogiston*. . . . . This inflammable principle, or phlogiston, is not one thing in animals, another in vegetables, another in minerals; it is absolutely the same in them all. . . . . This identity of phlogiston may be proved from a variety of decisive experiments; I will select a few, which may at the same time confirm what has been advanced concerning the constituent parts of sulphur.

"From the analysis or decomposition of sulphur effected by burning, we have concluded that the constituent parts of sulphur are two—an *acid* which may be collected, and an *inflammable principle* which is dispersed. If the reader has yet acquired any real taste for chemical truths, he will wish to see this analysis confirmed by synthesis; that is, in common language, he will wish to see sulphur actually made by combining its acid with an inflammable principle. It seldom happens that chemists can reproduce the original bodies, though they combine together all the principles into which they have analysed them. . . . In the instance, however, before us, the reproduction of the original substance will be found complete.

"As the inflammable principle cannot be obtained in a palpable form separate from all other bodies, the only method by which we can attempt to unite it with the acid of sulphur must be by presenting to that acid some substance in which it is contained. Charcoal is such a substance; and by distilling powdered charcoal and the acid of sulphur together, we can produce a true yellow sulphur, in nowise to be distinguished from common sulphur. This sulphur is formed from the union of the acid with the phlogiston of the charcoal; and the charcoal may by this means be so entirely robbed of its phlogiston, that it will be reduced to ashes, as if it had been burned. . . . .

"I will in this place, by way of further illustration of the term phlogiston, add a word or two concerning the necessity of its union with a metallic earth, in order to constitute a metal. Lead, it has been observed, when melted in a strong fire, burns away like rotten wood; all its properties as a metal are destroyed, and it is reduced to ashes. If you expose the ashes of lead to a strong fire, they will melt; but the melted substance will not be a *metal*, it will be a yellow or orange-coloured *glass*. If you pound the glass, and mix it with charcoal dust, or if you mix the ashes of the lead with charcoal dust, and expose either mixture to a melting heat, you will obtain, not a *glass*, but a metal, in weight, colour, consistency, and every other property the same as lead. The ashes of lead melted *without* charcoal become *glass*; the ashes of lead melted *with* charcoal become a *metal*. The charcoal, then, must have communicated *something* to the ashes of lead, by which they are changed from a glass to a metal. Charcoal consists of but two things—of ashes and of phlogiston; the ashes of charcoal, though united with the ashes of lead, would only produce glass; it must, therefore, be the other constituent part of charcoal, or phlogiston, which is communicated to the ashes of lead, and by an union with which the ashes are restored to their metallic form. The ashes of lead can never be restored to their metallic form without their being united with

* A Lecture delivered at the Royal Institution of Great Britain, April 28th, 1871.

some matter containing phlogiston, and they may be reduced to their metallic form by being united with *any* substance containing phlogiston in a proper state, whether that substance be derived from the animal, vegetable, or mineral kingdom; and thence we conclude not only that phlogiston is a necessary part of a metal, but that phlogiston has an identity belonging to it, from whatever substance in nature it be extracted. And this assertion still becomes more general, if we may believe that metallic ashes have been reduced to their metallic form, both by the solar rays and the electrical fire."

The foregoing account by Dr. Watson is almost a translation from Stahl's "Zymotechnica Fundamentalis, simulque experimentum novum sulphur verum arte producendi," in which he establishes what may be called the permanency of chemical substance—that metallic lead is reproducible from the ashes of lead, *sulphur verum* from the acid of sulphur. And, whether or not taking note of the oxidations and deoxidations effected, how little differently, even at the present day, would the actions referred to be described and explained. Is it not our habit to say that charcoal and sulphur and lead are bodies possessing potential chemical energy—that is, phlogiston; that in the act of burning, their energy which was potential becomes kinetic or dynamical, and is dissipated in the form of light and heat; that the products of their burning (including the gaseous product now known to be furnished by the burning of charcoal) are substances devoid of chemical energy, that is of phlogiston; that when the acid substance furnished by burning sulphur is heated with charcoal, some energy of the unburnt charcoal is transferred to the burnt sulphur, just as some energy of a raised weight may be transferred to a fallen one, whereby the burnt sulphur is unburnt, provided with energy, and enabled to burn again, and the fallen weight is lifted up, provided with energy, and enabled to fall again; that the potential chemical energy of metallic lead did not originate in the lead, but is energy or phlogiston transferred thereto from the charcoal by which it was smelted; and lastly, that the chemical energy of the charcoal itself, its capability of burning, its power of doing work, in one word its phlogiston, is merely a portion of energy appropriated directly from the solar rays.

If this be a correct interpretation of the phlogistic doctrine, it is evident that the Stahlians, though ignorant of much that has since become known, were nevertheless cognisant of much that became afterwards forgotten. For most of what has since become known, mankind are indebted to the snrpassing genius of Lavoisier; but the truth which he established, alike with that which he subverted, is now recognisable as a partial truth only; and the merit of his generalisation is now perceived to consist in its addition to—its demerit to consist in its supersession of—the not less grand generalisation established by his scarcely remembered predecessors. This being so, the relationship to one another of the Stahlian and Lavoisierian theories of combustion furnishes an apt illustration of the general truth set forth by a great modern writer, that "in the human mind, one-sidedness has always been the rule, and many-sidedness the exception. Hence, even in revolutions of opinion, one part of the truth usually sets while another rises. Even progress, which ought to superadd, for the most part only substitutes one partial and incomplete truth for another; improvement consisting chiefly in this, that the new fragment of truth is more wanted, more adapted to the needs of the time, than that which it displaces."

The partial truth contributed by Lavoisier was indeed more wanted, more adapted to the needs of the time, than the partial truth which it displaced. To him chemists are indebted for their present conception of material *elements*; and especially for their knowledge of the part played by the air in the phenomena of combustion, whereby oxygenated *compounds* are produced. The phlogistians, indeed, were not unaware of the necessity of air to combustion, but being ignorant of the nature of air were necessarily ignorant of the function which it fulfilled. To burn and to thrown off phlogiston being with them synonymous expressions, the air was conceived to act by somehow or other enabling the combustible to throw its phlogiston off; and a current of air was conceived to promote combustion by enabling the combustible to throw its phlogiston off more easily. Moreover, contact of air was not essential to combustion, provided there was present instead some substance, such as nitre, which equally with, or even more effectively than air, could enable the combustible to discharge itself of phlogiston. But while the phlogistians, on the one hand, were unaware that the burnt product differed from the original combustible otherwise than as ice differs from water, by loss of energy, Lavoisier, on the other hand, disregarded the notion of energy, and showed that the burnt product included not only the stuff of the combustible, but also the stuff of the oxygen it had absorbed in the burning. But, as well observed by Dr. Crum-Brown, we now know "that no compound contains the substances from which it was produced, but that it contains them *minus* something. We now know what this something is, and can give it the more appropriate name of potential energy; but there can be no doubt that this is what the chemists of the seventeenth century meant when they spoke of phlogiston."

Accordingly, the phlogistic and antiphlogistic views are in reality complementary and not, as suggested by their names and usually maintained, antagonistic to one another. It has been said, for example, that according to Stahl, the product of combustion is simple, and the combustible a compound of the product with imaginary phlogiston—which is false; whereas, according to Lavoisier, the combustible is simple, and the product a compound of the combustible with actual oxygen—which is true. But in this case, as in so many others, everything turns upon the use of the same word in a different sense at different periods of time. When Lavoisier spoke of red lead as being metallic lead combined with oxygen, he meant that the matter or stuff of the red lead consisted of the matter or stuff of lead *plus* the matter or stuff of oxygen. But when the Stahlians spoke of metallic lead being burnt lead combined with phlogiston, they had the same sort of idea of combination in this instance, as others have expressed by saying that the weight of a body is compounded of its matter and its gravity; or that steam is a compound of water and heat; or, to use a yet more Lavoisierian expression, that oxygen gas itself is a compound of the basis of oxygen with caloric. It is not, then, that the one statement, Stahlian or Lavoisierian, is false and the other true, but that both of them are distorted, because incomplete. Chemists nowadays are both Stahlian and Lavoisierian in their notions; or have regard both to energy and matter. But Lavoisierian ideas still interfere very little with our use of the Stahlian language. While we acknowledge that in the act of burning the combustible and the oxygen take equal part, just as in the act of falling the weight and the earth take equal part, yet in our common language we alike disregard the abundant atmosphere and abundant earth as being necessarily understood, and speak only of the energy of the combustible and of the weight, which burn and fall respectively. Whatever may be the fault of language, however, chemists do not omit to superpose the Lavoisierian on the Stahlian notion. They recognise fully that it is by the union of the combustible with oxygen that phlogiston is dissipated in the form of heat; and further, that phlogiston can only be restored to the burnt combustible on condition of separating the combustible from the oxygen with which it has united; just as energy of position can only be restored to a fallen weight on condition of separating it to a distance from the surface on which it has fallen.

That Stahl and his followers regarded phlogiston as a material substance, if they did so regard it, should interfere no more with our recognition of the merit due to their doctrine than the circumstance of Black and Lavoisier regarding caloric as a material substance, if they did so

regard it, should interfere with our recognition of the merit due to the doctrine of latent heat. But though defining phlogiston as the principle or matter of fire, it is not at all clear that the phlogistians considered this matter of fire as constituting a real body or ponderable substance; but rather that they thought and spoke of it as many philosophers nowadays think and speak of the electric fluid and luminiferous ether. The nondescript character, properly ascribable to phlogiston, is indicated by the following quotation taken from Macquer's "Elemens de Chymie Theorique," 1749. It must not, of course, be forgotten that the popular impression as to phlogiston having been conceived by its advocates as a material substance having a negative weight or levity, is erroneous; and is based on an innovation that was introduced during the struggling decadence of the phlogistic theory, and advocated more particularly by Lavoisier's subsequent colleague, Guyton de Morveau, in his "Dissertation sur le Phlogistique, considéré comme Corps grave, et par rapport aux changemens de pesanteur qu'il produit dans les corps auxquels il est uni," 1762. Macquer writes as follows:—

"La matière du soleil, ou de la lumière, le phlogistique, le feu, le soufre principe, la matiére inflammable, sont tous les noms par lesquels on a coutume de désigner l'élément du Feu. Mais il paroît qu'on n'a pas fait une distinction asséz exacte . . . . du nom qu'il mérite véritablement lorsqu'il entre effectivement comme principe dans la composition d'un corps, ou bien lorsqu'il est seul et dans son état naturel. Si on l'envisage sous cette dernière vue, le nom de Feu, de matière du soleil, de la lumière et de la chaleur, lui convient particulièrement. Pour lors, c'est une substance que l'on peut considérer comme composée de particules infiniment petites, qui sont agitées par un mouvement très rapide et continuel, par conséquent essentiellement fluide. Cette substance, dont le soleil est comme le réservoir général, s'en émane perpetuellement, et est répandue universellement dans tous les corps que nous connoissons; mais non pas comme principe ou essentielle à leur mixtion, puisqu'on peut les en priver, du moins en grande partie, sans qu'ils souffrent pour cele la moindre décomposition. . . . . Cependant les phénomènes que présentent les matières inflammables lorsqu'elles brulent, nous indiquent qu'elles contiennent réellement la matière du Feu comme un de leurs principes. . . . . Examinons donc les propriétés de ce feu fixé, et devenu principe des corps. C'est lui auquel nous donnerons particulièrement le nom de matière inflammable, due soufre principe, ou de phlogistique, pour le distinguer du Feu pur."

Again, much the same thing is to be found in Baumé's "Manuel de Chymie," 1765; as for example:—

"Nous considérons le feu sous deux états différens. Lorsqu'il est pur, isolé, et qu'il ne fait partie d'aucun composé . . . . Lorsqu'il est combiné avec d'autres substances, et qu'il fait un des principes constituans des corps composés . . . . On n'est pas certain si le feu est pesant. Il y a des expériences pour et contre. . . .

"Pendant la combustion des substances, le feu combiné se reduit en feu élémentaire, et se dissipe à mesure. Le célébre Boerhaave n'est cependant pas de ce sentiment; il dit que si cela étoit, la quantitè de feu élémentaire devroit augmenter à l'infini dans la nature. . . . . Mais il est facile de répondre à cette objection, en disant comme on est on droit de la présumer, que le feu élémentaire, dégagé des corps, se combine à mesure avec d'autres substances, et qu'il perd toutes ses propriétés de feu libre, en devenant principe constituent des corps, dans la composition desquels il entre. . . . . Le principe dont nous entendons parler ici, est celui que Stahl a nommé *phlogistique*."

In interpreting the above and other phlogistic writings by the light of modern doctrine, it is not meant to attribute to their several authors the precise notion of energy that now prevails. It is contended only that the phlogistians had, in their time, possession of a real truth in nature which, altogether lost sight of in the intermediate period, has since crystallised out in a definite form. "I trust," said Beccher, "that I have got hold of my pitcher by the right handle." And what he and his followers got hold of and retained so tenaciously, though it may be shiftingly and ignorantly, we now hold to knowingly, definitely, and quantitatively, as part and parcel of the grandest generalisation in science that has ever yet been established.

ON THE

EFFECT OF CERTAIN METALLIC SUBSTANCES BESIDES IRON ON CARBONIC OXIDE, INCLUDING SILICON.*

By I. LOWTHIAN BELL.

A DENSE fume is emitted from furnaces smelting the Cleveland ironstone, and this, condensing, lodges in large quantities in the pipes for conducting the gases to the boilers, &c. In the so-called flue-dust zinc is always found, and in most ores there is more or less manganese, occasionally copper, often titanium, and some other metals.

Under the circumstances, it was natural to try the power of metals thus often found associated with iron in its ores, in splitting up CO, to which was added experiments upon others not generally found in connection with that one which it is our more immediate province to examine.

In the following experiments every care was taken to have the CO free from cyanogen, otherwise a carbonaceous deposit, probably paracyanogen, takes place, and to have the metals and their oxides without a trace of iron. In one case, that of copper, in the first trials a minute quantity of carbon was found deposited, which subsequent investigation proved to be caused by a trace of iron being present.

Manganese.—

Dioxide (MnO_2) was obtained by boiling an alkaline permanganate with nitric acid, and drying the precipitate after it was thoroughly washed.

Manganoso-manganic oxide (Mn_3O_4) was got by heating pure $MnCO_3$ in a current of air until it contained no trace of CO_2. The $MnCO_3$ was procured free from iron by adding a small quantity of Na_2CO_3 to a solution of manganese, and boiling the resulting turbid liquor until every trace of iron was precipitated by the $MnCO_3$ thrown down at first. The filtered liquid thus freed from Fe was then precipitated by further addition of Na_2CO_3, and the precipitate thoroughly washed and dried.

These two oxides were exposed to a stream of pure CO with the results given below:—

	Expt. 376.	*Expt.* 377.	*Expt.* 378.
	MnO_2.	MnO_2.	Mn_3O_4.
Time of exposure ..	4½ hours	6½ hours	10 hours
Temperature—	Below red heat,	Zinc softened,	Zn fused, Sb not,
Loss of weight	—	19·6 per ct.	7·1 per ct.
Theoretical loss for reduction to MnO	—	18·4 ,,	7·0 ,,
Increase of weight on roasting in oxygen	7·53 per ct.	—	—
Theoretical increase of weight for Mn_3O_4	7·51 ,,	—	—
Carbon deposited ..	*nil*	*nil*	*nil*

From these trials it seems that MnO_2, at a heat short of redness, is converted by CO into MnO in 4½ hours, and that both MnO_2 and Mn_3O_4 lose oxygen readily at the temperature of melting zinc, both being reduced to MnO.

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* From the "Chemical Phenomena of Iron Smelting."

In no case was there a trace of carbon present. It would therefore appear that oxides of Mn are incapable of acting on CO, as Fe does, by separating this gas into C and CO_2, and that no suboxide Mn_2O is formed.

Copper.—

Pure copper oxide was got by electrolysis of re-crystallised sulphate, solution in nitric acid, evaporation, and ignition.

Expt. 379.—The oxide of copper was exposed at a temperature of melting zinc to CO for 7 hours, after which its composition was—

Copper	98·1
Oxygen	1·9
C	*nil*
	100

So that reduction was nearly complete in this time.

Exbt. 380.—At a low red heat, oxalic acid gas (equal vols. CO and CO_2) withdrew all the oxygen from oxide of copper. The loss of weight was 20·3 per cent—20·2 per cent being the theoretical equivalent of O. No carbon was precipitated.

Expt. 381.—Pure spongy Cu, obtained by reducing the above oxide in H, was kept in contact with CO for 10 hours, at a temperature of melting zinc. No change of weight, hence no C deposited.

Expt. 382.—Pure spongy Cu, exposed for 2 hours to oxalic acid gas, at a dull red heat, did not alter in weight.

Zinc.—

ZnO was obtained by treating pure $ZnCl_2$ with Na_2CO_3, and igniting the $ZnCO_3$ obtained—

Expt. 383.—Exposed at temperature of melting zinc to pure CO for 6½ hours. No carbon deposited, nor was there any sensible amount of reduction, the weight of the product not being altered by moistening with NO_3H, and gentle ignition.

Tin.—

Stannic oxide obtained by oxidation of block tin by nitric acid.

Expt. 384.—Action same as with ZnO in CO, at temperature of melting zinc. No carbon, nor any appreciable amount of reduction, the weight not altering sensibly by treatment with NO_3H.

Chromium.—

Expt. 385.—Anhydrous chromic oxide obtained by igniting potassium anhydro-chromate (bichromate of potash), and washing the product. Had become darker, but contained no carbon, and was in other respects unchanged by exposure to CO for 7½ hours, in lead bath, to about 410° C. (770° F.).

Expt. 386.—Hydrated chromic oxide, got by precipitation of chloride by means of ammonia, was exposed at the same time as the chromic oxide in previous experiment. Original colour bluish-green, changed to bright green. In other respects no change, save that almost the whole of the water of hydration was expelled.

Lead.—

Lead oxide got by ignition of crystallised nitrate :—

Expt. 387.—PbO exposed to CO, for 6½ hours, at a temperature of melting zinc. Perfect reduction ensued.

Expt. 388.—PbO, same treatment and same results, at red heat, in 6½ hours. No trace of carbon in either of these experiments.

Nickel.

Re-crystallised nickel nitrate (commercially pure) was boiled with nitric acid, and an excess of ammonia added. This dissolved the precipitated oxide of nickel, but left a trace of Fe_2O_3, which was separated in the usual way. The alkaline solution of the oxide of nickel was evaporated to dryness, and ignited in two portions. Either from the presence of a little adhering nitrate of ammonia, or from differences in the temperature at which ignition was effected, two different oxides were obtained—the one, compact, and nearly corresponding to NiO (Ni 78·7 O 21·3); and the other to Ni_2O_3 (Ni 71·1 O 28·9).

The two oxides of nickel thus obtained had the following composition, arrived at by reducing known weights of each in pure hydrogen.

	Compact Oxide.	Black Oxide.
Ni	77·5	71·2
O	22·5	28·8
	100	100

These two substances were exposed to a current of pure CO in the lead bath arrangement :—

Expt. 389.—Compact oxide. *Expt.* 390.—Black oxide.

	Time of exposure, 7½ hours. Temperature—Zinc melted, Antimony not affected.	Time of exposure, 10 hours. Temperature—Zinc melted, Sb not.
Composition of product. Ni ..	87·2	28·1
O ..	12·2	—
C ..	0·6	71·9
	100·0	100·0

In experiment 389 the residual oxygen is just 14·0 per cent of the weight of the total nickel present: this represents an oxide, having almost exactly the composition of Ni_2O (which requires 13·6 to 100 Ni). No nickel, in this case, was dissolved out by iodine; and as iodine was found to attack spongy nickel reduced by hydrogen, with ease, there could have been no metallic nickel present.

It was shown formerly (Experiments 62 to 69), that in several instances where Fe_2O_3 had been imperfectly reduced by CO, the proportion between the iron insoluble in iodine and the residual oxygen was almost exactly that required to form a suboxide, Fe_2O: hence it would appear that nickel and iron are analogous in this respect also, viz., that the suboxides of both are not acted on by iodine and cold water. As will be presently shown, Co_2O, if formed, is acted on thus, the product being, however, not the sesquioxide (as in the case of iron when the hydrated protoxide is thus oxidised), but the protoxide.

In the case of the Ni_2O_3 the carbon and oxygen determinations were faulty, and the sum of both was ascertained by finding the quantity of nickel present in the mass after exposure to CO. The result is such that there must have been a carbon deposition equal to about 250 per cent of the nickel actually present.

In a stream of oxalic acid gas (equal volumes of CO and CO_2) the compact oxide gave the following results :—

		Per cent.
Expt. 391.—O lost in	2½ hours .. at a low red heat,	20·4
Do.	3 ,, more do.	1·1
Do.	3 ,, ,, do.	0·2
	8½	21·7
Expt. 392.—O lost in	3 hours .. at a bright red heat,	22·0
Do.	2 ,, more do.	0·1
	5	22·1

The original oxygen present being 22·5 per cent, it appears that a mixture of equivalents of CO and CO_2 has the power, at both these temperatures, almost completely to deoxidise NiO.

Trials were then made to discover the action of the pure spongy metal on pure CO. A portion of the compact oxide was reduced in hydrogen until no more H_2O was evolved. A current of CO was then passed over the metallic Ni thus obtained :—

Expt. 393.—Time of exposure, 5 hours. *Expt.* 394.—5 hours.

	Temperature—Melting zinc.	Bright red heat
Composition of product. Ni ..	99·93	100·0
C ..	0·04	—
O ..	0·03	—
	100·0	100·0

The dissociation, therefore, of the CO at a temperature of melting zinc was very feeble, and at a bright red heat it was *nil*.

The power of the spongy metal to decompose CO_2 was next made the subject of investigation.

Expt. 395.—At melting zinc no appreciable amount of CO was obtained after 30 minutes' exposure, the metal gaining in weight 0·3 per cent, probably due to traces of moisture and air present.

Expt. 396.—At a low red heat the metal gained 3 per cent of its weight in 90 minutes.

Expt. 397.—At a bright red heat 40 c.c. of gas were obtained after 40 minutes' exposure, which was unabsorbed by potash. It was found to contain 96 per cent of CO.

Expt. 398.—Spongy nickel, at a moderately bright red heat, was unaffected after an exposure of 15 hours to oxalic acid gas (equal vols. of CO and CO_2).

The nickel oxide employed in these experiments might contain some cobalt, but, if so, it was the minutest trace; not the faintest indication of its presence being shown by the very delicate test afforded by the borax bead before the blowpipe.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL IRISH ACADEMY.

May 29th, 1871.

At the general meeting of the Academy, the President, the Rev. J. Jellett, read a paper upon "*The Cultivation of Beet-Root Sugar in Ireland.*"

Six specimens of the white beet, grown at the Model Farm at Glasnevin, had been examined with the Jellett saccharometer. The sliced root was digested with weak alcohol three or four times; the solutions were evaporated and then made up, after having been heated to 180°, to a given bulk with water. This solution was filtered through charcoal, and the first portions rejected as a certain amount of the sugar is absorbed. The sugar present in the beet almost entirely consisted of cane sugar. The following are the results of the six determinations:—

1st.	12·05	per	cent
2nd.	9·50	,,	,,
3rd.	12·58	,,	,,
4th.	12·59	,,	,,
5th.	11·62	,,	,,
6th.	12·43	,,	,,

Professor Jellett said that anything over 10 per cent might be considered very good and would pay to work.

Dr. Sullivan remarked (in a discussion that followed) that he was very glad to find his statements corroborated, viz., that beet-root could be easily grown in Ireland containing 10 to 12 per cent of sugar. No 2, which contained only about 9 per cent of sugar, had not been earthed up, and this fact is corroborative of a statement that a powerful sun was not necessary to produce saccharine matter in the root. All the other specimens had been earthed up.

Dr. W. Sullivan then read a paper "*On the Comparative Chemical Composition of Ancient Bronzes*," which was in connection with the ethnology, metallurgy, and commerce of the ancient people of Europe.

CORRESPONDENCE.

TECHNICAL EDUCATION FOR THE SONS OF FARMERS.

To the Editor of the Chemical News.

Sir,—By the enclosed extract you will see that I have been anxious to set on foot a scheme cognate to that of Mr. Little, whose plan I was most glad to read in your last number. I should be glad to work with him, and propose a central committee. By inserting my enclosed letter you will oblige me, and, I think, forward the cause. —I am, &c.,

Marshall Hall.

New University Club, May 27, 1871.

"*To the Editor of the 'Devizes Gazette.'*

"Sir,—In these days of Educational talk and of School Boards, there is no need of apology for addressing you upon a branch of the subject which receives too little attention, more especially in agricultural districts. Education must begin from above, and work downward. From the squire to the farmer, from the farmer to the labourer; and, again, from the manufacturer to the artizan must be its course. I certainly think that at this present day we are all awakening to the necessity of educating every man according to his trade, and also to the fact that every man has his trade; that the squire's duties should lead him to the study of political economy, of the law, of the natural sciences, and especially those connected with agricultural operations and accounts. The farmer, when without such things as agricultural chemistry and the physiology of plants, is in mediæval darkness; and the labourer, without the discipline of reading, writing, and arithmetic, is without the proper chances either of justly estimating his present position or of availing himself of the chances which may occur of bettering it. As I have said, the upper classes are quickly awakening to the requirements of a rational training for their sons. The next class also; witness the existence of the Royal Agricultural College: but it is for them to pass on the movement, as it were, to those beneath them. But an especially large class, that of farmers of medium and small holdings, can scarcely afford the means to send their lads to College, nor indeed can they always spare the time and labour which would be taken from home. It follows, therefore, that, if possible, a plan should be proposed for bringing at all events an elementary means of instruction in the higher branches of their own occupation within their reach, at such a cost of time and money as need deter none from taking advantage of it. And, premising that there shall exist a good foundation in reading, writing, and arithmetic, I think I can point out how to obtain this object. At the local centres which form the chief markets of the county, such as Salisbury, Devizes, Chippenham, Trowbridge, or such of these and other places as may be shown to be advisable, not merely from their population but from the number of farms around them, let there be established classes to be held on winter afternoons and evenings, first of every thing in bookkeeping, and then in botany, agricultural chemistry, and geology, and the first elements of surveying. There can be no reason why a class should not be held for mechanics, if sufficient in number. Such a work as Goodeve's "Elements of Mechanism" would be inestimable to the humblest artizan, and yet within his mastery. The farmer can take up no better book than "How Crops Grow," and "Church's Laboratory Guide." But none of these will be much good without the assistance of a master. And the way in which these classes might be held would be to pay, say two travelling professors, whose duties should be confined to the county, who should hold classes two evenings a week each at three places. This could be done by a very moderate subscription. It is by no means my idea to afford such classes as a species of charity. I think those who use them would ultimately be sufficient in number to pay for them. But not so at starting, and perhaps a subscription in aid may at all times be useful for proper development of my scheme. There can be no doubt that landlords subscribing will get their money back again through the class of tenants produced in a few years. Finally, these classes would always be an invaluable preparation for the College at Cirencester, and for others of the higher educational institutions. I need not

say that I hope to subscribe myself, and if ever a laboratory is established, say in Devizes, I shall hope to assist in the plain fitting out that will be requisite. I will also contribute to the educational library which will be necessary. At the approaching Quarter Sessions let me ask my brother landlords and my friends the farmers to talk these matters over. I beg those who kindly wish to assist to communicate with me, especially as to forming a private committee for consideration.—MARSHALL HALL, Brighton, Dec. 27, 1870."

MISCELLANEOUS.

Cambridge Local Examinations.—At the meeting presided over the other day by Lord Houghton, at the London University, it was shown that the percentage of those who passed this year, both of boys and girls, had decreased in every department. Of thirty-two juniors from the City of London, four took honours, thirteen passed, and fifteen failed. In that most popular of scientific subjects among the seniors, chemistry, two only passed, out of seventeen presented, one from the Proprietary School, Gravesend, taught by the Professor of Chemistry at the Birkbeck Institution, and one from Denbigh. The examiners declared that "many had come up to be examined on subjects of which they literally knew nothing!" —*City Press.*

The North Austrian Industrial Association.—We have been favoured with a paper from this Association, of which the following is a condensed summary:—It having been found that exhibitors at all the exhibitions hitherto held, and also many of the juries, expressed a well-founded dissatisfaction with the system of giving prizes, the industrial association above-mentioned has adopted the proposal of one of its members, M. F. Ritter von Wertheim, to request all who feel interested in this question to try to reply to the following query, which is put as a prize essay:—"In what manner are to be avoided, or how and by what simple and efficient means are to be improved, the methods of granting and distributing prizes hitherto pursued at exhibitions, at the Great Exhibition which is to take place at Vienna in the year 1873." For the three best essays on this subject, the North Austrian Industrial Association intends to give one of its great gold and two great silver medals, presented for this purpose by M. F. Ritter von Wertheim, upon the following conditions:—The writer or author is not to content himself with simple suggestions, but he should, even though this be done briefly, criticise and expose the advantage as well as the defects of the methods of granting and giving prizes as hitherto pursued at public, general, and other industrial exhibitions, and next propose his plan of an improved method. The essays must be sent in not later than the end of October next, and are to be provided with a motto, and with a sealed note bearing on the outside cover the same motto, and inside the author's name. The manuscripts of the essay remain the property of those to whom the medals shall be given, but the North Austrian Industrial Association (Der N. O. Gewerbe Verein), reserves to itself the right of printing and publishing the essays in its own Transactions. The manuscripts (to be sent carriage or postage paid, to the address of Herr C. V. Scherzer, President of the Association at Vienna) will not be returned, but the essays may be taken back by the authors from the office of the Association. The jury to consider the essays will be appointed by the Association; the medals will be awarded at the General Annual Meeting of the Association in December next. The names of the successful authors of the essays will be made public in all German and, as far as possible, all other European and American newspapers.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the CHEMICAL NEWS, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

NOTE. All degrees of temperature are Centigrade, unless otherwise expressed.

Polytechnisches Journal von Dingler, second number for April, 1871.

This number contains the following original papers and memoirs bearing upon chemistry and collateral sciences:—

Improved Copper-Zinc Battery.—L. Kohlfürst.—The arrangement alluded to in this paper cannot be described without reference to the woodcuts added to it.

Reduction of Chloride of Silver in the Moist Way.—Dr. Grüger.—This paper contains the detailed description of the reduction of chloride of silver dissolved in ammonia by means of metallic zinc, which process, according to the author, succeeds very well, and yields a silver of greater purity than is obtained by the process for reduction of silver by the moist way now in use, provided the operation be carried on in closed wide-mouthed stoppered bottles. The silver, after complete reduction (and this has to be tested for by means of a drop of the ammoniacal fluid being put into some hydrochloric acid), is first thoroughly washed with concentrated hydrochloric acid, next with water, and, lastly, for some moments with dilute ammonia, and then again with distilled water, and provided the zinc be used in large lumps thick enough to admit of being readily removed from the spongy silver. It is clear that this method of reduction involves the use of a considerable quantity of ammonia; but this, the author states, can in great measure be recovered by distillation.

Regeneration of Waste Silver Solutions used in Photography.—Dr. Grüger.—After first referring to the generally applied and well-known means now in use for this purpose, the author states that the best plan to treat these solutions is the following:—The solutions are boiled either in a porcelain basin or glass flask, and while boiling there is added to them recently precipitated, well washed, and moist oxide of silver, the boiling being continued for some time. The liquid is next filtered, and then evaporated to dryness, the heat being increased to fusion, so as to destroy ammoniacal salts; the residue is pure nitrate of silver. The sediment on the filter contains some oxide of silver, which must be added in excess; and, therefore, in order not to lose that, the filter is preserved, and the contents worked up at a subsequent operation. The nitrate of silver thus obtained is, by practical photographers, pronounced to be of excellent quality.

Theoretical Estimation of the Value of Various Fire-Clays.—Dr. C. Bischof.—The first portion of a lengthy monograph on this subject, which, however valuable the contents, does not admit of any useful abstraction.

Quantitative Estimation of Nitric Acid.—Dr. A. Wagner.—This method is based upon the fact that when saltpetre, or any other nitrate, is ignited, access of air being excluded, with an excess of oxide of chromium and carbonate of soda, the nitric acid oxidises the chromic oxide according to the formula $Cr_2O_3+NO_5=2CrO_3+NO_2$. 76·4 parts, by weight, of oxide of chromium are oxidised to chromic acid by 54 parts of nitric acid, or of 1 of chromic oxide by 0·7068 of nitric acid. The operation is performed by taking from 0·3 to 0·4 grm. of the nitrate, mixing it intimately with 3 grms. of chromic oxide and 1 grm. of carbonate of soda, introducing this mixture into a hard German glass combustion-tube, one end of which is drawn out, and a vulcanised india-rubber tube attached to it, which is made to dip for about a quarter of an inch into water, while to the other open end, by means of a cork and glass tube bent at right angles, an apparatus is fitted for the evolution of carbonic acid gas, which is made to pass through the tube before igniting it, and kept passing through all the time until the tube is quite cool again after ignition. The contents of the tube are placed in warm water, and after filtration the chromic acid is estimated by Rose's method. This process of estimating nitric acid, having been tested by the author with various nitrates, has been found to yield very accurate results.

Industrial Manufacture of Treacle and Dry Sugar from Starch.—C. Krötke.—A lengthy practical treatise on this subject.

Annalen der Chemie und Pharmacie (Supplementband viii.), No. 1, 1871.

This number contains the following original papers and memoirs:—

Occurrence of the Ammoniacal-Magnesian Sulphate in the Lagoons of Tuscany.—Dr. O. Popp.—This essay contains the account of a series of investigations made by the author on the constituents of the boric acid-producing lagoons of Tuscany. The salt alluded to above does not occur to the same quantity in the water of all these lagoons, since that of those producing boric acid in the purest and largest quantity is either not at all, or only slightly, impregnated with the ammoniacal-magnesian sulphate, which is found in a pure state

in the lagoons of Sasso and Aqua Viva. The re-crystallised salt was found to consist, in 100 parts, of—NH_3, 9·355; MgO, 11·043; $2SO_2$, 44·410; 7HO, 35·127;—total, 99·935. Formula—

$$NH_4O,SO_3+MgO,SO_3+6HO.$$

Formation of Boracic Acid in the Fumaroles of Tuscany.—Dr. O. Popp.—This memoir records, in the first place, the various theories and hypotheses which have been hitherto published for the purpose of explaining the presence, origin, and formation of boracic acid in the active volcanic formation of Tuscany; while, secondly, the author brings forward some experiments tending to explain the formation of boracic acid by the decomposition of boracic minerals under the influence of various reactions which may be supposed to take place aided by the high temperature of the locality, as called forth by volcanic action.

Cumaric Acid, Hydrocumarine, and Hydrocumaric Acid.—M. C. Zwenger.—After describing at length the preparation of cumaric acid from cumarine by the action of a very concentrated solution of caustic potassa, the author states that the purified acid is difficultly soluble in cold, somewhat more readily in boiling, water, from which it is deposited in anhydrous needle-shaped crystals. The acid is more readily soluble in alcohol and ether; the solutions exhibit a strongly acid reaction to test-paper, and decompose alkaline and other carbonates. Cumaric acid fuses at 195°; by dry distillation it yields chiefly phenol; on being fused with caustic potassa, hydrogen is given off and salicylate and acetate of potassa formed. The formula of this acid is $C_9H_8O_3$. Next follows a detailed description of the salts of this acid, and a discussion on the cumarine contained in the *Trifolium repens*, and in the so-called *fahanitea*; it appears that in these vegetables cumaric acid is also present. Hydrocumaric acid is obtained, along with other substances, by treating an aqueous solution of excess of cumarine with sodium amalgam. The new acid is a crystalline body, scarcely soluble in water, but more readily so in alcohol and ether; formula, $C_{18}H_{18}O_6$. By the elimination of two molecules of water, hydrocumaric acid is converted into hydrocumarine, $C_{18}H_{14}O_4$, a solid crystalline body, almost insoluble in water, alcohol, and ether, and fusing at 222°.

Molecular Weight of some Protoxides.—Dr. A. Ladenburg.—This monograph contains the following sections:—General review; protoxides of iron, manganese, and chromium; tin compounds.

Schiel's Chloral-Uric Acid.—Dr. N. Lubavin.—The main result of this paper is to prove that the acid alluded to, described by M. Schiel (*Ann. Chem. Pharm.*, vol. cxii., p. 78, 1859), and obtained by that author by the action of an aqueous solution of chlorous acid upon uric acid, is not a chemical compound at all, but a mixture of parabanic acid with chloride of ammonium; while the author of this paper also found that, by the action of chlorous acid upon uric acid, intermediate, but unstable, oxidation compounds were formed.

Derivatives from Anethol.—Dr. A. Ladenburg.—After referring at length to his former researches, and also those of other chemists on this subject, the author describes the mode of preparation of anol from anethol by the action of fused caustic potassa. The new compound (anol) $C_9H_{10}O$, is soluble in alcohol, ether, chloroform, and boiling water; the substance is very prone to decomposition, and, owing to the small quantity obtained, no derivatives of this body were prepared. The lengthy essay further treats on the action of PCl_5 and bromine upon anethol.

Researches on Vanadium.—Dr. H. E. Roscoe.—The third part of this lengthy monograph, divided into the following sections:—Bromides of vanadium; vanadium and iodine; salts of vanadic acid; artificially-made vanadinite.

Second Axiom of the Mechanical Theory of Heat, and its Application to some Phenomena of Decomposition.—Dr. A. Horstmann.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 7, 1871.

This number contains the following original papers and memoirs:—

Bromal and the By-Products obtained in its Preparation.—Dr. L. Schäffer.—The author, while preparing hydrate of bromal on the large scale by causing the vapours of bromine to pass into alcohol, care being taken to avoid an excess of the haloid, and thereby preventing the formation of bromated bromethyls, studied the substances resulting from this reaction by first submitting the raw product to distillation in a steam-bath, and next employing a sand-bath. By the steam-bath heat some bromide of ethyl, acetic ether, and solution of hydrobromic acid were driven off; while, by further heating, three different groups of bodies were obtained, viz.:—At from 100° to 130°, chiefly hydrated bromhydric acid; at from 165° to 180°, bromal and an oily substance insoluble in water; above 180°, some substances boiling with partial decomposition. From the distillate which came over at between 165° and 180°, bromal was separated by the admixture of water, and the hydrate of bromal obtained in a pure state by recrystallisation. Bromal boils, without decomposition, at 173°, and is not congealed at −20°. The hydrate of bromal fuses at 53·5, cannot be distilled without decomposition, being converted into water and bromal, which latter, on being mixed with perfectly absolute alcohol, yields a bromal-alcoholate, a solid crystalline body, fusing at 44°, difficultly soluble in water, but readily so in alcohol and ether; on being submitted to distillation it is converted into alcohol and bromal. The formula of this alcoholate of bromal is $C_2Br_3OH+C_2H_6O$. A large portion of the further contents of this memoir is devoted to exhaustive researches on bibrom- and tribrom-acetic acid, and compounds and derivatives thereof.

Action of Phosphuretted Hydrogen upon the Iodides of Methyl and Ethyl.—Dr. A. W. Hofmann.—This very lengthy essay contains the account of a series of researches instituted by the author, in continuation of former experiments, on the preparation of phosphines. The essay, which contains the experiments in the methyl and ethyl series, is not well suited for useful abstraction.

Coal-Tar Cresols.—Dr. H. L. Buff.—Upon a portion of creosote boiling at from 204° to 205°, the author has caused chloride of benzoyle to act, thereby obtaining a solid and a liquid body. The former, a crystalline substance, fuses at 70°, and is, as regards its properties, identical with the benzoate of para-kresol obtained by MM. Latschinoff and A. Engelhardt; the liquid cresol is perfectly colourless, boils at 205°, and is stated by the author to be a para-kresol.

Some Combinations of Tolan.—Drs. Limpricht and Schwanert.—This is a preliminary notice of some researches, not yet finished, on the action of chlorine compounds and bromine upon tolan, which yields, it appears, several isomeric chlorides and bromides.

New Compounds of the Camphor Series.—Dr. I. Kachler.—This paper, a fragment of a large essay the author intends to publish, contains the record of some researches on the action of nitric acid upon camphor. It appears that not only camphoric and camphresic acids are formed, but also a fluid combination of camphor and nitric acid—a nitrate of camphor—and another peculiar acid. The nitrate of camphor, $2(C_{10}H_{16}O).N_2O_5$, is a rather unstable, liquid, oily compound, instantaneously decomposed by water, metals, and their oxides, ammonia, aniline, and phenol, but soluble, without decomposition, in alcohol and ether.

Thio-Aniline and Thio-Toluidine.—V. Merz and W. Weith.—This exhaustive and very lengthy monograph is not well suited for any useful abstraction.

Some of the Hydrocarbons of the Marsh Gas Series.—C. Schorlemmer.—The author gives a series of formulæ of hydrocarbons prepared by him, and the boiling-points of these substances, all of which are normal—that is to say, that the carbon-atoms are united in a simple manner.

Contribution to our Knowledge on Ditolyles.—Dr. Th. Zincke.—This paper, illustrated with woodcuts, strictly belongs to crystallography.

Researches on Valeraldehyde.—Dr. A. Schröder.—This essay contains the following sections:—Preparation and properties of valeraldehyde; action of chlorine upon valeraldehyde; sulpho-valeraldehyde and seleno-valeraldehyde.

Researches on the Sulphuretted Urea.—A. Arzruni.—The author describes, at great length, the mode of preparation of a sulphourea from amido-benzoic acid. The new body, $C_8H_8N_2O_2S$, named monoxy-benzoyl-sulpho urea, is insoluble in cold water, alcohol, and ether, but soluble in boiling water and alcohol. On being heated by itself, this new compound is decomposed, giving off sulphuretted hydrogen, and yielding oxybenzoyl-urea, $C_8H_6N_2O_2$. The relation which the monoxy-benzoyl-sulpho urea bears to other already known similar compounds is set forth by the following formulæ:—Monoxy-benzoyl-sulpho urea—

$$\left.\begin{matrix}(CS)''\\C_7H_5O_2\\H_3\end{matrix}\right\}N_2;$$

dioxy-benzoyl-sulpho urea—

$$\left.\begin{matrix}(CS)''\\(C_7H_5O_2)_2\\H_2\end{matrix}\right\}N_2;$$

monoxy-benzoyl-phenyl-sulpho urea—

$$\left.\begin{matrix}(CS)''\\C_7H_5O_2\\C_6H_5\\H_2\end{matrix}\right\}N_2.$$

Annales des Mines, No. 4, 1870.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

New Process for the Treatment of Gold and Silver Ores.—L. E. Rivot.—This very valuable monograph is divided into the following chapters:—Introduction; auriferous veins and lodes; argentiferous veins and lodes; amalgamation processes; laboratory experiments, conducted by MM. Regnault and Cumenge; first series of experiments on the action of steam upon simple native and artificially-prepared sulphurets, and upon complex sulphurets; action of super-heated steam upon these and other similar substances; combined action of super-heated steam and oxide of iron, or roasted pyrites mixed with the substances just alluded to; experiments on the large scale made in California; laboratory experiments on amalgamation and other metallurgical operations; observations on amalgamation, and the influence thereupon of oils, lime, and metals; metallurgical methods and apparatus. The essay is illustrated by a series of engravings.

Condensed Report of the Labours Performed at the Departmental Chemical Laboratory established at Mézières (Ardennes).—E. Nivoit and M. Létrange.—This paper contains, among other particulars, a series of analyses of phosphatic nodules (so-called coprolites) found in various localities of this part of France. It appears that the quantity of phosphoric acid contained in these nodules averages 18, does not exceed 22, and not fall below 13 per cent. Average composition of the metal of an old bell of Mézieres, in 100 parts—Tin, 20·91; lead, 2·74; zinc, a trace; copper, 76·35. Cadmia from a blast furnace was found to consist, in 100 parts, of—Oxide of

zinc, 89·10; oxide of lead, 5·91; oxide of iron and alumina, 1·90; lime, 0·97; sulphuric acid, 0·13; silica, 0·56;—total, 98·57.

Memoir on the Principal Sites of the Mineral Lodes and Veins of Saxony and Northern Bohemia.—MM. Lévy and Choulette.—This essay is a very valuable contribution to the knowledge of the metallurgically-workable minerals in the countries named.

Report on the Official Inquiry on the Condition of the Miners and Factory Workers in Belgium.—M. Demongeot.

Bulletin de l'Académie Impériale des Sciences de St. Petersbourg, vol. xv., No. 4, 1870.

This number does not contain any papers relating to chemistry or allied sciences.

Vol. xv., No. 5, 1871.

The only papers relating to physical sciences in this number are :—

Preliminary Notice on the Application of Secondary or Polarisation Batteries to Electro-Magnetic Motors.—Professor Jacobi.—The author first enters at length into theoretical discussions on the various polarisation batteries, or substitutes therefor, proposed at various times by different physicists, more especially in England and France. The author next describes a polarisation battery made of leaden plates put into sulphuric acid (1·3 sp. gr.), and each having 900 centimetres' surface, the troughs (6·5 centimetres long by 6·3 wide and 13·6 high) being made of gutta percha. Of this kind of galvanic element the author employs fifty, and charges them by the aid of four large-sized Grove's or Bunsen's cells.

Micrographical Researches.—Dr. A. Stuart.—This paper, illustrated by woodcuts, contains a detailed description of a new and ingeniously-devised plan for the measurement of objects seen under the microscope.

Journal für Gasbeleuchtung und Wasserversorgung, No. 7, 1871.

The only matter relating to chemistry and collateral sciences met with in this paper is—

Oxygen, its Occurrence, Preparation, and Application for Illuminating Purposes; and on a New Method of Oxygen Illumination.—Dr. J. Philipps.—This periodical also contains a brief review of the work of the author just named, and recently published by him in German at Berlin. It appears that the carboxygen lamp invented by the author, and obtainable from the firm of Georg Berghausen, at Cologne, is, in every respect, a great success. The lamp need not be fed with pure oxygen, the author having found that an artificial air containing 50 per cent of oxygen yields light of the same intensity as pure oxygen. The lamp consumes, per hour, 4·8 English cubic feet of the gas just alluded to under a pressure of 30 m.m., and with a consumption of 20 grms. of a fluid the nature of which is not explained. The author's pamphlet is illustrated with woodcuts and two lithographic plates.

Journal fur Praktische Chemie, No. 5, 1871.

The greater portion of this number is filled with the continuation and end of the lengthy essay—

Contribution to our Knowledge of the Geminated (Gepaarten) Compounds of the Pentatomic Nitrogen.—C. W. Blomstrand. —See Chemical News, vol. xxiii., p. 597.

The only other paper, on the—

Products of the Acid Fermentation of Wheaten Bran—A. Freund—Contains the very detailed description of a series of experiments made by the author with the view of thoroughly investigating the various products of the acid fermentation of wheaten bran. The main results arrived at are, that this fermentation mainly produces lactic acid, but, in addition thereto, formic, acetic, butyric, and capronic acids are formed; but neither propionic nor valerianic acids are generated. The author states that he is engaged in making further researches on this subject, taking care to cause the process of fermentation to proceed at varying temperatures, in order also to find thus, if possible, a more expeditious and advantageous method of preparing propionic acid, instead of its preparation from cyanide of ethyl.

NOTES AND QUERIES.

Nitrobenzol.—Will any of your readers kindly inform me how best and safely to distil nitrobenzol so as to obtain it colourless?—Mirbane.

Removing Sulphur from Coke.—(Reply to "R.")—You will find in Ronalds and Richardson's "Chemical Technology," vol. i., part i., p. 124, every information you desire on the subject; the quotation in our Notes and Queries would occupy too much space. The work referred to was published in 1855 by Messrs. Baillière. Consult, also, "Abridgements of all Specifications of Patented Inventions" (No. 30, Preparation and Combustion of Fuel), published by the Commissioners of Patents.

Removing Sulphur from Coke.—In reply to your correspondent, "R.," in Notes and Queries of your last issue, Professor Calvert patented, in 1851, a process for purifying coke similar to that described by him, the only difference being that the salt was mixed with the coal before coking in a solid form, instead of as brine. This process failed for the reason that, by the decomposition of the salt, sulphate of soda, and some other combinations of the sulphur and the sodium were formed (the chlorine escaping principally as hydrochloric acid probably), and by these means the sulphur, instead of being removed, was fixed in the coke. I am superintending the working out of a process, patented by my brother in 1868, for the removal of sulphur and phosphorus from coke and ores, and have obtained some good results from it. Calcination, or coking with common salt, is a feature in this process, but the ore and the coke are washed in water after the ignition. The compounds formed during the ignition being soluble in water, this effectually removes them.—F. J. Rowan, Atlas Steel Works, Glasgow.

Alteration of Arsenious Acid.—Can any of your readers tell me if it is known as a fact that arsenite of potash in solution is liable to alteration on keeping? Eight years ago, I made a solution, containing 139·6 grs. in a decigallon, for chlorine testing for a friend—a paper maker. Half the quantity has stood for some years in a stoppered bottle, half full; and, on testing it yesterday against a freshly-prepared solution, I found a discrepancy of $5\frac{1}{2}°$—the As_2O_3 had partly already become As_2O_5. If this is not a known fact, it may be useful to many of your subscribers to learn that, unless sufficiently freshly made, the arsenious acid solution is not an entirely reliable test. Though, regarding the amount of error introduced, and the time the solution had been kept, I should say that the error in one year would be hardly sufficient to impair its usefulness as a manufacturer's test.—John S. Linford.

Welding of Cast-Steel and Iron.—In a well-known American paper, we find a notice on the subject of welding cast-steel together, which briefly comes to the following, and might, perhaps, be tried in the welding of steel to iron. The author, evidently a practical man, states that he first upsets the two ends that are to be welded considerably larger than the welded part is to be when hammered down to the proper size; next, split one of the ends of the bar a little deeper than the bar is thick, draw out the ends of the lips and narrow them up at the points, and draw out the end to be welded into this into a blunt wedge shape, narrowing the end also; next make, with a cold chisel, a cut on one side of the wedge-shaped end, in order to prevent it from slipping out when sticking them together, and afterwards heat the two ends that are to be united to a cherry-red heat, using common borax; unite them in the usual way, driving them together endwise, then, closing down the two lips with the hammer at a bright cherry-red heat, they will stick together; then put on plenty of borax, and heat in a charcoal fire to a point just above a bright cherry-red heat, but not to a sparkling heat as in welding iron.

MEETINGS FOR THE WEEK.

Monday, June 5th.—Royal Institution, 2. General Monthly Meeting.
Tuesday, 6th.—Royal Institution, 3. Rev. Prof. Haughton, "On the Principle of Least Action in Nature."
—— Zoological, 9.
Wednesday, 7th.—Microscopical, 8.
—— Geological, 8.
—— London Institution, 2. Distribution of Prizes and Certificates by the President, Thomas Baring, Esq., M.P.
Thursday, 8th.—Royal Institution, 3. Prof. Tyndall, LL.D., F.R.S., "On Sound."
—— Royal Society Club, 6.
Friday, 9th.—Royal Institution, 9. Prof. Tyndall, LL.D., F.R.S., "On Dust and Smoke."
—— Astronomical, 8.
—— Quckett Microscopical Club, 8.
Saturday, 10th.—Royal Institution, 3. J. N. Lockyer, Esq., F.R.S., "On the Instruments Used in Modern Astronomy."
—— Quekett Microscopical Club. Excursion to Homerton; to meet at Broad Street Station at 2 p.m.

TO CORRESPONDENTS.

Errata.—In No. 600, p. 249, line 18 from bottom of left-hand column, *for* "$Tl_2PtCy_4,COTlO_2$" *read* "$Tl_2PtCy_4,COTlo_2$."
Page 250, line 5 from top of left-hand column, *for* "that no combination" *read* "that combination."
Page 250, line 2 from top of right-hand column, *for* "falls" *read* "fills.'

A Subscriber.—A work on the subject, by the Editor, is in the press; it will be duly announced.
Subscriber.—The product cannot, we believe, be obtained in England.
A. Ridge.—We have received the white carbonate; what are our correspondent's wishes concerning it?

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THE CHEMICAL NEWS.

VOL. XXIII. No. 602.

NOTE ON THE SPECTRUM OF URANUS AND THE SPECTRUM OF COMET I., 1871.*

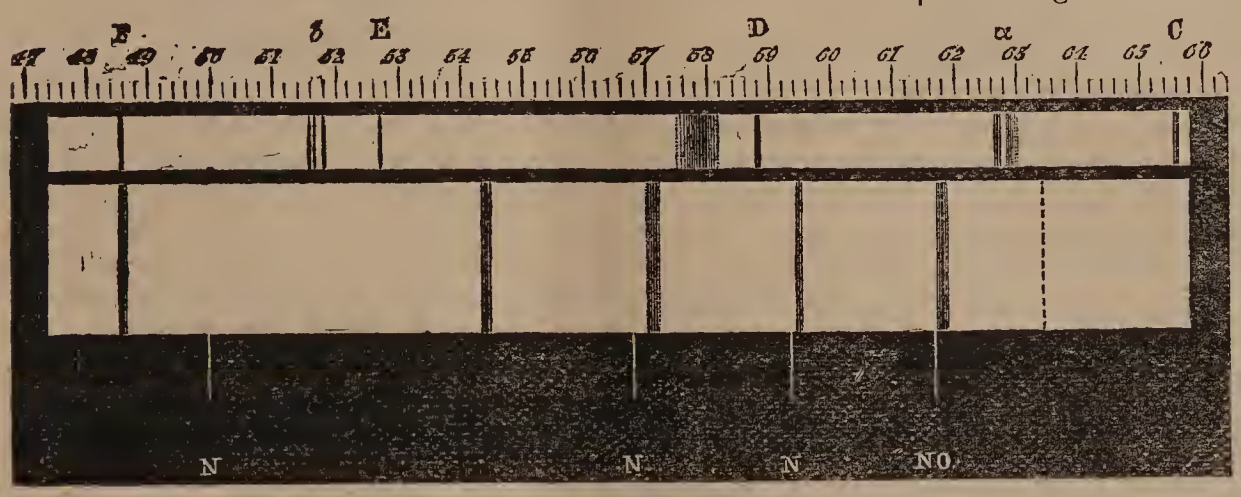
By WILLIAM HUGGINS, LL.D., D.C.L., V.P.R.S.

In the paper "On the Spectra of some of the Fixed Stars,"† presented conjointly by Dr. Miller and myself to the Royal Society in 1864, we gave the results of our observations of the spectra of the planets Venus, Mars, Jupiter, and Saturn; but we found the light from Uranus and Neptune too faint to be satisfactorily examined with the spectroscope.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in my hands by the Royal Society, I have succeeded in making the observations described in this paper of the remarkable spectrum which is afforded by the light of the planet Uranus.

It should be stated that the spectrum of Uranus was observed by Father Secchi in 1869.‡ He says—"Le jaune y fait complétement défaut. Dans le vert et dans le bleu, il y a deux raies très larges et trés naires." He represents the band in the blue as less refrangible than F, and the one in the green as near E.

The spectrum of Uranus, as it appears in my instrument, is represented in the accompanying diagram. The narrow spectrum placed above that of Uranus gives the



relative positions of the principal solar lines, and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre.

The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from about C to about G.

On account of the small amount of light received from this planet, I was not able to use a slit sufficiently narrow to bring out the Fraünhofer lines. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances.

The spectroscope was furnished with one prism of dense flint-glass, having a refracting-angle of 60°, an observing telescope magnifying 5½ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum.

The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave-length of about 544 millionths of a millimetre.

The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others.

The measures taken of the most refrangible band showed that it was at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen.

Three of the bands were shown by the micrometer not to differ greatly in position from some of the bright lines of the spectrum of air. A direct comparison was made when the principal bright lines were found to have the positions, relatively to the lines of planetary absorption, which are shown in the diagram. The band which has a wave-length of about 572 millionths of a millimetre is less refrangible than the double line of nitrogen which occurs near it. The two planetary bands at 595 and 618 of the scale appeared very nearly coincident with bright lines of air. The faintness of the planet's spectrum did not permit of certainty on this point; I suspected that the planetary lines are, in a small degree, less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely, the double line of nitrogen.

As carbonic acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took measures with the same spectroscope o the principal groups of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas.

There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere.

Spectrum of Comet I., 1871.

On April 7 a faint comet was discovered by Dr. Winnecke. I observed the comet on April 13 and May 2. On both days the comet was exceedingly faint, and on May 2 it was rendered more difficult to observe by the light of the moon and a faint haze in the atmosphere. It presented the appearance of a small faint coma, with an extension in the direction from the sun.

When observed in the spectroscope, I could detect the light of the coma to consist almost entirely of three bright bands.

A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. I was not able to do more than estimate roughly the position of the less refrangible band. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangible side.

It would appear that this comet is similar in constitution to the comets which I examined in 1868.*

* Read before the Royal Society, May, 1871.

† *Phil. Trans.* 1864, p. 413; and for Mars, *Monthly Notices R. Ast. Soc.*, vol. xxvii., p. 178.

‡ *Comptes Rendus*, vol. lxviii., p. 761, and *Le Soleil*, Paris, 1870 p. 354.

* *Phil. Trans.* 1868, p. 555; and *Proc. Roy. Soc.*, vol. xvi., p. 386.

ON A

NEW METHOD OF PREPARING SUPER-SATURATED SALINE SOLUTIONS.

By L. C. De COPPET, Ph.D.

IN 1868-69 I made, in the laboratory of the Royal College of Chemistry, a series of experiments on supersaturated solutions, of which the following are some of the results.*

Supersaturated solutions of sodic sulphate are usually prepared by cooling, in closed vessels, solutions saturated at a higher temperature.

I have found that so-called supersaturated solutions of sodic sulphate may be prepared at ordinary temperatures by simply dissolving the anhydrous salt in cold water.

To ensure the success of the experiment it is necessary to protect the solution from contact with the dust floating in the atmosphere. The anhydrous salt itself must be rendered "inactive" if for any length of time it has been exposed to the air. This is done by heating it to a temperature above 33° C. (I have found 40° C. sufficient, but it is safer to heat as high as 100° or 200°) and cooling in a closed vessel.

The salt must be added to the water in small quantities at a time, first, because the heat evolved by its contact with the water would otherwise be sufficient to raise the temperature several degrees, and it might be objected that this was the cause of the supersaturation; secondly, because the salt, if added all at once, forms a hard cake which dissolves but slowly.

The greater part of my experiments were conducted in the following way:—The salt was put into a short wide tube sealed at one end, drawn out and bent at right angles at the other, so as to pass, together with a thermometer, through the neck of a small flask. When this tube containing the anhydrous salt had been heated and cooled in order to render the salt "inactive," and the flask well washed and half-filled with cold water, the narrow end of the tube was introduced into the flask (not so far as to touch the water), and the space left between it, the thermometer, and the neck of the flask filled with cotton-wool. By gently shaking the tube, the salt would fall into the water, a very little at a time, and dissolve, without sensible rise of temperature, in much larger quantities than the ordinary crystallised salt ($Na_2SO_4,10H_2O$).

If the flask is placed in a large water-bath, the temperature may be kept very nearly constant throughout the experiment.

On one occasion, the temperature of the water at the beginning of the experiment was 13·5° C. Salt was added, and the flask often shaken, to hasten the solution, without being withdrawn from the water-bath. In two hours the temperature had slowly risen to 14°, and it being then evident that no more salt was dissolved—in other words, that the solution was saturated with anhydrous sodic sulphate—a portion of the perfectly clear menstruum was poured into several clean dry flasks, each provided with a cork. Two of these flasks I succeeded in weighing before the solution crystallised, and found both times that the strength of the solution was 35·8 parts of anhydrous sodic sulphate (Na_2SO_4) to 100 water. The concentration of this solution was, therefore, nearly three times that of the ordinary solution saturated with the normal salt—

$$Na_2SO_4,10H_2O,$$

which, at 14° C., contains 12·4 parts anhydrous salt to 100 water.

Another experiment showed the solubility of anhydrous sodic sulphate at 21° C. to be very nearly 52·7 parts of Na_2SO_4 to 100 water; the solubility of the hydrate—

$$NaSO_4,10H_2O,$$

at the same temperature is but 20·9 parts anhydrous salt to 100 water.

These experiments show that an ordinary saturated solution of anhydrous sodic sulphate is identical with what is commonly called a supersaturated solution. Of course I do not mean to imply by the words "solution of anhydrous sodic sulphate" that the salt *in* the solution is in the anhydrous state.

It is now very generally admitted that crystals of the 10-atom salt ($Na_2SO_4,10H_2O$) always bring about the solidification of a supersaturated solution of sodic sulphate. Among the many who of late years have investigated the subject, Mr. Tomlinson is, I believe, the only one who holds the opposite view.

Completely effloresced Glauber's salt, though anhydrous, also invariably causes crystallisation.

It would seem from this that anhydrous sodic sulphate, prepared by drying crystals of $Na_2SO_4,10H_2O$ at temperatures below 33° C. is not identical with that prepared by heating at temperatures above 33°. They are, probably, isomeric modifications. The former, whether anhydrous (effloresced) or combined with 10 atoms water, always causes the supersaturated solution to crystallise; the latter, whether anhydrous or combined with 7 atoms of water not only never does so, but may be dissolved in a solution already highly supersaturated.

Supersaturated solutions of sodic carbonate and of magnesic sulphate may be prepared without heating in the same manner as those of sodic sulphate. I used for the purpose anhydrous sodic carbonate and partially dehydrated magnesic sulphate.

* A more detailed account of the experiments here referred to was published early in 1869 in the *Bulletin de la Société vaudoise des Sciences Naturelles*, vol. x., p. 145.

ON A METHOD OF

FIXING, PHOTOGRAPHING, AND EXHIBITING THE MAGNETIC SPECTRA.*

By ALFRED M. MAYER, Ph.D.

THE figures produced in iron-filings, when these are set in momentary vibration on a surface placed over a magnet have received considerable attention from natural philosophers. The geometrical discussion of these spectra made by Lambert, Roget, and others, have developed their symmetrical properties, and thereby have evolved the law of that action which emanates from the magnet. De Haldat has used them as a means of exploring the distribution and intensity of the effect of juxtaposed magnets variously arranged. But, above all, have the researches of Faraday and W. Thomson on "the magnetic field," and on "the lines of magnetic force" given to these spectra—even when merely regarded as conventional symbols—an importance which has been fully shown; especially by Faraday, who was guided by their consideration to some of his most important discoveries. They have thus risen to so high a theoretical importance that a method which will fix them without danger of distortion, photographically reproduce them, and readily serve to exhibit them to the largest audiences, will, I imagine, be acceptable to both investigators and lecturers.

The only process of fixing these spectra known to me is that practised by De Haldat and Faraday, which, however, is but an application of the magnetic spectra of the method previously invented by Savart for preserving the Chladni figures of vibrating plates. In this process the spectra, produced in the usual manner either on glass or card-board, have pressed upon them a sheet of paper coated with mucilage, to which the filings adhere. In this operation of the transfer many particles are deranged from their positions and the figures are yet more distorted by the shrinkage of the wet paper, and are therefore not fit to

* See a neat "Démonstration par le calcul des courbes magnétiques de le loi de l'inverse du carré de la distance," by M. Cellerier, published as a note on p. 592, vol. i., of De la Rive's Traité d'Electricité.

serve in measures of precision; while the impressions cannot be exhibited with much more facility than the originals.

My process is as follows:—a clean plate of thin glass is coated with a firm film of shellac, by flowing over it a solution of this substance in alcohol,* in the same manner as a photographic plate is coated with collodion. After the plate has remained a day or two in a dry atmosphere, it is placed over the magnet, or magnets, with its ends resting on slips of wood, so that the under surface of the plate just touches the magnet. Fine iron-filings, produced by "draw-filing" Norway iron which has been repeatedly annealed, are now sifted uniformly over the film of lac by means of a fine sieve. The spectrum is then produced on vibrating the plate, by letting fall vertically upon it, at different points, a light piece of copper wire. The plate is now cautiously lifted vertically off the magnet and placed on the end of a cylinder of pasteboard, which serves as a support in bringing it quite close to the under surface of a cast-iron plate (1 foot in diameter, ½ inch thick), which has been heated over a large Bunsen-flame. Thus the shellac is uniformly heated, and the iron-filings, absorbing the radiation, sink into the softened film and are "fixed."

I generally allow the heat to act until the metallic lustre of the filings has disappeared, by sinking into the shellac, and the film appears quite transparent. This degree of action is necessary when photographic prints are to be made from the plate, but when they are to be used as lantern slides I do not carry the heating so far. After the plate has cooled, it is allowed to fall upon its ends, on a table, so that any filings which have not adhered may be removed.

A short experience will give the proper strength of shellac solution to obtain a film so thick as *just to be sufficient to hold the filings*, and the requisite amount of heat to firmly cement them, without injuring the transparency of the film.

The plates can now serve (1) for the most accurate measures upon the magnetic field; (2) for a photographic positive, which, in the printing-frame will produce the lines in white upon a dark ground, giving most beautiful and distinct impressions;† (3) or, if it is required to exhibit these figures to an audience, the plates are provided with glass covers, kept from touching the spectra by intervening slips of cardboard, and there result "slides," in every way fit for giving a fine exhibition, when the images are projected upon a screen. I have thus obtained images, clear and sharp, of over 12 feet in diameter.

By this process many plates have been produced;‡ showing the action of single magnets of various forms, and of juxtaposed bars; as well as the effects of electric currents led by wires through holes drilled in the plates. Those exhibiting the inductive action of magnets on bars of soft iron and the interaction of magnets and electric currents are peculiarly interesting. An approximate representation of the resulant lines of the terrestial magnetic action has been obtained by magnetising *equably tempered* steel discs of from 2 to 3 inches and even more in diameter. The magnetic axis or axes of these discs are *predetermined* by making them the continuations of the axes of very powerful electro-magnets, terminated with cones of soft iron with slightly rounded apices. The arcs of the great circles, including the terrestrial magnetic poles, having been calculated, the axes of the electro-magnets are inclined to that angle, while the steel disc is held close to their poles. On passing the current the disc is magnetised and we have an approximate representation of a section of the earth's magnetic effect. These results when viewed as photographic prints, or, as exhibited by the lantern, are so beautiful and instructive as to appear to me to warrant this somewhat formal description of the process of their production.

PS. Since sending the above to press I have subjected plates, coated with "sensitised" collodion to the action of the magnetic field. I had hopes of thus obtaining a *physical* impress on the plate which would appear on flowing the "developer." Sensitised films on glass and on iron plates were placed over and between the poles of an electro-magnet with cores 1·7 inches in diameter. Some plates were developed after removal from the magnet, others while under the magnetic action—with and without the light having acted upon them—but no trace of effect has been detected.

I had also imagined that the magnet's action should have placed the affinities in a more unstable condition, so that the film would rise in sensitiveness after exposure in the magnetic field; but this, also, I could not detect; nevertheless, I have not given up the supposition that some action will be evolved when more appropriate films, far higher magnetic action, and more delicate measures of actinic effect are used.—*American Journal of Science.*

* The shellac dissolved in strong alcohol is allowed to stand a week or more, and the clear supernatant solution is then decanted.

† Photographic prints from a series of eight of these plates I have presented to Harvard College: American Academy of Sciences; Sheffield Scientific School; Columbia College; Stevens's Institute of Technology, Hoboken; Lehigh University, Pa.; American Philosophical Society; Franklin Institute; Peabody Institute, Balt.; Smithsonian Institution; Chicago Academy of Sciences; and to the University of Virginia.

‡ Several of these are 16 inches long by 10 wide.

ON THE

EFFECT OF CERTAIN METALLIC SUBSTANCES BESIDES IRON ON CARBONIC OXIDE, INCLUDING SILICON.*

By I. LOWTHIAN BELL.

(Concluded from p. 260).

Cobalt.

A COBALT oxide was prepared from the purified nitrate in the same way as the oxides of nickel above described.

It was almost pure Co_3O_4 (Co 73·5, O 26·5), its composition being—

Co	73·3
O	26·7

A small trace of Ni might be present, but if so, it could not materially affect the result, inasmuch as CoO is much more powerful in splitting up CO than is NiO.

Specimens of this oxide of cobalt (Co_3O_4), were exposed in the lead-bath arrangement.

		Expt. 399.	*Expt.* 400.
Time of exposure—		7½ hours.	10 hours.
Temperature—		Zn fused, Sb not.	Zn fused, Sb not.
Composition of product.	Co soluble in I	47·2	17·3
	Co insoluble in I		20·4
	Carbon	46·4	56·8
	Oxygen	6·4	5·5
		100·0	100·0

The condition in which the cobalt exists was not determined—that is, whether part as metal and part as oxide, or whether the whole was as suboxide. In Experiment 399, the oxygen per 100 of total cobalt present was 13·56, and in Experiment 400 14·58, which correspond nearly with Co_2O (100 Co, 13·6 O). It was ascertained that the anhydrous Co_3O_4 used was attacked by iodine and water, so that it was not possible by the use of this reagent, as in the case of iron and nickel, to distinguish between metallic cobalt and its oxide. The oxygen in Experiment 400 existed to the extent of 27·0 per 100 parts of that cobalt which was found to be insoluble in iodine and cold water, which indicates that the action of iodine on the Co_2O probably formed gives rise to CoO, and not Co_2O_3; the former formula requiring 27·1 of O per 100 of Co, thus:—

$$Co_2O + I_2 = CoI_2 + CoO,$$

* From the "Chemical Phenomena of Iron Smelting."

which indicates a double difference between the behaviour of the lower oxides of Co and Fe under these circumstances. The fact that the Co_3O_4 originally used was attacked by iodine, however, probably indicates that Co_2O_3 is producible by the action of iodine on the lower oxides; a reaction probable, *à priori*, from the well-known formation of Co_2O_3 from the action of chlorine on alkalised solutions of cobalt.

The presence of CO_2 to the extent of an equal volume does not prevent CO from completely reducing the cobalt oxide under examination (Co_3O_4).

Expt. 401.	*Expt.* 402.
Temperature—Dull red.	Temperature—Bright red.
O lost, in 2½ hours, 26·5 per ct.	O lost, in 3 hours, 26·6 per ct.
3 ,, more, 0·1 ,,	
26·6 ,,	

the original oxygen being 26·7 per cent.

The effect of cobalt in its metallic state was next tried on pure CO, the Co being obtained by acting on Co_3O_4 by H, until no more H_2O was formed.

		Expt. 403.	*Expt.* 404.
Time of exposure—		5 hours.	4½ hours.
Temperature—		Melting zinc.	Low red heat.
Composition of product.	Co	98·3	99·7
	C	1·4	0·1
	O	0·3	0·2
		100·0	100·0

To ascertain the power of the spongy metallic cobalt to withdraw O from CO_2, in—

Expt. 405.—120 c.c. of gas unabsorbed by KaHO was collected by passing CO_2 over Co at a bright red heat for thirty-five minutes. The 120 c.c. so obtained contained 96 per cent of CO.

Platinum—

Although there was little probability of a metal having so feeble an affinity for oxygen as platinum to exert any power of splitting up CO, the following experiments were performed, under the idea that possibly the dissociation of CO by heat alone (which Experiment 373 *et seq.* show, does not readily take place), might be assisted by the occlusion of that gas in the pores of a spongy substance gifted with high absorbing powers for gases.

This metal was obtained in its spongy condition—

(A) by ignition of ammonium chloroplatinate.
(B) ,, potassium chloroplatinate and washing product.

Expt. 406.—Spongy platinum (A), inert, at melting zinc.
Expt. 407.— ,, ,, ,, ,, low red heat.
Expt. 408.— ,, ,, (B), ,, melting zinc.
Expt. 409.— ,, ,, ,, ,, low red heat.

In one of these cases there was a trace of carbonaceous matter deposited, due, probably, to the presence of hydrocyanic acid vapour in the CO employed, as the gas used was not wholly freed from this impurity.

Silicon—

Expt. 410.—A specimen of this element, in its graphitoidal form, weighing 1·405 grms., dried at 100° C. (212° F.), lost .. .. 0·005
Just ignited, and lamp removed, lost .. .. 0·013
0·018
Then heated to redness for half an hour, it gained .. .. 0·006
Ditto, half an hour more, it gained .. .. .. 0·004
0·010

The substance then contained some body, probably carbon, which exposure to air at a red heat removed, and some modification of silicon, oxidisable at this temperature.

The 1·397 grms. of silicon remaining after this treatment were exposed to a stream of pure CO for six and a half hours, at a temperature a little above that of melting zinc:—

No change in weight.

Expt. 411.—The same specimen was then exposed to CO at a bright red for a period of six hours, precautions, before and after the experiment, in both cases, being observed to exclude atmospheric air:—

	Per cent
The gain in weight was	0·18
Burnt in oxygen, gain on original substance was	0·29
CO_2 collected = 0·08 grm., equal to C	0·15

Looking at the previous behaviour of this specimen of graphitoidal silicon, it is doubtful whether this small quantity of C is owing to carbon deposition, but may rather be due to trace of C or graphite in the silicon itself.

Titanic Acid—

Expt. 412.—Cubical crystals of nitride of titanium are commonly found in the hearths of extinguished blast-furnaces. The specimen of amorphous TiO_2 employed was obtained by igniting ammonium titanate: 0·464 grm. was exposed for six and a half hours to a temperature a little above that of melting zinc. The TiO_2 was faintly blackened on its surface:—

	Per cent.
The gain in weight was	1·2
Burnt in oxygen, it lost again	1·0
CO_2 collected 0·019 = C on original TiO_2	1·1

Expt. 413.—The specimen of TiO_2, freed from carbon by ignition in O, would consist of—

	Grm.
Ti	0·283
O	0·181
	0·464

It was exposed to a bright red heat for six hours to a stream of pure CO. It was ascertained, by combustion in oxygen, collection of CO_2, and difference of weight, to be now composed of—

Ti	0·2830
C	0·0040
O	0·1725
	0·4595

From these numbers, it appears that the TiO_2 has lost 4·7 per cent of its oxygen, and deposited carbon to the extent of 0·9 per cent of the TiO_2 used, or 1·19 per cent of the metallic Ti present.

ON THE

METHODS OF ANALYSIS AND THE COMPOSITION OF VARIOUS CHEMICAL MANUFACTURING PRODUCTS.

By M. GASTON TISSANDIER.

Palm Oil.

PALM OIL is prepared from the fruit of the palm tree (*Clæis Guineensis*) as follows:—On its arrival at maturity, the fruit is plucked and thrown into a heap on the ground, where it is left for about a month. Fermentation is thus produced. When this is sufficiently advanced, the mass is thrown into large iron vats and boiled with water, the fruits being crushed in the hands of the labourers from time to time. After prolonged boiling, they are pounded in rude mortars formed from the trunks of trees, the kernels are removed, and the shells again boiled. The oil then floats on the surface of the liquid, and is collected with large wooden spoons.

Properties.—Palm oil is solid at the ordinary temperature of our climates, its colour is a reddish yellow, and it is esteemed in proportion to the deepness of its colour. Its perfume resembles that of the iris or the violet. It melts at from 30° to 35° C., according to its age, but when freshly prepared its melting-point is lower. It is easily soluble in alcohol, and still more so in sulphide of carbon and ether. About 80,000 tons are annually manufactured. The oil mixes readily with water, which is frequently used to adulterate it: 50 per cent may be added without fear of detection by the most practised eye. In order to estimate the water contained in palm oil, a certain weight of it must be heated on a stove at 110° C.; desiccation will then ensue in a few hours. Unadulterated commercial palm oil contains from 3 to 8 per cent of moisture. A very superior quality of oil is obtained from the kernels of the palm, which are very large.

Ash.—Sand or earth is frequently mixed with palm oil during manufacture; to estimate this, calcine 50 grms. of oil in a porcelain capsule, and after the charcoal thus formed is burnt off, weigh the ash obtained. Palm oil sometimes contains from 4 to 5 per cent of sand.

Impurities.—Sundry organic substances, such as the remains of palm leaves, &c., also occur; in order to estimate these, melt 100 grms. of oil in a capsule upon a water-bath, let it stand an hour, decant the limpid oil, and treat the precipitated impurities with sulphide of carbon, so as to dissolve the oil which adheres to the sides of the vessel. The impurities must be thrown on a filter, washed with sulphide of carbon, dried at 100°, and weighed. Some analyses are subjoined:—

Loss in Palm Oil.

	I.	II.
Moisture	8·25	3·12
Ash	0·45	1·20
Organic impurities	1·01	0·80
Total loss	9·71	5·12

White-Lead—Zinc-White.

Commercial white-lead is frequently adulterated with sulphate of baryta, carbonate of baryta, and carbonate of lime. The presence of sulphate of baryta may be tolerated when its proportion does not exceed 5 per cent.

The mode of analysis is as follows:—Weigh 1 grm. of white-lead, and calcine it in a small porcelain capsule; treat the residue, while hot, with pure nitric acid diluted with water—this dissolves the oxide of lead, while the sulphate of baryta remains insoluble.

Now filter, and precipitate the filtered liquid with sulphuretted hydrogen; a formation of sulphide of lead will then ensue, and this must be dried at 100° C. and weighed. The liquid, separated from the sulphide of lead, is treated with ammonia and sulphhydrate of ammonia, which will betray the presence of oxide of zinc, which sometimes occurs in white-lead, by precipitating it as sulphide of zinc. Filter this latter, and evaporate the filtered liquid to dryness, previously adding chlorhydric acid; take up the residue with water and chlorhydric acid, and add ammonia and oxalate of ammonia, which will precipitate the lime as insoluble oxalate of lime.

The sulphide of lead is converted, by calculation, into carbonate; the sulphide of zinc is dissolved in chlorhydric acid after calcination, and precipitated as carbonate of zinc by the addition of carbonate of soda. The carbonate of zinc is collected on a filter, washed with boiling water, dried, calcined, and weighed; the calcination transforms it into oxide of zinc, which is thus extracted directly from the specimen of colour under examination.

To ascertain whether the white substance which was insoluble in the acid liquid used in the first experiment be really sulphate of baryta, proceed as follows:—Mix it thoroughly with dry, pure, finely-powdered carbonate of soda; heat it to redness in a platinum crucible, until the fluid mass no longer effervesces; let it cool, and boil it with warm water, which dissolves the sulphate of soda formed, without acting upon the carbonate of baryta which is formed during the reaction. The aqueous solution should be precipitated by chloride of barium, which determines the formation of insoluble sulphate of baryta; the insoluble substance remaining on the filter is treated with pure chlorhydric acid diluted with water, and should yield a precipitate of sulphate of baryta on the addition of a drop of sulphuric acid.

Occasionally, but rarely, kaolin and clay are met with in white-lead.

The white-lead to be tested is not invariably met with in powder; the specimen may be mixed with oil and turpentine; sometimes its adulteration is not suspected till after its application to walls or wainscoting. In the first case, in calcining the well-mixed paint, take no notice of any oil that may be burnt. In the second, scrape the paint off the wall, and treat it in the same way; care must be taken not to scrape the stone also; it is better to pay attention to the sulphate of baryta only, neglecting any carbonate of lime which may result from the calcareous wall.

Composition of Adulterated White-Lead.

Substances estimated.	I.	II.
Carbonate of lead	85·25	44·33
Oxide of zinc	—	5·30
Sulphate of baryta	10·12	40·25
Carbonate of lime	4·63	10·12
Clay	—	—
	100·00	100·00

Specimen No. II. is by no means the most adulterated with which we have met. Some months ago, a so-called white-lead was analysed by us which contained no trace of lead; it consisted of carbonate of lime and sulphate of baryta, the oil and turpentine being replaced by soap-suds.

Zinc-white is subject to the same adulterations as white-lead, and the analytical process for oxide of zinc is the same as for white-lead.

Water used for Feeding Boilers.

All who have the management of steam-engines must have been inconvenienced by their boilers becoming incrusted with deposits, or corroded by nitrous vapours or chlorhydric acid, arising from impurities in the water employed. Water of good quality, such as river water, spring water, and that of streams, seldom contains in solution more than from 1 to 3 decigrms. per litre of saline matter; this generally consists of carbonate of lime, alkaline chlorides, sulphate of lime, and a small quantity of organic matter.

When water contains much carbonate of lime, say 5 to 10 decigrms., it will encrust the boiler, forming a hard and injurious deposit; this is a *calcareous* water. If it contains much sulphate of lime, say 1 grm., it will leave an abundant deposit, and will not cook vegetables or dissolve soap; this is a *selenitic* water. Lastly, if it contains nitrate of magnesia or chloride of magnesium, it produces nitrous vapours and chlorhydric acid, which corrode the metal; this is a *corrosive* water.

To ascertain whether the above-mentioned substances are contained in a water, pour 1 litre of it into a very clean, deep dish, and evaporate it to dryness over the water-bath at 100° C. The residue must be carefully scraped off the dish with a platinum spatula, swept together with a brush, and weighed; its weight will give some notion of the quality of the water, which is pure in proportion to the smallness of the deposit.

Nitrate of Magnesia and Chloride of Magnesium.—The dry residue is weighed, and extracted with alcohol at 40°; this dissolves only the chloride and nitrate of magnesia and a small quantity of alkaline chlorides. Filter, wash with alcohol, and evaporate the alcoholic solution to dryness over the water-bath on a platinum capsule.

Heat the residue gradually at a higher temperature; if red nitrous vapours appear, it contains nitrate of mag-

nesia; if the vapours are white and acid, and form a thick white cloud upon contact with a glass rod moistened with ammonia, they consist of chlorhydric acid. In both these cases, the water is corrosive, and on its evaporation will attack metals. After completing the calcination, filtering off the resulting magnesia, and carefully weighing it, it may be changed by calculation into nitrate of magnesia or chloride of magnesium.

This method is far preferable to the direct estimation of the magnesia in water; for water may be magnesian without being corrosive, as all salts of magnesia do not thus yield acids when subjected to heat. Here the acids are plainly perceived, and the corrosive action is proved.

Chlorine.—Measure 1 litre of the water to be tested, and precipitate the chlorine by a slight excess of nitrate of silver, collect the chloride of silver on a filter, and wash it with boiling water. After drying on a stove, fuse partially in a small tared porcelain capsule, weigh it, and calculate the quantity of chlorine contained.

Sulphuric Acid.—Precipitate 1 litre of water by chloride of barium. The sulphuric acid is precipitated as sulphate of baryta.

Lime.—Precipitate 1 litre of water with oxalate of ammonia, and all the lime will be precipitated as oxalate of lime.

The sulphuric acid is transformed into sulphate of lime, and the excess of lime calculated as carbonate of lime.

The estimation of these substances may be directly verified by working upon the dry residue which was exhausted by the alcohol. That portion which is insoluble in alcohol is treated with water, which dissolves the chlorides and alkaline sulphates, and leaves the carbonates of lime and magnesia, sulphate of lime, and silica in an insoluble state.

That part of the residue which is insoluble in water must be treated with acetic acid, which dissolves only the carbonate of lime, and does not attack the sulphate. Evaporate the acetic solution to dryness, calcine the residue, and all the carbonate of lime contained will be reproduced. By evaporating the aqueous solution, the alkaline chlorides and sulphates are obtained; it is, therefore, obvious that by a succession of solvents it is possible to separate all the contained salts, and in some measure to isolate them.

It need scarcely be said that these methods are only intended to be practically and expeditiously applied by operators, and that no attempt at a mode of completely analysing water is here contemplated.

Some estimate of the purity of water may be gained by simply examining the precipitates yielded with nitrate of silver, chloride of barium, and oxalate of ammonia. Pour these three reagents successively into a water of good quality, and the cloudiness will be slight; repeat the process with *selenitic* well-water, and abundant white precipitates will ensue, due to the action of the chloride of barium and oxalate of ammonia.

Hydrotimetry.—This method, which has been much over-rated, is far inferior to that above described. It is based upon the action of water upon a standard alcoholic solution of soap. This solution is poured into the water, drop by drop, by means of a burette, and well shaken at each addition, ceasing when a persistent froth is formed; the number of divisions of the burette gives the hydrotimetric standard. Unfortunately, when water is very much charged with sulphate of lime, it *immediately* yields a persistent froth; and I am now in possession of well-water which, containing 1·20 grs. of sulphate, should estimate more than 100°, whereas the soapy froth is persistent at 8°. The latter figure indicates that the water is excellent, while it is really execrable.

Purification.—When water contains much bicarbonate of lime, it may be freed from a portion of it by adding milk of lime, which will precipitate insoluble carbonate of lime. If the water to be purified be placed in large reservoirs, and agitated while the milk of lime is poured in, it will be found, when it has settled, to contain much less bicarbonate of lime. The action of lime also removes the corrosive properties of water; the lime, in fact, changing chloride of magnesium into chloride of calcium, which does not decompose under the influence of heat, and transforming nitrate of magnesia into nitrate of lime, which decomposes only at a very high temperature, and may be evaporated without evolving nitrous vapours. These reactions are, however, difficult to bring into practice, and unsuited to the purposes of manufacture.

MANUFACTURE OF SODA AND CARBONATE OF SODA FROM CRYOLITE.*

By J. LAWRENCE SMITH.

MANY and vain have been the attempts to supersede the process of Leblanc for the production of soda, and vast amounts of ingenuity and capital have been expended without success. But this statement does not apply to the production of soda, to a limited degree, out of a curious mineral found in Greenland called cryolite. This mineral was exhibited in abundance at the Exposition, and as there is an important manufacture of soda from it established at Natrona, Pennsylvania, the details of the method employed will be briefly described. These details have been kindly furnished to me by J. A. Hagemann, the chemist of the Danish Cryolite Mining Company, who directs at this time the manufacture of soda from cryolite by the Pennsylvania Salt Manufacturing Company. We may look for the best information on the subject of the above-named company's works, which are imitations on a large scale of the factory built by Julius Thomsen, of Copenhagen, in 1856. Some information has also been obtained from an essay by Samuel F. Simes.

Cryolite and its Source.

Before proceeding to a description of the process of manufacture, it is well to give some short account of this mineral and its present commercial movement.

The mineral cryolite, or "ice stone," has been known as a mineralogical rarity for many years, as far back as the latter part of the last century; thirty years ago it was worth fully a dollar an ounce, and it was considered only as a rare and curious mineral, of no value in the arts. Up to 1850 no attempt had been made to put it to any practical use, although it was known to exist in Greenland in large quantities and to contain a large amount of soda, for its composition is—

3	Sodium	69·00
2	Aluminium	27·26
6	Fluorine	114·00
		210·26

It has a specific gravity of 2·95, with a hardness of 2·05. Much of it is found colourless and pure. The associated minerals are galena, blende, spathic iron ore, iron pyrites, and copper pyrites.

With the exception of some few scattered specimens that occur at Miask, in the Ural mountains, it is found exclusively in immense quantity. It is procured from the mines of Ivigtout on the west shore of South Greenland, on Arsuk Fiord, between Julian's Hope and Frederick's, latitude 61° north, longitude 48° west. The main deposit here forms a mass of 600 feet in length, and 200 feet in width, and descending to an unknown depth. It lies at the base of granite hills, that rise a little distance from the edge of the Fiord, the shores of which are very bold and are almost fathomless a few feet from the shore.

The Danish Government is the owner of this mine, and in 1864 Christian IX. granted the exclusive right of mining

* From the Report of the United States Commissioner to the Paris Exposition.

cryolite to the "Cryolite Mining Company of Handelshelhabet," a company organised in Copenhagen. This contract was made by a Danish company, in consequence of the results of the labours of a young Danish chemist, Julius Thomsen, in 1850, who discovered a cheap and easy method of rendering cryolite available in technical chemistry for the manufacture of soda and alum, and his process is carried on in Europe and this country on such a scale as to consume annually 12,000 to 15,000 tons of cryolite.

Process of Decomposing Cryolite.

Thomsen's process of decomposing cryolite is simply by lime, either the wet or dry way. The decomposition may be represented by the following formula :—

$$2(Al_2Fl_3+3NaFl+6CaO)=$$
$$=3NaO+2Al_2O_33NaO+12FlCa.$$

Another method is to calcine finely-powdered cryolite and mix it with six equivalents of lime; the product will be "caustic soda, aluminate of soda, and fluoride of calcium." Another is to boil cryolite with the same proportion of lime in the form of milk of lime, and a similar decomposition takes place. The soda and aluminate are soluble in water; the latter is subsequently decomposed by carbonic acid. Both the wet and dry process will now be described.

The Dry Process.—The cryolite is dried in a furnace, and by a crusher is reduced to small fragments, which are ground in a burrstone mill to a fine powder, and uniformity in the powder is insured by passing it through a bolter of fine wire gauze. The powder is then mixed with slaked lime, or with pulverised chalk, in proportion to its purity, so that for each equivalent of pure cryolite there shall be a little more than six equivalents of lime. This mixture is effected on chaser mills, such as are used for crushing linseed and other oily grains. This mixture is now calcined, and here arises the great difficulty of the process, for the soda or carbonate of soda and fluoride of calcium fuse at a low temperature, and if allowed to fuse would almost entirely prevent the subsequent lixiviation with water. Care must therefore be taken to prevent the temperature from rising high enough to fuse the mass, while at the same time the mass must be heated sufficiently to effect the decomposition.

When the factory at Copenhagen was erected in 1856, this decomposition was effected in iron retorts similar to gas retorts, and set in the same way, and the carbonic acid so liberated (when chalk was used) was conducted to the vats to decompose the aluminate of soda from a previous operation. But this process entailed costly repairs, and much expensive handling. Thomsen then constructed a furnace, which is now successfully used in all the cryolite factories.

It contains two fire-places at opposite ends. The flame from one of them passes *underneath* the hearth, which is formed of large slabs of fire-clay 2 feet square, and supported on square pillars. At this farther end, the flame meets the other, and both pass *over* the hearth, then rise and pass over the arch, and under an evaporating pan, to which they give off the last portions of heat. Finally, they pass into the flue of the chimney.

The mixture is spread on the hearth about 3 inches thick, and is not disturbed for about one hour, at which time it is turned over with a rake, and again left for about three-quarters of an hour, after which it is drawn out, and fresh material is introduced, each charge being about 1000 pounds. The decomposition should be complete in the portion withdrawn, and it forms a granular, loose mass of an ash colour. After the mass has thoroughly cooled, it is lixiviated in an ordinary tank, and the dissolved soda and aluminate of soda is drawn off, and the residue is properly washed. The solution will, on an average, mark 26° to 28° Baumé.

The lye, being a strong solution of soda, carbonate of soda, and aluminate of soda, is treated with carbonic acid, in order to produce carbonate of soda and alumina. The carbonic acid is produced by the combustion of coke, and the products of combustion are drawn off by means of a fan, and passed through stacks filled with coke, down which water is made to trickle; and in this way the carbonic acid is washed and purified. The purified gas is passed from these stacks into a large horizontal cylinder, from 40 to 50 feet long, through the middle of which a shaft with paddles rotates. The cylinder is about half filled with liquor, the paddles are put in motion, and the gas is let on through one end of the cylinder, the unabsorbed gases escaping at the other end. When the solution is saturated with carbonic acid, it is run off into settling-boxes capable of holding one charge. In about from four to six hours the alumina will have separated from the liquor which is drawn off, and the deposited alumina is freed from the adhering soda by washing it with water, and filtering upon proper filters. The clear solutions first drawn off has a density of about 31° Baumé, and the alumina is left in the form of a granular powder. In this way two valuable products are produced from the cryolite—carbonate of soda and alumina. The latter contains but a trace of soda, and is a valuable material for the manufacture of alum and sulphate of alumina.

Considering the nature of the mineral and its impurities, sometimes amounting to 10 or 15 per cent, the carbonate of soda is remarkably pure, its only impurity being one or two per cent of sulphate of soda, the sulphuric acid having been formed from the sulphides that are associated with the cryolite.

The Wet Process.—If cryolite be boiled with six equivalents of lime, the decomposition which takes place is similar to what occurs in the dry process; but if two equivalents of cryolite be boiled wfth fifteen equivalents of lime, the resulting product will be caustic soda, aluminate of lime, and fluoride of calcium.

$$2(Al_2Fl_3+3NaFl)+15CaO=$$
$$=6NaO+2Al_2O_33CaO+12FlCa.$$

In either case the decomposition is effected as follows :—Into a large vertical cylinder, provided with a perpendicular shaft having paddles, and a steam-pipe, ending close to the bottom in a perforated ring, is introduced milk of lime, made from about 15,000 pounds of good lime. The paddles are now put in motion, and the mass is agitated for a little time, and the liquid is then assayed and measured, so as to ascertain the exact amount of lime present. The cryolite is then added in fine powder in the proportion desired to accomplish a given result. Two equivalents of cryolite, and twelve equivalents of lime will produce, as already stated, caustic soda and aluminate of soda. Fifteen equivalents of lime will furnish all the soda as caustic soda, with aluminate of lime. Fluoride of calcium is formed in both cases.

After two or three hours' boiling and agitation, the decomposition is generally completed (testing will indicate the liberation of all the soda), and the contents of the agitator are discharged on a suitable filter. The clean liquor which will form on top of the sediment is drawn off, and the sediment is treated with water as long as the filtered liquid contains soda,

If the first proportion of lime has been used, and aluminate of soda has been formed, the liquid is treated with carbonic acid, as in the dry process, and all the soda is converted into carbonate, and the alumina is deposited as an insoluble residue. If the latter proportion of lime is used, and all the soda be in the solution as caustic soda, all that it is necessary to do is to evaporate the liquid to dryness in pans or kettles. In this manner caustic soda, containing 75 per cent of NaO is manufactured on a very large scale at Natrona.

It is evident that the wet process is decidedly the simpler of the two, there being less handling, and less costly apparatus; but the feeble strength of the lye produced (10° B.) by this process, and consequently the large amount of water to be evaporated, is a great drawback to it. Where alumina is of but little value, and caustic soda is much sought after, the wet process with fifteen equivalents of

lime may be most profitably employed. Where the contrary is the case, the dry process is to be preferred, producing a lye of 26° to 28° B., and requiring but little concentration in order to crystallise.

Besides working out all the details of this process, Julius Thomsen has devised volumetric processes of analysis adapted to different stages of the operation. Weber and Brother, of Copenhagen, not only furnished all the means necessary for conducting his experiments, but established a factory in Copenhagen, and sent vessels to Greenland for the cryolite. Their factory has been the model on which others have been erected.

There are four establishments in Germany, consuming annually about 2000 tons of cryolite; and in 1867 the one in this country was erected by the Pennsylvania Salt Manufacturing Company, having a capacity for working up about 6000 tons of cryolite. This company, in 1867, imported 8000 tons, and sent out to Greenland during the summer nineteen vessels of an average capacity of 450 tons, of which two were lost in the ice. The approach to the coast is considered dangerous on account of the fields of ice, which sometimes form a thick and impenetrable belt of 80 to 100 miles in width. Off the western coast of Greenland the wind scatters the ice, and a good navigator can penetrate the openings without delay. No loss of life has yet occurred in this trade, as the ice affords a refuge for a shipwrecked crew until removed by the Esquimaux, or until they escape by their boats to the settlements. The mines are worked, from May to October, by about one hundred and fifty men. In the salt works of Natrona, Pennsylvania, more than half a million of dollars have been invested, and employment has been given to five hundred men. The alumina manufactured there in connection with the soda is supplied to the largest makers of alum in this country. The various manufactures from cryolite have a market value of over 1,500,000 dollars in gold.

There has been no important improvement on Thomsen's processes, except it be one by G. A. Hagemann, on which a patent has just been obtained.

Bauxite (a mineral containing 80 per cent of alumina) is sometimes mixed with the cryolite to increase the yield of alumina.

Much space has here been devoted to this manufacture of cryolite soda, from the fact that cryolite and its products were conspicuous in the Exposition, and that the value of cryolite and its treatment, as a soda-yielding substance, is but little understood and appreciated by technical chemists generally.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

June 1st, 1871.

Professor Frankland, F.R.S., President, in the Chair.

The following gentlemen were elected Fellows:—H. Adrien, H. Durham, G. Martineau, E. Neison.

Dr. Debus, F.R.S., delivered a lecture on "*Ozone*."

The lecturer began by stating why ozone is considered to be an allotropic modification of oxygen; then discussing whether there are reasons to assume the existence of two allotropic modifications, and concluded with a review of some of the properties of ozone.

Van-Marum, in 1785, seems to have been the first to observe that the passage of electric sparks through oxygen causes this gas to become more energetic in its action on other bodies, and to assume at the same time a peculiar odour. Next Schönbein, in 1840, took this subject up, and instituted a series of experiments on the nature and properties of ozone. He proved that the gas developed at the positive electrode, when a galvanic current is passed through water acidulated with sulphuric acid, possesses the same properties as oxygen which has been subjected to repeated electric discharges, and that the electrolytic oxygen retains those properties when kept over water. He attributed the change undergone by the gas to a peculiar substance to which he gave the name of ozone. It was further found by him that ozone is formed in air, when phosphorus is allowed to oxidise in it. His experiments, however, led to no positive results as regards the nature of ozone; his theory that water and air contained a small quantity of an unknown substance, which in composition was analogous to hydrochloric acid, and which by electricity and other means was decomposed into hydrogen and ozone, was an opinion that could not be supported by facts.

The first experiments establishing the fact that ozone was only an allotropic modification of oxygen were made by Marignac and De la Rive about 1845. Among the more important observations Marignac had made regarding ozone were: That it is best obtained by passing air slowly over phosphorus in a tube 1 metre in length and 6 m.m. in diameter; that it cannot be obtained from dry air, neither from dry and pure oxygen by means of phosphorus; and that ozone contains nothing but oxygen. Schönbein did not accept this conclusion, but asserted (1847) that ozone was a peroxide of hydrogen. His opinion was supported by the experiments of Williamson and Baumert. It appeared, therefore, that there existed two kinds of ozone: the one, obtained by passing electric sparks through oxygen, or by the action of phosphorus on air, being allotropic oxygen; and the other, obtained by the agency of the electric current, being a peroxide of hydrogen.

Against this latter view Andrews maintained that ozone prepared by electrolysis did not differ in its nature from ozone obtained through other methods. He was borne out by Soret, who, in 1863, made some experiments which in many respects were only a repetition of those instituted eighteen years earlier. For drying the gas to be examined, Williamson employed an ordinary chloride of calcium tube. It was not known in 1845 that the last traces of moisture cannot be removed from a gas unless the chloride of calcium tubes are very long.

Thus, according to our present knowledge, ozone is an allotropic modification of oxygen.

The lecturer next proceeded with the consideration whether there was another modification of oxygen, the so-called antozone. This substance is said to be formed either by the action of concentrated sulphuric acid on baric peroxide, or along with ozone when oxygen is submitted to silent electrical discharges, and one of its best characteristics is the property to assume in contact with water a cloudy appearance and to oxidise the water to hydrogen peroxide. But an experiment of Messrs. Nasse and Engler shows that antozone is only peroxide of hydrogen vapour diffused through a very large quantity of air or oxygen.

There exists, therefore, only one allotropic modification of oxygen, viz., ozone.

About the properties of ozone, the lecturer did not think it necessary to say much, as they are so well known, and he directed attention only to one reaction—the decomposition of potassic iodide. This reaction is differently explained by different observers. Schönbein's observation, however (according to which iodine is protected through potassic iodide against the action of potassic hydrate), seems to lead to the best explanation. It may be assumed that on adding potassic hydrate to a solution of iodine these reactions take place:—$KHO \times I_2 = KIO + HI$, and then $KH + OHI = KI + H_2O$. Now, if an excess of potassic iodide be added, the potassic hypo-iodite and the potassic iodide produce again iodine and potassic hydrate, thus:—$KI + OKI = KKO + I_2$, then $KKO + H_2O = 2KHO$, and here the excess of potassic iodide will prevent the potash from attacking the iodide, but allow the latter to act upon starch. From this, now, the action of ozone on a mixture of potassic iodide and starch is easily understood: In the

first place, the potassic iodide being in excess, ozone forms in presence of water potassic hydrate and free iodine. But as by degrees the potassic iodide is decomposed and diminished in quantity, the potash commences to act on the iodine and to form potassic iodate and iodide. The latter is acted upon by the ozone until a certain amount of KHO has accumulated and can with free iodine form iodate and iodide. This process goes on until all the iodide is converted into iodate and the mixture has lost its blue colour.

NOTICES OF BOOKS.

Analytical Tables for Students of Practical Chemistry. By J. Campbell Brown, D.Sc., Professor of Chemistry and Toxicology at the Liverpool Royal Infirmary School of Medicine, and Lecturer of the Liverpool School of Science. London: J. and A. Churchill. Liverpool: A. Holden. 1871.

The author has published this work, the first part of a series, at the request of several teachers as well as students. The tables are not intended to supersede *vivâ voce* instruction, but to be used as notes by which to follow and recall to mind the demonstrations of the teacher. The following are the contents of this useful book:—Table for the Analysis of Gases; Preliminary Examination for Simple Salts and Mixtures; Table for the Separation of Metals into Groups; followed by six tables for the Detection of the Separate Groups; General Examination for Mineral Acids; Special Tests for Mineral Acids; Examination of Organic Compounds; Behaviour of Some Neutral Substances whose Presence may be Suspected by their Taste; Table for the Separation of Organic Bases; Characteristic Tests for the Principal Organic Bases. Although these tables are arranged for the examination of complex substances, it is evident that they may be conveniently employed for the detection of simple substances by omitting all those details which refer to the separation of one substance from another.

CORRESPONDENCE.

AGRICULTURE AND CHEMISTRY.

To the Editor of the Chemical News.

Sir,—Permit me to say a few words on two matters of great agricultural importance which have been recently alluded to in the Chemical News. I refer to the scientific education of farmers' sons and to the manure trade.

In the year 1866 I made an experimental trial of the plan lately proposed by Mr. Marshall Hall. I then commenced a series of forty lectures to a class of farmers' sons at Kingscote, a village about 15 miles from the Agricultural College. The lectures were given under the auspices of the well-known Kingscote Agricultural Association, a thriving and enlightened farmers' club. The class, numbering something like twenty, was quiet, regular in attendance, and passed a fair examination at the close of the course, which extended over two years. The lectures were devoted to inorganic, organic, and agricultural chemistry, the subjects being taken in this order, that a good scientific basis for the agricultural application of chemistry might be secured. At the same time an agricultural bias was given, even to the earlier lectures, by illustrative experiments, made, as far as possible, with substances produced on the farm or useful to the farmer, with plants, soils, and manures. More than this, the pupils of my class were encouraged to repeat the chief experiment shown in the lectures, and several of them actually fitted up little laboratories of their own at home. I regret that I have not been able to repeat so encouraging an experiment, but the great difficulty was in the distances which both students and teacher had to travel in order to reach the place of meeting. My own journeys, during this course, give a total length of 1200 miles, and, owing to the absence of a railway, had to be performed under peculiar difficulties. But I am convinced that the system which I carried out in 1866-7, and which Mr. Marshall Hall has just suggested in his letter to the *Devizes Gazette*, may, when suitably modified in accordance with local circumstances, be carried out with immense advantage to the general farming interest. I ought to add that the scheme above described owed much to the suggestion and help of the Principal of the Agricultural College. I hope, on another occasion, to develope further, a method of scientific instruction for young farmers, and to describe a plan for the creation in England of agricultural experimental stations where chemistry might be brought to bear upon agriculture in several directions.

I would now ask for a little more space, in order to describe briefly one of the most obvious aspects of the manure question. How is it that manufactured manures, such as the different varieties of superphosphate of lime, are often sold to farmers at a price *so much higher* than that at which their crude constituents can be purchased? We know that the process of manufacture is not tedious or troublesome, and that its cost is small. And we know also that many manufacturers of superphosphate supply manure-dealers wholesale with fair qualities of this manure on terms which allow the latter a broad margin of profit, but that the farmer has to pay not the manufacturer's charges only, but those of the dealer as well. Mr. Little has pointed out* one of the ways in which a farmer may get a manure for, say, £4 per ton, which shall be guaranteed to contain 26 per cent of "soluble phosphate," and for which he would have to pay, in the ordinary course, from 5 to 6 guineas, if not more. But it is only fair to point out that two manures may analyse equally well and yet differ in their effectiveness very considerably. This remark applies with especial force to nitrogenous manures in which the nitrogen may exist in various degrees of availability. It is, on the other hand, highly probable that the soluble part of a mineral superphosphate, provided some decomposing carbonaceous matter be present, is as useful as the corresponding constituent of a bone superphosphate. Thus the argument for difference of price which manufacturers urge by reason of their employment of the best materials must not be pushed too far. An instance in point may serve to illustrate my meaning clearly.

In the spring of this year some samples of manure were sent to me for examination. I was subsequently informed that the price charged for them was £8 8s. per ton. Now, on analysis, I could only rate them at about £5 5s. per ton, assuming their several manurial elements to have been derived from the most valuable crude materials. But what was the argument against my valuation which Messrs. A and Co., the well-known manufacturers of this manure, urged with the farmer who had purchased the manure? It was to the following effect:—"Our manures are the final result of long-continued thought and experiment; they have been successfully submitted in the field to the severest trials by numerous farmers; we have sunk thousands of pounds in arriving at our present processes of manufacture, &c., and, moreover, our manures are compounded and mixed in such admirable proportions, and with such exquisite skill, that they must not be judged of by the inadequate test of chemical analysis." Let us pursue this admirable argument a step farther: let us apply it to an animal's food instead of a plant's food. Only mix stones with bread, with a sufficient amount of exquisite skill, and anxious thought, and long experience, and the innutritious stone or other rubbish becomes a most useful material for sustaining life!

Unfortunately, the farmer has not only to deal with mixed manures (which too often justly deserve the epithet "nostrum" which Mr. Little uses in this connection), but

* Chemical News, vol. xxiii., p. 248.

he has to suffer from the gross frauds of purposed adulterations. We shall not easily forget the revelations as to the ochre-grinding machine at a Liverpool guano-works, nor the *exposés* which are continually made in our agricultural journals. But most manures *are* tempting subjects for adulteration. After all they won't crystallise and won't distil, and so come under the great class of "dirts." Here is an analysis of a very dirty dirt indeed, which was analysed in my laboratory last week, having been purchased for a good superphosphate, at a good price, by a deluded agriculturist.

Analysis of Superphosphate; from Mr. ——, *May* 24, 1871.

Water	17·33 per cent.
*Organic matter and combined water	16·31 ,,
†Monocalcic phosphate	1·74 ,,
Tricalcic phosphate	5·02 ,,
Calcium sulphate	12·92 ,,
Common salt	16·82 ,,
Sand	19·00 ,,
Alumina, ferric oxide, and alkaline salts.	10·86 ,,
	100·00

It is evident than in making this exquisite mixture the zeal of the operator had outrun his discretion: he had, evidently, scraped the floor a trifle *too* hard.—I am, &c.,

A. H. Church.

Royal Agricultural College, Cirencester.

MISCELLANEOUS.

Marlborough College.—The Head Master of Marlborough College having determined to introduce natural science as part of the school work, Mr. G. Farrer Rodwell has just been elected science master, and therefore resigns his present appointment at Clifton College, Bristol. Mr. Rodwell enters upon his new duties at the commencement of Michaelmas term.

The Educational Lectures delivered at the London Institution during the past session, by Professor Huxley, Dr. Odling, and Mr. R. A. Proctor, were followed by examinations, and on Wednesday last the prizes and certificates obtained by the students were distributed by Thomas Baring, M.P., F.R.S., the President of the Institution. In Chemistry, the first prize was awarded to Frederick Garrett, and the second prize to A. J. Richardson. In Biology, Miss Dora Harris gained the first prize, while A. Percy Lloyd and Miss F. L. Tolmé obtained second prizes. In Astronomy, the first prize was gained by A. J. Wallis, and second prizes fell to Miss Annie Piper and Edward Garrett.

British Association for the Advancement of Science.—The next meeting of this Association is to be held at Edinburgh, and will commence on Wednesday, the 2nd of August, under the Presidency of Sir William Thomson, M.A., LL.D., D.C.L., F.R.S., &c. The facilities now afforded by the several Railway and Steam-Boat Companies to parties travelling from all parts of Great Britain and the Continent, render it probable that this meeting will be very numerously attended. The local authorities and the representatives of the various scientific societies, as well as all those officially connected with the association, earnestly desire that the members and Associates should receive a cordial welcome, and that everything possible should de done to make the visit agreeable and instructive.

British Association of Gas Managers.—The Eighth Annual General Meeting of the members of this Association will be held on Tuesday, Wednesday, and Thursday, the 13th, 14th, and 15th of June, 1871, at the Royal Dublin Society's Rooms, Kildare Street, Dublin. Edward White, Esq., Vice-President, will occupy the chair. *Order of Proceedings.*—Tuesday, at eleven o'clock, Inaugural Address by the Vice-President, and Reading of Papers and Communications; at seven p.m., Lecture by Dr. Letheby on "The Analysis of Gas." Wednesday, at eleven a.m., Reading of Papers and Communications, Election of Officers for ensuing Year, &c.; at six p.m., the Members and Friends will dine together at the Ancient Concert Rooms, Dublin. Thursday, Excursion to Glendalough, to visit the Lead and Silver Mines, and other places of interest in the neighbourhood. *List of Papers and Communications to be submitted to the Meeting.*—Report of the Committee on the establishment of a Benevolent Fund and the Reduction of Sunday Labour. "Lithology of Gas Coals," by Mr. Jas. Paterson, Warrington; "On the Best Form of Condenser," by Mr. W. J. Warner, South Shields; "On a New Equilibrium Gas-Governor," by Mr. Chas. Hunt, London; "On the Economy of Employing Exhausters," by Mr. Geo. Anderson, London; "On the Apparatus for and Method of Manufacturing Sulphate of Ammonia, suitable for small Works," by Mr. Jas. Eldridge, Richmond; "On the Cause and Prevention of Choked Ascension-Pipes," by Mr. John Somerville; "Carbonising Accounts," by Mr. W. J. Warner, South Shields; "Carbonising Accounts," by Mr. John Niven, Clayton; "Coal Storeage and Spontaneous Combustion of Coals," by Mr. A. H. Wood, Hastings; "Spontaneous Combustion in Coal-Stores," by Mr. A. C. Fraser, Middlesborough; "On Gas Measurement," by Mr. F. W. Hartley, London; "On the Manufacture of Gas from Coal-Tar," by Mr. J. Arnott, Leeds; "Additional Experience of Tar Pavement," by Mr. G. Anderson, London.

* Containing nitrogen equal to 0·372 per cent ammonia.
† Corresponding to 2·31 per cent bone earth made soluble.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 8, 1871.

From the protocol of the meeting of this Society held on the 8th of May last, we learn that it is intended to submit to the approval of an extraordinary meeting of the members a proposal of the Council concerning the publishing in separate numbers of a large number of memoirs sent into the Society, the extent and length of which essays prevents them being published along with the *Berichte*. This number contains the following original memoirs and papers:—

Derivatives of Phosphuretted Hydrogen which Correspond to Ethylamine and Diethylamine.—Dr. A. Hofmann.—The eminent author describes, in this exhaustive essay, another series of researches belonging to those mentioned in former papers on this subject, and treats, in this instance, on—Monoethyl-phosphine—

$$C_2H_7P = \left\{ \begin{matrix} C_2H_5 \\ H \\ H \end{matrix} \right\} P,$$

a very mobile liquid, colourless, insoluble in water, highly refractive specifically lighter than water, boiling at 25°, very strong odour, bitter taste, very readily oxidisable, bleaching cork as chlorine does, acting strongly upon caoutchouc, which becomes transparent and loses its elasticity. When brought into contact with chlorine, bromine, or fuming nitric acid, ethyl-phosphine takes fire; it combines with sulphur and sulphide of carbon, forming liquid compounds therewith; combines, likewise, with concentrated hydrochloric, hydrobromic, and hydriodic acids, forming salts, the first-named of which forms a double salt with chloride of platinum, which, in its outward appearance, is quite as beautiful as crystallised chromic acid. Diethyl-phosphine,

$$C_4H\ P = \left\{ \begin{matrix} C_2H_5 \\ C_2H_5 \\ H \end{matrix} \right\} P$$

also a highly refractive liquid, insoluble in water, boils at 85°, absorbs oxygen with such rapidity as to cause, occasionally, the spontaneous ignition of this liquid. In most of its more prominent properties, this compound corresponds to that just alluded to. The author calls attention to the paralellism of the ammonia and phosphuretted hydrogen derivatives in the following manner:—

Ammonium iodide.		Phosphonium iodide.
H_4NI		H_4PI.
$(C_2H_5)H_3NI$	Primary substitution	$(C_2H_5)H_3PI$.
$(C_2H_5)_2H_2NI$	Secondary substitution	$(C_2H_5)_2H_2PI$.
$(C_2H_5)_3HNI$	Tertiary substitution	$(C_2H_5)_3HPI$.
$(C_2H_5)_4NI$	Quaternary substitution	$(C_2H_5)_4PI$.

Action of Pentachloride of Phosphorus upon Chloral-Ethyl-Alcoholate.—Dr. L. Henry.—The contents of this paper are a reply to a series of observations made by M. O. Jacobsen on the author's investigations on the action of the pentachloride of phosphorus upon the chloral-alcoholate, the product being $CCl_3---CHCl---C_2H_5O$, or the thrice chlorinated chlorethyl-oxyethyliden. This body is, as regards its composition, a four times chlorinated ethyl-ether. It appears that M. Jacobsen, having made a series of experiments on the substitution products of ether, has found a tetrachlor-ether of a different nature; and the greater portion of this paper is devoted to the explanation of some views held by the last-named author, and refuted as untenable by the author named at the heading of this paper.

Contribution to the History of Naphthazarine.—MM. A. A. de Aguiar and A. G. Baeyer.—This paper is a continuation of a former paper on this subject, and contains the following sections:—Trioxy-naphtho-chinon from dinitro-naphthaline, $C_{10}H_3(HO)_3O_2$; trioxy-naphtho-chinon from naphthazarine; mixture of tri- and tetra-oxy-naphtho-chinon, $C_{10}H_3(HO)_3O_2, C_{10}H_2(HO)_4O_2$.

Fatty Oil from Grape Pips.—A. Fitz.—The pips contained in grapes contain from 15 to 18 per cent of an oil, which has been the subject of a series of researches made by the author, and recorded in this essay. The oil, having been saponified, was found to contain palmitic, stearic, and erucic acids, the latter fusing at 34°; formula, $C_{22}H_{42}O_2$. The lead-salt of this acid is difficultly soluble in ether in the cold, but readily so in warm ether; the same obtains with alcohol. The erucic acid exceeds in quantity the two other acids just named, the three being, in the neutral oil, combined with glycerine. When fused with caustic alkali, erucic acid is converted into arachnic acid, $C_{20}H_{40}O_9$, and into acetic acid. The author states that the oil alluded to might be used for culinary purposes; of course it is only obtainable in quantity in wine-growing countries.

New Mode of Formation of Trithionic Acid.—Dr. F. Muck.—The contents of this paper, however interesting in a scientific point of view, do not admit of any useful abstraction, owing to the necessity of reproducing a lengthy series of formulæ.

Some Alloys.—Dr. A. Bauer.—The alloy of platinum and lead, composed, as described by the author in a former paper on this subject, according to the formula PtPb, may be obtained in larger quantities, and free from finely-divided platinum being mixed with it, by fusing in a crucible the platinum, to which a slight excess of lead has been added under a cover of borax glass (previously fused and pulverised borax). The combination of the two metals is attended with a considerable evolution of heat and light. The crucible should be allowed to cool very slowly by being placed in hot ashes, and is, after cooling, broken up, when the alloy is obtained in the shape of a crystalline bismuth-coloured mass, and readily freed from the small excess of lead it contains by being treated with acetic acid. The calculated density of this alloy is 14·89, proving that a contraction takes place when this alloy is formed. *Amalgam of Lead and Mercury.*—Two parts, by weight, of lead are fused, and to the molten metal 1 part, by weight, of mercury added, and the two metals thoroughly mixed; and this mixture, after cooling, treated with acetic acid until no more white-lead and acetate of lead were formed. The unaltered portion of the amalgam, having been carefully analysed, was found to consist, in 100 parts, of—Lead, 40·82; mercury, 59·17; corresponding to the formula Pb_2Hg_3. Sp. gr. of this amalgam, 12·6085. *Lead and Palladium.*—One part, by weight, of each metal was fused together in a crucible, and the alloy afterwards treated with acetic acid as above alluded to for other alloys. The residuary alloy, having been analysed, was found to consist, in 100 parts, of—Palladium, 60·79; lead, 32·20. Formula, Pd_3Pb; sp. gr., 11·25. This alloy is difficultly fusible, and is not capable of occluding hydrogen. *Lead and Gold and Lead and Silver.*—1·5 parts of gold and 11 parts of lead, on the one hand, and 10 parts of silver and 7 parts of lead were fused together. The alloys were, in each case, rolled out to thin sheets, and then treated with acetic acid; but after the removal of the last traces of white-lead, which was formed by the aid of very dilute acid, it was found that the remaining metals were nearly perfectly pure gold and silver, and no alloy of these metals and lead was, under any conditions, obtainable. *Zinc and Copper.*—The author relates that, some thirty years ago, a sample of beautifully-crystallised brass was sent to the Vienna Mint for investigation, since that alloy had been accidentally formed at some brass foundry. The analysis then made by Dr. F. Hoffmann proved this sample to consist, in 100 parts, of—Copper, 74·513; zinc, 25·487; leading to the formula Cu_3Zn. The author observes that, many years ago, Dr. Croockewit published an exhaustive monograph on the subject of metallic alloys, an abstract of which was published in the *Annalen der Chemie und Pharmacie*, vol. lxviii., p. 290.

Analysis of some Minerals met with in the neighbourhood of the Native Warm Baths of Teplitz.—Dr. J. Stingl.—Quartzose porphyry (sp. gr., 2·567) contains, in 100 parts—Silica, 75·74; alumina, 15·07; peroxide of iron, 3·86; magnesia, 0·63; lime, 0·95; potassa, 1·85; soda, 0·51; water, 1·43. Ochry deposit from a source of mineral water, the Neubad, at Schönau, near Teplitz, Bohemia (sp. gr., 2·73), contains, in 100 parts—Peroxide of iron, 2·24; protoxide of manganese, 5·34; lime, 43·77; carbonic acid, 34·84; water, 3·11. This portion, 88·96 per cent of the whole, is soluble in hydrochloric acid; the rest, 11·04 per cent, insoluble therein, consists of—Silica, 9·44; alumina, 1·06; peroxide of iron, 0·42; lime and alkalics, traces. Another similar ochry deposit, but from a different locality, was found to contain, in 100 parts—Matter soluble in hydrochloric acid, 29·86; insoluble therein, 70·14. Analysis of a kind of travertine deposited from a hot spring (sp. gr., 2·698) contains, in 100 parts—Silica, 0·16; carbonic acid, 41·50; lime, 52·50; peroxide of iron, 2·49; alumina, 0·25; soda, 1·75; water, 1·29; and traces of lithia and magnesia.

On Gallein.—Dr. A. Baeyer.—The author describes the mode of preparation, by means of fusing together, at a temperature of 200°, kept up for some hours, a mixture consisting of 1 part of anhydride of phthalic acid with 2 parts of pyrogallic acid. The cooled mass is treated with hot alcohol, filtered, and diluted with water, whereby a precipitate is obtained of the substance just named (gallein), which, as far as the author's researches go, appears to be a new addition to the already large number of artificially-obtainable dyes. It is a brownish-red coloured body, in large lumps exhibiting a greenish yellow metallic hue; insoluble in cold water; difficultly soluble in boiling water, the solution being red-coloured; readily soluble in alcohol—colour of solution, deep red; soluble in aqueous caustic potassa solution, colour beautifully blue; ammoniacal solution, violet-coloured. It appears that gallein has a great similarity to hæmatein; and the author states that the former substance is, therefore, the first artificially obtained belonging to the group of the peculiar constituents of logwood and similar dyestuffs. Not only phthalic acid, but trimesic, pyromellitic, and prehnitic acids, and oil of bitter almonds, yield, when fused, as above stated, with pyrogallic acid, gallein.

Researches on the Constitution of the Benzol Derivatives.—Dr. V. von Richter.—This lengthy memoir contains the following sections:—On dibrombenzol; a new synthesis of acids by the action of cyanide of potassium on some nitro-compounds, according to the formula—

$$C_6H_4\left\{\begin{matrix}Br\\NO_2\end{matrix}\right\}+CNK=C_6H_4\left\{\begin{matrix}Br\\CN\end{matrix}\right\}+KNO_2;$$

isomeric nitrobenzols; conversion of brom-nitrobenzols into brom-benzoic acids; isomeric chloro-nitrobenzols; conversion of chloro-nitrobenzols into chlorobenzoic acids; meta-brombenzoic acid; on the constitution of nitrous ether.

Researches on Valeraldehyde.—Dr. A. Schröder.—This paper treats on:—*Valeraldin.*—Obtained by causing a current of dry ammoniacal gas to be passed over sulpho-valeraldehyde—

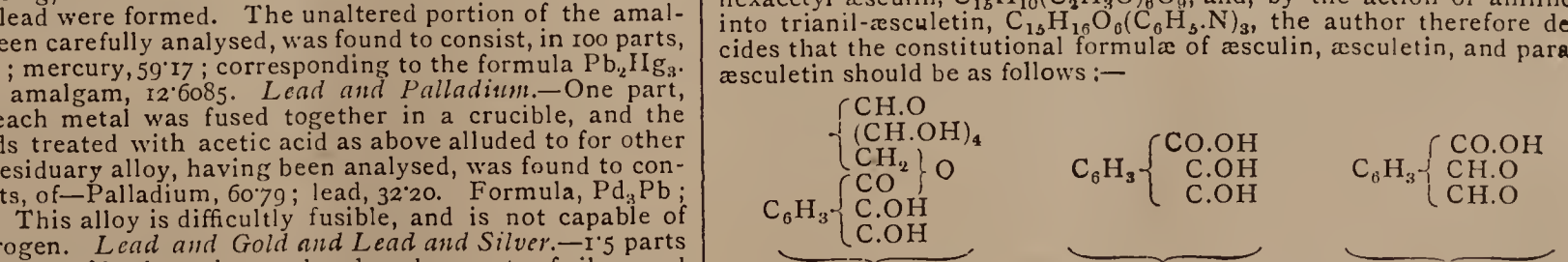
$$3C_5H_{10}S+2NH_3=C_{15}H_{31}NS_2+NH_4,HS.$$

Valeraldin fuses at 41°; its vapour density is 144·50; it is a somewhat indifferent body, insoluble in water, and soluble in alcohol and ether. *Carbo-valeraldin.*—Formed by the reaction of excess of liquid ammonia upon a mixture of sulphide of carbon and valeraldehyde, according to the following formula:—

$$2C_5H_{10}O+2NH_3+CS_2=C_{11}H_{22}N_2S_2+2H_2O.$$

Carbo-valeraldin, $C_{11}H_{22}N_2S_2$, is insoluble in water, soluble in cold alcohol and ether, crystallises from its solution in the last-named body, fuses at 115° in a vacuum, but is dissociated, when heated in contact with air, at between 100° and 160°; vapour density, 60·08. The last portion of this paper treats on the constitution of valeraldehyd-ammonium by the precise determination of its sp. gr. in the state of vapour. The anhydrous valeraldehyd-ammonium, $C_5H_9(NH_4)O$, has at 160° a vapour density of 52·16, as found by experiment: calculation gives 51·5; therefore the vapour density of valeraldehyd-ammonium is normal.

Constitution of Æsculin.—Dr. H. Schiff.—Considering that æsculin, $C_{15}H_{16}O_9$, is converted, by the action of acetanhydride, into hexacetyl-æsculin, $C_{15}H_{10}(C_2H_3O)_6O_9$, and, by the action of aniline, into trianil-æsculetin, $C_{15}H_{16}O_6(C_6H_5.N)_3$, the author therefore decides that the constitutional formulæ of æsculin, æsculetin, and para-æsculetin should be as follows:—



Æsculin. Æsculetin. Para-æsculetin.

Quantitative Estimation of Pigments and Dyes by means of the Spectroscope.—Dr. H. Schiff.—The contents of this valuable paper are not suited for useful abstraction.

On Biuret.—H. Huppert and J. Dogiel.—After referring to Dr. A. W. Hofmann's researches on this subject (see CHEMICAL NEWS, vol. xxiii., p. 227), the authors state that they have succeeded in converting allophanic ether into biuret by fusing that ether in an atmosphere of ammonia gas; but the quantity of biuret obtained is, in the first place, very small, and the product is impure, while, moreover, a large portion of the allophanic ether is volatilised before the reaction, which requires a temperature higher than the point of sublimation, 115°, of the ether, sets in. The reaction, therefore, here alluded to is only of scientific interest, taken in connection with Dr. Hofmann's researches.

Journal für Praktische Chemie, No. 6, 1871.

This number contains the following original memoirs and papers:—

Action of Chloride of Sulphur upon Aniline in the Presence of Sulphide of Carbon.—A. Claus and W. Krall.—The contents of

this very lengthy essay, commenced in No. 5 of this periodical, and recording an exhaustive series of experiments, may be summarised as follows:—By the action of chloride of sulphur upon aniline, the sulphide of carbon does not act as an indifferent solvent, but is an active ingredient; since, in the first place, 4 molecules of aniline, 2 of chloride of sulphur, and 1 of sulphide of carbon react upon each other according to the following formula:—

$$4(C_6H_7N)+2SCl+CS_2=S+2(C_6H_7N.HCl)+C_{13}H_{12}N_2S.$$

If more chloride of sulphur is used than agrees with the formula just quoted, that substance then causes the decomposition of the sulphocarbanilide; but the sulphide of carbon does not, in this instance, come into active play, and the reaction takes place according to the formula—

$$2(C_{13}H_{12}N_2S)+2SCl=C_7H_5NS+C_{19}H_{17}N_3HCl+HCl+3S.$$

Formation of Transparent Cubes of Chloride of Sodium similar to Rock-Salt.—Dr. L. A. Buchner.—This paper is, in a measure, the reproduction of the contents of an essay published by Dr. Mohr (*Poggendorff's Annalen*, vol. cxxxv., p. 667, 1868), "On the Formation of Rock-Salt." The author relates, in confirmation of the facts adduced by Dr. Mohr, a few instances which prove that the formation of cubes of rock-salt, and of other chlorides isomorphous therewith, actually takes place slowly by the spontaneous evaporation of concentrated brines and the gradual diffusion of the thus supersaturated solution downwards (in the vessel wherein that solution is contained), so that the cubes form, and increase gradually in size, at the bottom of the vessel which contains the saline liquor. The author also observed, in one instance, the formation of chloride of ammonium in cubical crystals deposited from the so-called Kœchlin's copper liquor (*Liquor cupri ammoniato-muriatici*), the evaporation having been caused by the not well fitting of the stopper; but this process had taken years to produce the cubical form of crystals of sal-ammoniac alluded to.

Structural Formulæ.—Dr. A. Claus.—A critical review on Professor Kolbe's lengthy esssay on this subject, lately published in this periodical.

Researches on Hydrated Oxide of Iron.—Dr. E. Brescius.—After referring to the various formulæ given by a large number of authors to the hydrates of oxide of iron, the author records a series of experiments made with the view to ascertain the true composition of the hydrated oxides alluded to. It appears that, neither when dried over sulphuric acid, nor, also, when dried at 100°, until in each case no change of weight took place, did it possess the composition corresponding to the formulæ Fe_2O_3HO, Fe_2O_32HO, and Fe_2O_33HO, which are quoted by various authors. The hydrates of the oxide of iron are, further, very hygroscopic, and, in addition thereto, may retain water mechanically. The author also found that the most stable hydrate he could obtain, Fe_2O_32HO, loses a portion of its water at 100°, and another portion only at a red heat. The composition of the hydrate which had been kept for fifteen months under water is not that which agrees with the formula given to it by Dr. Wittstein, $2Fe_2O_33HO$. The author is engaged with further experiments on this subject.

Dissociation Phenomena Exhibited by Aqueous Solutions of Chloride of Iron.—Dr. F. W. Krecke.—The first, and as yet incomplete, portion of an essay on this subject.

Moniteur Scientifique, May 1 and 15, Nos. 345 and 346 (double number), 1871.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Memoir on the History of the Discovery of Genuine (Véritable) Artificial Alizarine.—P. Alfraise.—This very lengthy and exhaustive memoir is divided into the following chapters:—Origin of our researches of this discovery (viz., of artificially-prepared alizarine); history of the scientific labours from the time (1826) of the discovery of alizarine in madder (by MM. Robiquet and Colin, at Paris) up to the time of the synthetical preparation of alizarine; history of the more important pseudo-alizarines discovered before the genuine artificially-prepared alizarine was found. The author comes to the conclusion that the material obtained from anthracen is not at all identical with the alizarine obtained from and present in madder; and his reason for this conclusion is, that when alizarine from madder is oxidised with nitric acid, only phthalic acid is produced, not accompanied by any trace of bitter-tasted nitrated body of the phenic series, while alizapurine (the author prefers so to designate, or, also, call anthrazarine, the substance obtained from anthracen), oxidised in the same manner, yields always a large quantity of a bitter-tasted nitrated acid, only accompanied by a faint trace of phthalic acid. Lastly, the author states that he will shortly publish, in a subsequent number of this periodical, the mode of preparation of an artificially-obtainable alizarine, which is really identical with that of madder.

Although not belonging directly to the domain of physico-chemical science, we quote here the titles of the two following very interesting and valuable memoirs:—

Critical Observations on the Use of Words Derived from the Greek Language in Scientific Nomenclature.—Dr. Egger.

Observations on Words Derived from the Arabic Tongue.—Dr. L. A. Sédillot.—This memoir also contains a brief, but valuable, review of the influence exercised by the Saracens in Southern France, and other portions of Southern Europe, on civilisation and the diffusion scientific as well as industrial knowledge.

Zeitschrift für Chemie von Beilstein, No. 5, 1871.

This number contains the following original papers and memoirs:—

Isomeric Toluylen Diamines.—Drs. F. Beilstein and A. Kuhlberg.—This paper treats on the following substances:—Para-meta-toluylen-diamine, $pmC_7H_6(NH_2)_2$.—This body, fusing at 99° and boiling at 280°, has been fully investigated and described by Dr. A. Hofmann (*Jahres. f. Chem.*, 1861, 512); the sulphate of this base, $C_7H_6(NH_2)_2.H_2SO_4+2H_2O$, is soluble in water, precipitated from its solution in that fluid by alcohol, and crystalline. Para-ortho-toluylen-diamine, $poC_7H_6(NH_2)_2$, is a solid body, fusing at 88·5, boiling at 265°, and readily soluble in cold and hot water; but these solutions are spontaneously decomposed by exposure to air. The sulphate of this base, $C_7H_6(NH_2)_2.H_2SO_4+1\frac{1}{2}H_2O$. Meta-ortho-toluylen-diamine. $moC_7H_6(NH_2)_2$, a solid body, colourless, crystalline, fuses at 80°, boils at 270°, and is very prone to spontaneous decomposition. The sulphate of this base, $C_7H_6(NH_2)_2.H_2SO_4$, is anhydrous, difficultly soluble in cold, but somewhat more readily in hot, water; this solution is precipitated by alcohol.

Some Derivatives of Meta-Toluidine.—E. Wroblevsky.—The author describes a series of compounds obtained by causing various reagents to act upon pure meta-acettoluid, viz.:—Brom-acettoluid, $C_7H_6Br(NH.C_2H_3O)$, a beautifully crystalline body, fusing at 156°, and readily soluble in alcohol and boiling water. Brom-toluidine, $C_7H_6Br.NH_2$, fuses at 57°, boils (but is, also, then completely dissociated) at 240°, soluble in alcohol, but in water very difficultly; this base yields crystalline well-defined salts with hydrochloric, sulphuric, and nitric acids. *o-m*-dibrom-toluol, $C_7H_6BroBrm$, is fluid, even at −20°, and boils at 287°. Dibrom-*m*-toluidine, $C_7H_5BroBrx(NH_2)m$, a crystalline substance, soluble in alcohol, fuses at 50°, and does not combine with acids. *o*-brom-*m*-kressol, $C_7H_6Bro(HO)m$, also a solid body, crystallises in golden yellow-coloured needle-shaped crystals, very difficultly soluble in water, as well as in alcohol, and fuses at 88·5°.

Chemistry of the Superphosphates.—Dr. K. Birnbaum.—This very lengthy paper is the first instalment of a monograph, the chief aim of which is to investigate thoroughly all the causes of the origin of the so-called reduced phosphates in superphosphates. This portion of the memoir contains a very detailed account of the result of the author's researches on the mono-calcium phosphate, its properties, behaviour towards reagents at a higher temperature, and on the di-calcium phosphate.

NOTES AND QUERIES.

Carbolic Acid.—Some information as to the finishing of carbolic acid, or phenic alcohol, into the pure crystals, would be most acceptable to me. I am now making, for another person, about 200 gallons of crude carbolic acid per week, but, as there is no demand for it, I have a chance to work it up.—W. G. Holding.

Nitrobenzol.—(Reply to "Mirbane.")—You will find every information on the subject in the work "On Aniline and its Derivatives," by M. Reimann, Ph.D., &c., revised and edited by W. Crookes, F.R.S., &c. (London: Longmans and Co.), page 6 and following. On the small scale, you can distil nitrobenzol in a glass retort placed in an oil-bath and provided with a Liebig's condenser.

MEETINGS FOR THE WEEK.

Monday, 12th.—Royal Geographical, 8.30.
Tuesday, 13th.—Photographic, 8.
—— Ethnological, 8.
Thursday, 15th.—Chemical, 8. Sir B. C. Brodie, "An Experimental Enquiry as to the Action of Electricity upon Oxygen."
—— Royal, 8.30.
Friday, 16th.—Royal Institution, 9. William Bradford, Esq., Artist, of New York, "On the Esquimaux and Ice of Greenland."

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The Antiseptic System: A Treatise on Carbolic Acid and its Compounds, with an Inquiry into the Germ-Theories of Putrefaction, Fermentation, and Infection, the Theory and Practice of Disinfection, and the Employment of Antiseptics in Practical Medicine and Surgery. By A. E. Sansom, M.D., London, M.R.C.P.

London: Henry Gillman, Boy Court, Ludgate Hill, E.C.

THE CHEMICAL NEWS.

VOL. XXIII. No. 603.

ON THE

ABSORPTION OF SULPHUR BY GOLD, AND ITS EFFECTS IN RETARDING AMALGAMATION.*

By W. SKEY,
Analyst to the Geological Survey of New Zealand.

A PART of the duty assigned to me while recently on the Thames gold-field, was to investigate into the causes of the loss of gold experienced by the proprietors of the several batteries in that district, when working the auriferous material.

From an acquaintance with the various mechanical arrangements adopted there for securing the gold, and from a knowledge of the nature and amount of such loss, I was soon led to infer that much of it could scarcely be referred to any of the causes severally supposed to be operative.

The affinity of gold and its richer alloys for mercury is so great, and their negativeness or unaffectibility to and by the various chemical substances naturally having contact with them so universally and so authoritatively affirmed, that it is generally supposed all we have to do in order to obtain an amalgam of gold and mercury is to present them to each other, with their surfaces free from dust or stain, and to all appearance chemically clean.

But, in experimenting upon a few samples of the Thames gold, I found that though apparently quite free from any such stains, &c., they would not uniformly amalgamate over their entire surfaces; some, indeed, would not amalgamate in the least, though to all appearance bright, clean, and chemically similar to those which did; the action of boiling water upon these did not in the least affect their negativeness to the mercury. This could not, therefore, be owing to the intervention of air mechanically adherent to them.

Observations taken since my return show, besides, that several of the cleanest-looking samples of the Otago gold which have been deposited in the Colonial Museum, manifest a similar negativeness to mercury—even to whole samples of fine alluvial gold, and those surfaces of small nuggets so situated that it was impossible they could have been hand-soiled.

All these specimens were rendered readily amalgamable by cold solutions of cyanide of potassium, nitric acid, chromic acid, or chloride of lime acidified, and, with the exception of the more cupreous of these, they were also rendered amalgamable by roasting them in an open fire for a few minutes.

Obviously, therefore, some foreign substance occupied these non-amalgamable surfaces, excluding the mercury from contact with the auriferous alloy, which substance was disipated or decomposed by heat, and capable of removal by the several reagents specified; the question then arose, What could this substance be? and I began to suspect that sulphur, in some form or other, was this substance, since its deportment under these conditions would certainly be very similar.

I therefore well cleaned the surfaces of several samples of Thames and Otago gold, and one of pure gold, and placed them for a few seconds in sulphuretted hydrogen gas, well washed them afterwards, and dipped them into clean mercury, when, in the place of being instantly whitened over their whole surfaces by the mercury, they absolutely refused to amalgamate with it upon any part, and even seemed to exercise a repulsive force upon it.†

The same effects, or rather *non-effects*, in relation to the mercury, followed when alkaline sulphides were substituted for the sulphuretted hydrogen, also when the samples of gold were kept a short time in boiling water, in contact with sulphur—a platinum crucible being used as a precautionary measure against the introduction of alkaline matters. As in the case of the first series of specimens, these were rendered amalgamable by treatment with cyanide of potassium, nitric acid, chromic acid, or chloride of lime, also by the application of heat to the less cupreous ones.* The effects of heat on those samples containing 6 per cent of copper (and perhaps much less) being to produce such a film of the oxide upon their surfaces that the mercury is still excluded from them, though from a different cause from that above mentioned. These samples also readily amalgamated with sodium amalgam.

Gold thus treated with sulphuretted hydrogen, sulphur, or alkaline sulphides, and thoroughly washed, then put in pure cyanide of potassium, gave a good reaction of sulphur to the nitro-prusside test; and I have also obtained in the same manner, very distinct reactions of sulphur upon several of the native gold samples in the Museum.

These results clearly show that gold, even when in its purest state, is by no means so negative to sulphur and its compounds as is supposed, but that, on the other hand, it absorbs sulphur with great avidity; and they further show, that when this sulphur is absorbed by gold, even when only in very minute quantity, the metal refuses to amalgamate, although there is no visible change induced in its appearance.

Taken in connection with the presence of sulphur (or a compound of it) upon many samples of native gold, and the certainty that one or other of the sulphurising agents specified does frequently occur naturally in metalliferous rocks, it seems highly probable that a large area of the natural surfaces of native gold is sulphurised, and thus rendered, according to the degree of this, non-amalgamable.

If this is so, a very sufficient cause appears for the heavy loss in the precious metal experienced by the mill-owners in working their auriferous reefs by the amalgamation process, since it would only be those portions or grains thus sulphurised, which have chanced to get their surfaces abraded during their extraction or milling, that would be at all likely to adhere to the mercury used.

Whether this absorption is a purely mechanical one, as is assumed when platinum acts upon sulphuretted hydrogen and other gases, or whether it is a truly chemical one, is a matter of some interest to enquire into, particularly in connection with the, as yet, unsolved problem relative to the mode of solution and deposit of native gold; also, in relation to the question of absorption generally.

By what can properly be deduced from the facts above stated, and those which have manifested themselves to an investigation carried on especially to determine this point, it certainly appears that this absorption is the effect of chemical action.

Thus, if this absorption is mechanical, the sulphur must be in one of the two following conditions:—

I. As free sulphur.

II. Combined with hydrogen, as sulphuretted hydrogen.

(1). That it is not as free sulphur was evidenced by the fact that boiling ether or bisulphide of carbon—two liquids having considerable affinities for sulphur—would not remove it from the gold; for, after long contact with these solvents, and an after thorough washing, the gold still refused to amalgamate.

Neither of these liquids had any effect upon clean gold in regard to its behaviour with mercury.

(2). That it is not combined with hydrogen, and thus condensed on the gold surface as sulphuretted hydrogen, appeared from the circumstance that sulphurous acid effected no apparent change on it; the action of this acid

* Read before the Wellington Philosophical Society.

† This was illustrated at the close of the meeting.

* The chlorides of gold and mercury, also nitrate of silver, have been found lately to have the same effect. See paper "On the Reduction of various Metals from their Solutions by Metallic Sulphides, &c," C. N., vol. xxiii., p. 32).

on sulphuretted hydrogen being a rapidly decomposing one, sulphur being liberated in a free state, capable of being detected and identified as in this state, which I could not accomplish.

Not appearing to be in either of these forms, therefore we must assume it to exist in chemical union with the metal as a sulphide of gold, forming a film of true auriferous pyrites, as was first suggested to me by Dr. Hector.

Independent of the proof derived from experiment, it may be expected that sulphur brought into close contact with a metal which we know does form chemical union with it in a wet way, and at common temperatures, would then be in an extremely favourable condition for the exercise of chemical affinities; and the same argument applies for absorption being generally chemical, wherever there are affinities existing at the temperatures we employ between the absorbants and the absorbed substances.

Indeed, so far these experiments and these arguments are deemed conclusive in favour of the absorption of sulphur by gold being chemical, by so much are we compelled to diverge from the received opinion that the absorption of the common gases by platinum is always a mechanical one, and are compelled to distinguish varieties of absorption.

The affinities of sulphur, also oxygen, for platinum, are superior to their affinity for gold; why not therefore suppose sub-sulphides, or sub-oxides, to form, when these substances are respectively absorbed; but the whole question of these minute actions of metallic surfaces requires rigorous investigation, and for this I doubt not the mercury test here used for proving absolute cleanliness of surface will often prove very useful.

Reverting to that which more strictly falls within the scope of this paper, as sulphur has been found upon native gold, I should be quite prepared to find the metals tellurium and selenium also upon the natural surfaces of the gold of Transylvania and the Thames (these substances being isomorphous with sulphur and more fixed), as telluric gold is found in Transylvania, and the character of the two golds assimilates.

In conclusion, I would beg to observe the necessity of fully establishing the character of this absorption of sulphur by gold. If it is a chemical act, as present evidence tends to show, and sulphides of gold and platinum are so easily and so rapidly formed, we cannot doubt but that sulphur plays a very important part in the solution and translation of these metals from rock masses to intersecting quartz reefs, or from deep strata to superficial positions.

In whatever form, however, sulphur is thus absorbed by sold, it is certainly the greatest obstacle to a thorough and complete amalgamation which we have to contend with; no doubt other substances occasionally intervene to prevent or retard this process, such for instance as the oxides of iron and organic matter, but sulphur and its isomorphs must, I think, be the most actively and the most frequently concerned in this refusal of natural gold surfaces to amalgamate. In case further investigation should prove this sulphurisation of the natural surfaces of gold to be general, it will be easy to look for a remedy against its effects; but, as yet, it would be useless to speculate as to what this should be. As stated, there are several ways of removing these films: perhaps chloride of lime in conjunction with muriatic acid would prove serviceable, but, unfortunately, it could only be applied to the "stuff" before amalgamation, and there might be a loss of gold occasioned by solution.

The sodium amalgam of Crookes would be a safe and certain remedy, and easily applied; its effect would be in relation to this sulphide, to decompose it, and so expose the encrusted gold to the action of the mercury.

Doubtless the great benefit which has often attended the use of this amalgam has been principally due to the exercise of this kind of action.*

* For further discussion on this subject, see *N. Z. Geol. Survey Reports*, 1870, p. 70.

SUGAR FROM MELONS AS COMPARED WITH SUGAR FROM BEETS.

Mr. W. Wadsworth, in a letter to the *Sacramento Union*, maintains that sugar can be made more profitably from melons than from beets. He says:—

The sugar from cane, maple, beets, parsnips, the sweet-gourd, and all the varieties of melons, when manufactured perfectly pure, are chemically identical. In Hungary and Italy there are numerous large establishments for the manufacture of melon sugars. The cost of melon sugar as compared with beet sugar is in favour of the melon. Every German or French authority on the culture of beets for sugar, admits the necessity of two, and recommends three, deep and thorough ploughings of the land to properly fit it for the culture of beets. With melons it is quite otherwise. To secure the largest yield and best beets, the seed should be planted in rows two feet apart, and from eight to ten inches apart in the row. For beets, all the land—for illustration say fifty feet in width—must be ploughed at least twice. For melons, only four beds, twelve feet apart and each only four feet wide, or sixteen feet in width of ploughed land, against fifty for beets, will need ploughing.

The great expense of beet culture is in the hand-hoeing and weeding of every row, and in most lands as many as three of these weedings are required in a season, before the leaves are large and spreading enough to keep down the weeds. The difference between the weeding of four rows of melons and twenty-five rows of beets is very considerable; whilst the exhaustion of the fertility of the soil is in the same proportion. With both crops the land between the rows is kept free from weeds with the horse-hoe or cultivator, at the same expense. Young melon plants are not as tender and delicate for the first eight days as beets. It is evident, therefore, that the expense of culture is largely in favour of melons, it being less than one-third the cost of beets per acre.

In gathering the two crops the difference is again in favour of melons, for they only have to be picked from the vine and thrown into carts; then, without washing or any other process, are ready for the mill. Beets must be first pulled, thrown into heaps to protect them from the sun, then each beet must be handled in having its crown of leaves and rootlets cut off, and then, before it is ready for the rasp or cutter, must be washed thoroughly clean.

The gathering and handling of melons is an agreeable and cleanly operation compared with that of beets. Large quantities of melons in certain localities can be sold for direct consumption in the early part of the season, or whenever worth more in that way than for sugar, spirits, or vinegar; it is not so with beets. Sugar making can commence a full month earlier from melons than from beets, and with winter water melons, as in Hungary, continued as late as with beets. Melons yield their seed every year with no extra expense for cultivation. Beets require a second year, with land, and careful culture and gathering of the seed. Melon seeds will yield sixteen per cent of their weight of excellent table oil. Beet seeds, beyond what are needed for seed, are of no value. The oil from the surplus seeds of melon sugaries in Hungary pays one-half the cost of cultivating the entire melon crop. The yield of melons per acre, in favourable soils, is equal to that of beets. The yield of sugar is as seven per cent from melons to eight per cent from beets; but the cost of manufacture is decidedly in favour of melons; they require less time, less bone-black, less machinery, less power, and less fuel, because no water is added, which cannot be said of beet juice by the ordinary process of extraction.

The natural purity of the juice of melons is so superior to that of beets, that whilst the melons furnish an agreeable "food and drink," and a delicious sweet, the juice of

beets is so acrid and herbaceous to be wholly unpalatable. The defecation and refining processes for melon juice and sugar are therefore attended with far less trouble and cost. That part of the beet which in many instances grows above ground, exposed to the sun, is of little or no value for sugar, whilst the hotter the sun and the drier the air, the better and sweeter the melon. The larger the sweeter generally, whilst the reverse is true of beets.

Beet juice and pulp exposed to the air, will turn black in fifteen minutes, and fermentation commences immediately from the rasp. Melon juice and pulp will not blacken at all, and will not begin to ferment in the open air before the third day from the melon. Beets are remarkable for their power of extracting alkaline and saline substances from the soil, which injures their value for sugar. Melons are equally remarkable for letting these salts entirely alone in the soil.

No centrifugals or presses are required to separate the juice from the pulp, as with beets; but all except the rinds and seeds go into the defecating kettles together. Cloth filters, concentrators, and a vacuum pan are as necessary as for beets. The buildings are less costly, because requiring less strength to hold in position the centrifugals and other necessary machinery for beet sugaries. The chemical processes of melon sugar making do not differ materially from those for the making of beet sugar, except in their simplicity. Spirits in large quantities can be extracted from the fermented juice of melons and the refuse of the sugarie, and "pure cider vinegar" is made therefrom in ten hours that cannot be distinguished from the genuine article. The melon rinds, with dry grass or straw, make an excellent food for milch cows.

USES OF INFUSORIAL SILICA.

INFUSORIAL silica is now employed as a substitute for heavy spar, in the manufacture of certain kinds of rubber goods. As india-rubber will float on water, it is desirable to have something to add to it that is lighter than heavy spar, and the silica seems admirably adapted to take the place of the heavy earth. By mixing three to six parts of infusorial silica to one part of freshly burnt lime, and stamping the whole, after slightly moistening it, into a suitable mould, artificial stone of any desired form can be made. Such stones become extremely hard, are impervious to water, are finer grained than cements or *béton*, can be used for gas or water pipes, and will take any colour. As there are large deposits of diatomaceous earth in various parts of America, this application for artificial stone and cement is well worthy of consideration.

By combining infusorial earth with native magnesite and chloride of magnesium, a cement is produced which is known in Germany under the name of albolith cement. The chloride of magnesium, obtained as an incidental product in salt manufacture, is very cheap in some parts of Germany, and the occurrence of large deposits of magnesite renders this variety of cement available in Europe for many purposes. A fine glaze for earthenware is obtained by fusing infusorial earth with crude borate of lime, or boronatrocalcite. A variety of porcelain can be made by fusing infusorial silica with the borate of magnesia of the Stassfurt mines. This kind of porcelain can be cast, pressed, and, if sufficiently thin, can be blown as easily as glass. It is capable of extensive use in the arts.

Infusorial silica is the best material for absorbing nitroglycerine, in the manufacture of dynamite, and is used for that purpose. Ordinary sand is not sufficiently porous. The ready solubility of this form of silica in soda suggests its application in the manufacture of liquid quartz. It is not a little singular that an earth which has long afforded test objects for microscopists, and has been employed as a polishing powder, should become an article of so much importance in the arts.—*Scientific American.*

ON THE

ESTIMATION OF MANGANESE IN SPIEGELEISEN AND FERRO-MANGANESE.

By THOMAS ROWAN, F.C.S.

THE following process for the determination of manganese is, I need hardly say, not novel; but, having had occasion to make many estimations of manganese in spiegeleisen and ferro-manganese, I can recommend it as superior for speed and accuracy, where manganese in quantity has to be determined, to the other processes commonly resorted to.

A convenient quantity of the sample, say 20 grains, in as fine a state of division as possible, is digested in a long-necked flask, with about 1½ ozs. of hydrochloric acid, until complete solution is effected. The solution is then oxidised with chlorate of potash added from time to time, and finally boiled until traces of chlorine can no longer be detected.

The solution is now saturated by liquid carbonate of soda added in small successive quantities, the flask being well agitated after each addition of the alkali. On nearing the point of saturation, each addition of the carbonate of soda occasions the separation of small quantities of carbonate of iron and manganese, which disappear on the flask being shaken. When this is observed the alkali must be added with extreme caution, but the deep blood-red colour now acquired by the solution will, after a little practice, be a sufficient indication to the operator that the desired point of saturation or neutralisation has been attained. Or, if it is preferred, the smallest excess of carbonate of soda may be added, and the permanent precipitate which forms re-dissolved by the cautious addition of hydrochloric acid, introduced drop by drop.

Before treating with carbonate of soda, the solution should be evaporated to as small a bulk as possible, the presence of much free acid causing the expenditure of an unnecessary amount of alkali, and the violent effervescence caused by the escaping carbonic acid often entails loss by the projections of portions of the solution from the flask.

From 6 to 8 ozs. of water are now added, and the iron precipitated as the basic acetate of iron, by the addition of a concentrated solution of acetate of soda. If the solution has previously been successfully neutralised with carbonate of soda, the ferric acetate will at once make its appearance; but if that point has not been reached, the iron will only precipitate after boiling, and then not completely, and the precipitate will be found to be so gelatinous as to cause much trouble by clogging the filter.

After the addition of the acetate of soda, the flask and contents are briskly boiled for about twenty minutes, and then allowed to repose for a few minutes, that the acetate of iron may settle to the bottom of the flask, when the solution is carefully decanted from it on a filter. More water is then added to the flask, with a few drops of acetate of soda, boiled for five or six minutes, and again decanted from the precipitate. This is repeated a third time. The acetate of iron can then be thrown on the filter and washed with boiling water. The filtrate, with washings, is removed to a beaker and brought to a temperature of about 100° F., and a stream of chlorine gas passed through until a faint smell of that gas can be detected from the liquid. This is ascertained by stopping, from time to time, the passage of the chlorine, blowing from the surface of the liquid in the beaker the gas that may have lodged there, and then testing by the smell. If chlorine can be detected the addition of the gas is finally discontinued. The beaker is now carefully covered and set aside in a moderately warm place for six hours. The precipitated binoxide of manganese is removed by filtration, and chlorine is again passed through the filtrate to ascertain whether all the manganese has

been removed. When the precipitated manganese settles to the bottom of the beaker, if the liquid above is purple-coloured it indicates that an excess of chlorine has been used, and that permanganic acid has been formed. This is easily remedied, as the permanganic acid is at once reduced to the binoxide by the presence of any organic matter. A few drops of alcohol may be added, and the precipitated binoxide of manganese filtered off.

The precipitated binoxide of manganese is re-dissolved by pouring hot dilute hydrochloric acid on the filter. The manganese is re-precipitated as carbonate by a solution of carbonate of soda, and boiled well to expel all carbonic acid, the carbonate of manganese being slightly soluble in carbonic acid. The carbonate of manganese is then collected on a filter, washed well with boiling water, dried, ignited, and weighed as the red oxide.

The binoxide of manganese has such a tendency to appropriate alkali that it cannot be directly ignited to the red oxide. When this is done without re-dissolving and re-precipitating as carbonate, I have found that one part of the ignited precipitate contains 0·0842 of alkali, which must be deducted from the weight before calculating the percentage of metallic manganese.

If the weight of the ignited red oxide thus obtained be 3 grains, the following calculation is made:—

Wt. of red oxide ppt.

1 : 0·0842 :: 3 : 2·7474

In a rigorous analysis; however, it will be found more satisfactory to proceed as directed above, namely, re-dissolve the binoxide of manganese first obtained, and re-precipitate as carbonate before proceeding to ignite and weigh. The amount of metallic manganese is readily ascertained from the weight of the ignited red oxide, 100 parts of which contain 72·05 parts of metallic manganese.—*Engineering*.

A NEW METHOD FOR THE

SEPARATION OF GOLD AND SILVER ON THE LARGE SCALE.

By Dr. F. GUTZKOW.

The usual method employed on the large scale, for a considerable number of years, for separating gold from silver, copper, and other metals, consists in treating the alloy with concentrated sulphuric acid at a high temperature, and precipitating the silver from its dilute sulphuric acid solution by means of metallic copper, the desilverised liquor being used for the preparation of sulphate of copper. This, however, is in many respects deficient; for, in the first place, there is a large bulk of water required to dissolve the rather difficultly soluble sulphate of silver, and this large bulk of liquid requires, of course, large-sized vessels to hold it. Of far more importance is, secondly, the fact that it involves the production of a large quantity of sulphate of copper, a salt now obtained as a by-product in such large quantities, in various metallurgical and other operations, that, owing to its always yet rather limited consumption, this salt is very difficultly saleable. The manufacturer has, in most instances, to content himself with accepting a price for this article which often only barely covers the expense of the sheet copper employed and the copper present in the alloy which has been operated upon. Taking these matters duly into consideration, the author, who, while residing at San Francisco, in California, was for a series of years the manager of the extensive silver refining works belonging to the San Francisco Assaying and Refining Company, considered it to be of the highest importance to make a radical change in the method of silver refining just alluded to, and to limit the manufacture of blue vitriol to the comparatively small quantity of the copper contained in the crude unrefined ingots. It was found unsuitable to substitute sheet-iron for the sheet-copper in the precipitation owing to the fact that, by the employment of iron, copper is, of course, again precipitated along with the silver; and the separation of the copper from the silver does not admit of any other method readily executed on the large scale than a repetition of the same process again. The author found, however, that the reducing agency of sulphate of iron (green vitriol) can be applied successfully to the solution of sulphate of silver. The process about to be here described has been employed for a series of years in the works above-named, and by this process several thousand hundredweights of fine silver have been produced. It should be observed at the outset that it is not readily possible, for reasons which are not here further alluded to, to work on the large scale with sulphate of iron upon the solution of sulphate of silver in water. On the contrary, it is required to prepare, first, crystallised sulphate of silver, free from impurities, inclusive of metallic gold in finely-divided state, sulphate of lead, and other substances, insoluble in a solution of green vitriol. The pure crystallised sulphate of silver is next to be acted upon by a hot and concentrated solution of green vitriol. The very hot, turbid, thickish fluid obtained by the action of boiling concentrated sulphuric acid upon the silver alloy under operation is poured into a large-sized cast-iron cauldron, containing dilute sulphuric acid at 58° Baumé = 1·617 sp. gr., and previously heated to 110°; a small quantity of water is next added; and after the liquor (having been left at rest for a few minutes) has become clear, the solution is syphoned over into another similar cauldron, so placed and arranged as to admit of being thoroughly cooled by means of cold water externally applied. For every hundredweight of silver refined, 10 cubic feet of the dilute acid (sp. gr. 1·617) are taken. The addition of water just alluded to is intended to reduce the very concentrated acid silver solution to the same density, and the quantity of water to be added may be therefore inferred from this explanation. The addition of water is intended, however, to effect something else yet. Precipitates of sulphate of lead and sulphate of silver are formed, and the latter does not become quite permanent until, first, all the lead which was in solution is precipitated; and, moreover, these heavy precipitates greatly aid the throwing down of all substances which render the liquid turbid, and especially the gold. By the means just described, a clear liquid, quite free from any lead and gold, is far more rapidly and completely obtained than by the method usually applied—viz., the pouring of the very concentrated sulphuric acid silver solution into water.

The liquid, having been cooled in the manner just alluded to, and reduced to a temperature of from 30° to 40°, is, by means of a pump, transferred again to the upper cauldron, there to be used again as acid at 38° B. At the bottom of the cauldron in which the cooling took place the sulphate of silver will be found deposited, forming a hard, yellow-coloured, crystalline crust about 2 inches in thickness. This crystalline mass is tolerably free from adhering acid; but at the deepest part of the vessel will always be found some strongly acid mother liquor, and this acid is to be again used to dissolve a fresh quantity of silver. The crystalline mass, consisting of sulphate of silver, is removed from the cauldron by means of iron shovels, and placed on the perforated false bottom of a wooden box lined inside with lead, and placed on wheels, so as to be capable of being moved from one place to another; between the false and real bottom, a tap is placed for running off liquid. Along with the crystals, and adhering thereto, is a red powder, chiefly consisting of sulphate of copper. The next step is to run through, or, more correctly, pour over, the crystalline mass a very hot and very concentrated aqueous solution of protosulphate of iron (green vitriol). The salt of copper is first dissolved, and therefore that liquid is run off separately, afterwards to be used for the preparation of sulphate of copper. As soon as the solution which runs off begins to exhibit

the pure brown colour due to sulphate of peroxide of iron, the solution is caused to run into a large, very shallow vessel, in which, on cooling, the largest portion of the silver salt is decomposed, and some metallic silver is deposited in a spongy state, which substance is collected and placed on a large filter. The greater portion of the crystalline mass of sulphate of silver which had been placed in the box is, however, converted slowly on into a dense coherent mass of metallic silver, and the reduction may be considered complete as soon as the vitriol solution which runs off has assumed the green colour it originally possessed. The metallic silver is next washed with pure hot water, then pressed in a hydraulic press, and lastly melted.

The iron solution which has collected in the large-sized shallow vessel just spoken of, after having become sufficiently cool, is poured or run into a lead-lined tank wherein some scraps of old sheet-iron are placed, and is thus again re-converted into sulphate of protoxide of iron, to be used at a subsequent operation. The small quantity of silver and copper which are separated in the metallic state by this last-mentioned operation is collected from time to time, and put along with the crystals of sulphate of silver contained in the vessel, wherein they are to be exposed to the action of the sulphate of iron solution. The copper is almost immediately converted into sulphate of copper on coming into contact with sulphate of silver. On the large scale, for every hundredweight of silver reduced from the sulphate 20 cubic feet of green vitriol solution are required.—*Berichte der Deutschen Chemischen Gesellschaft zu Berlin.*

NOTE ON THE ANALYSIS OF MOLASSES.*

By M. A. VIVIEN,
Director of the Laboratory of the Association of Sugar Manufacturers, Paris.

M. DUBRUNFAUT published, in the number of the *Sucrerie Indigène* for June 20, 1870, p. 75, an article in which he tries to prove again that the methods pursued in the analysis of molasses, and considered hitherto exact, are false; from which it follows that all the results given by analytical chemists up to this time, and even until quite recently by himself, are and should be stigmatised as errors. If M. Dubrunfaut has proved what he advances, nothing is to be done but to suppress all chemists, for the manufacturers of sugar can no longer depend upon their dicta. But fortunately one cannot help seeing in this an exaggeration, which it is easy to reduce to the proportions of reality.

How has M. Dubrunfaut discovered that the method usually followed was false? In trying to determine the sugar in a molasses comparatively by a method of his own invention, and by that at present in use, he perceived that these two methods did not give identical results, and he concluded that the one in use was false and that the other was exact.

Still, whatever may be the authority attached to the name of M. Dubrunfaut, to his long experience in the matter, and whatever, on the other hand, may be my personal disqualifications, I do not hesitate to dispute his affirmation, and to call the attention of chemists to a doubtful point, the explanation of which is becoming so much the more urgent as the sale of molasses by analysis tends to become a general custom.

What is the method which M. Dubrunfaut recommends? a faint sketch of it is seen in the number of the *Sucrerie Indigène* for Nov. 5, 1869, p. 201, but the explanation is insufficient to be comprehended by every one; it is also necessary to trace the chemical reactions more minutely. If I understand aright, the method which M. Dubrunfaut recommends is nothing other than the method taught by M. Jacquelin in the laboratory of the Ecole Centrale of arts and manufactures. The following is the method as taken from the laboratory book of the school:—

Determination of Glucose by the Alkaline Method.—In presence of alkalies and under the influence of a temperature of 100° C., glucose has the property of uniting with alkalies, and consequently neutralising their effects; if then we boil a solution of glucose sugar with an excess of soda, it is only necessary to determine, by alkalimetry, the amount of soda which has not been neutralised, to find the quantity of glucose. Crystallisable sugar under the same conditions undergoes no change, and consequently does not saturate the alkali.

Composition of the Solutions—

No. 1. Soda solution (alkaline); 75 grammes of caustic soda are dissolved in distilled water; the solution is diluted up to one litre.

No. 2. Acid solution; 100 grms. of monohydrated sulphuric acid are diluted with distilled water up to the volume of one litre.

These two solutions neutralise each other volume for volume, and the titration is conducted in the following manner:—

Titration of the Solution.—Two grammes of pure dry sugar, to which is added 0·4 c.c. of fuming hydrochloric acid and a small amount of water, are kept for a space of 15 minutes at a temperature of 100° C., easily attained and maintained by means of a water bath. The solution of inverted sugar is diluted to 200 c.c., and consequently contains 1 gramme of sugar in the 100 c.c.; 50 c.c. are taken, which are carefully neutralised with an alkaline solution, say of soda, then we add 10 c.c. of the test soda solution, and the temperature maintained at 100° C. Under these circumstances the glucose neutralises a certain quantity of soda; nothing more remains but to add the sulphuric acid solution in small portions at a time from a graduated burette, and to test, from time to time, by red litmus paper and a trace of the inverted solution, if the point of saturation is reached. The paper cannot be placed in the solution on account of the brownish chestnut colour, caused by the heat, masking every change of colour. To effect the saturation 7·95 c.c. of the sulphuric solution has been used, and 10 c·c. of the soda solution require 10 c.c. of the sulphuric solution; therefore the inverted sugar has saturated the same quantity of alkali that $10 - 7{\cdot}95 = 2{\cdot}05$ c.c. of sulphuric solution would take up.

Therefore, 1 gramme of sugar made uncrystallisable equals in acidity 0·41 gramme of HO,SO_3. The solutions being titrated, the test of a molasses is as follows:—50 grammes of molasses, average sample, are dissolved in water and diluted to one litre; 50 c.c. of this solution are taken, which corresponds to 2·5 grammes of molasses; this solution should then be ascertained to be perfectly neutral, and then 10 c.c. of the soda solution is added. It is then kept at 100° C. for 15 minutes, and the akalimetric test made. Suppose 8·1 c.c. of sulphuric solution were used, $10 - 8{\cdot}1 = 1{\cdot}9$ c.c.; therefore, 2·5 grammes of molasses contains a quantity of glucose equal to 1·9 c.c. of sulphuric solution, or, 100 grammes of molasses corresponds to 76 c.c., or we have $4{\cdot}1 : 1 = 76 : x$; from which $x = 18{\cdot}536$ per cent. Therefore, 100 kilos. of molasses contain in glucose what is equal to 18·536 kilos. of glucose sugar. To determine the quantity of sugar in an uncrystallisable state, 50 c.c. of the original molasses solution are again taken, to which is added 1·5 grammes pure HCl, and the solution kept for 15 minutes at 100° C. This is then carefully neutralised, 10 c.c. of the soda solution added, the temperature maintained as before at 100° C. for 15 minutes, and the alkalimetrical determination made. Suppose it was necessary to use 5·8 c.c. of sulphuric solution, we then have $4{\cdot}1 : 1 = 168 : x$; from which $x = 40{\cdot}975$ per cent.

* Translated from the *Sucrerie Indigène* by E. Waller, E.M.

Then 100 kilogrammes of molasses contain—

40·975 sugar in all;
18·536 glucose.

22·430 in crystallisable sugar.

This method was taught some years since at the Ecole Centrale, and it appears to be the one which M. Dubrunfaut recommends and practices. Not only is it false, but it is very difficult of execution. We cannot judge without great difficulty of the exact point of saturation. M. Dubrunfaut, after having praised the saccharometric method, arrives at the conclusion that it is false, the results being varied by the presence of divers organic substances, and noticeably by that of asparagine. M. Dubrunfaut stopped there, and, in spite of his knowledge and perspicacity, he did not perceive that this asparagine falsifies the results if the alkaline method is used, even when taking sucrate of lime. This is what we are about to attempt to show.

A molasses, of whatever origin, is a solution of sugar, of mineral and organic substances. Certain organic substances can, under the influence of heat and prolonged boiling, decompose into multiple products, of which some are acids, and these acids by neutralising the alkali used will be reckoned as glucose. I will only take the case of asparagine, and this example will suffice to prove the fact which I advance. Under the influence of such an alkali as lime, and a temperature of 100°, the asparagine, a neutral body, changes to aspartic acid, with disengagement of ammonia. This reaction takes place during defecation in sugar refining, and is explained by the following formula :—

$$CaO + 2HO + \underbrace{C_8H_8N_2O_6}_{\text{Asparagine.}} = \underbrace{C_8H_7NO_8,CaO}_{\text{Aspartate of lime.}} + NH_3.$$

One equivalent of lime is therefore neutralised by an equivalent of aspartic acid, and the method advised by M. Dubrunfaut may indicate the presence of glucose in a solution which does not contain it at all. Is there any reason, then, to wonder that analytical chemists are unwilling to follow this mode of operation?

Since the question of the molasses analysis is on foot, I take the opportunity of describing briefly the method of analysis which I practise in order to submit it to discussion.

Every analysis should be made in duplicate, if one wishes to be certain of the figures which he gives; also the analysis of molasses should be made by two distinct methods, care being taken to work upon a sample which is very homogeneous. I operate in the following manner :—

Molasses Analysis.

(1). *Formation of the Average Sample.*—All of the sample to be analysed should be passed through a sieve sufficiently fine to remove from it all foreign bodies, such as black grains, splinters of wood, straw, &c. It often happens that very dense molasses allows sugar to crystallise at the bottom of the bottles; taken hot from the filters it deposits the sugar in cooling, and if care is not taken to re-dissolve the sugar in the average sample its strength will appear too small. The molasses of M. Boulard, sugar manufacturer at Palinges (Saone et Loire), which gave occasion for the article of M. Dubrunfaut, was precisely in this condition, and if all the experimenters have not taken the precaution which I have just mentioned, they have naturally given figures too small.

First Analysis. Crystallisable Sugar.—16·35 grms. of the average sample of molasses are diluted in about 100 c.c. of water and clarified with—

(1). 10 c.c. of a solution of tannin.[1]

(2). 30 c.c. of a solution of basic acetate of lead at 30° B.

The tannin precipitates the nitrogenous albumenoid substances, of which M. Dubrunfaut could not get rid; it precipitates also the salts of lime, and, moreover, the lime of sucrate of lime, the rotatory power of which falsifies the indications of the saccharometer. The basic acetate of lead is employed in such quantity as to precipitate all the tannin, and to unite all the colouring matter which has not been eliminated by the tannin.

This clarification being terminated, the whole solution is diluted up to 200 c.c. It is then filtered and tested with the saccharometer. The number observed multiplied by 2 gives the amount of crystallisable sugar, excepting a correction which must be applied if there is too much glucose, in 100 kilos. of molasses.

Second Analysis. Total Sugar and Glucose Sugar.—50 grms. of the average sample of molasses are dissolved in water, rendered neutral by very alkaline tartrate of soda, and diluted to 500 c.c. Thus is obtained a first solution (A). This solution, filtered if necessary, is placed in a graduated burette, and run, drop by drop, into 10 c.c. of boiling copper solution of Trommer, Fehling, Barreswil, or others, to complete disappearance of the blue colouration (practice of M. Violette). This copper solution is sufficiently dilute to correspond only to about 0·005 gr. of sugar. 100 c.c. of the solution A is taken, acidulated with 20 to 25 c.c. of tartaric acid (25 per cent solution), the whole maintained at a temperature of 100° for 15 or 20 minutes and neutralised by soda. The solution thus inverted and neutralised is diluted to 1 litre. This solution B is treated in the same manner as has just before been done for glucose, but with a copper solution about ten times as strong. Suppose we have found 20 c.c. solution A to 10 c.c. of copper solution corresponding to 0·00575 of sugar; we have the proportion :—

20·6 : 0·00575 = 1,000 : x; from which x = 0·028 glucose,

and 10·2 of solution B to 10 c.c. of the copper solution corresponding to 0·0575 gr. of sugar, we have the proportion :—

10·2 : 0·0575 = 10,000 : x; from which x = 56·37 total sugar.

Finally, supposing that we have found by the saccharometer an observation oscillating between 27·75° and 28° which corresponds to 55·5 or 56, we take the mean and the analysis is interpreted as follows :—

	Per cent.
Crystallisable sugar	55·50
Glucose	2·28
Total sugar	55·78

The inversion by tartaric acid and the neutralisation by soda have the advantage of giving solutions which permit a thorough reduction of the copper solution without allowing at the end of the operation the colouring of the solution with a brown tint which might mask the indications of the final reaction, and prevent the formation of an annular precipitate, while all the sugar is precipitated. In operating thus, results are obtained agreeing within a degree at most; there is no need to seek for greater exactitude in a technical analysis of molasses. Moreover, for a difference of a degree the difference in selling value is much less than that which exists between two analyses of sugar made approximately to ½ per cent—conditions accepted in commercial transactions in sugar. As every one can determine for himself the exactitude of these methods of analysis, I will finish this article by requesting M. Dubrunfaut to examine them and say for himself if he finds them exact; I claim at the same time, in return, his obligation to explain in all its details the method which he considers exact, for we, whom he accuses of being deaf to his advice, know nothing of it.

The 15th of March, 1870, he delivered to M. Monfourny a certificate of analysis of molasses thus expressed :—

The density of the molasses is 41° Baumé.

It contains in crystallisable sugar ..	37·600	per ct.
In actual glucose titrated by sucrate of lime	4·016	,,
Total in sugar	41·616	
Sulphatic ash	12·384	,,
In which lime forms a part to extent of	0·353	,,

The molasses is neutral to litmus paper.

The glucose determined by copper solution gives 7·692 per cent, which constitutes an error of 3·676 per cent more of fermentable sugar if this reagent, employed by many analysts, is used.

Signed, Dubrunfaut.

From this, it results that it is erroneous to use the copper method, but *now*, three months after, M. Dubrunfaut does not appear to be of the same mind any longer, since, he says:—"The percentage of sugar may be taken by the copper method, which is very sensitive. . . . We must reject the saccharometer, which gives figures radically inexact."*

What conclusion must we come to? It would be well for the light to be established, and that M. Dubrunfaut, whose ability every one recognises, should assist us to do so.

Finally, I will express my regret that M. Dubrunfaut did not quote in full the certificates of analysis which he pretends are inaccurate. I have in my hands the certificate of MM. Laire and Galot. I only speak of this one, for I have not seen that of M. Zalinski, but I am inclined to believe that M. Zalinski diluted the molasses in such a way as to bring it up to 40° B.† which explains why he found so small a percentage.

	Certificates as quoted by M. Dubrunfaut.		Real certificates.	
	MM. Lair and Galot.	M. Vivien.	MM. Lair and Galot.	M. Vivien.
Degrees Baumé,			41·5	42·05
Weight of 1 hectolitre,				
Crystallisable sugar, per cent	53·35	55·50	53·35	55·50
Glucose sugar, ,,	0·00	0·28	0·00	0·28
Total sugar, ,,	53·35	55·78	53·35	55·78
Ash, ,,	9·76	10·385	8·57	10·336
Which corresponds to a saline coefficient of ,,	5·46	3·34	6·22	5·34

Finally, in taking M. Dubrunfaut's figures as the only exact ones, I do not find a greater difference than 3·55 per cent (55·78—52·222), and not 8 per cent at all. In the same way, between 52·222 and 46·270, I find only a difference of 5·952 per cent, and not 11 or 12 per cent. It would be well, then, to know on what basis M. Dubrunfaut goes to establish the differences which he quotes.

LECTURE EXPERIMENTS.‡

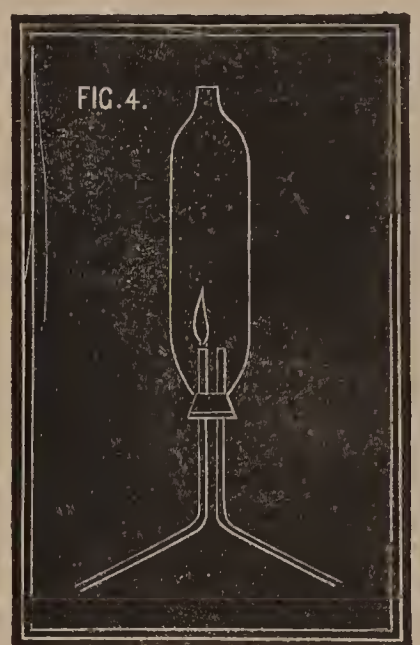
By Professor THOMSEN, of Copenhagen.

Reciprocal Combustion of the Elements of Water.

This may be demonstrated very instructively in the following manner. A pair of narrow platinum tubes, 1 centimetre long and 1 m.m. in diameter, are formed up of quite thin platinum foil. These tubuli are melted into a pair of small glass tubes, and are thus made the burners of the two gases, hydrogen and oxygen. The glass tubes are passed through openings about 1 to 1½ centimetres apart in an india-rubber cork. One tube is connected with the oxygen, the other with the hydrogen reservoir. After the cocks of the reservoirs are proportionately opened the hydrogen is ignited. The cork with its two burners is then inserted into a glass tube about 10 to 15 centimetres long, with its upper end much narrowed down, but still left open. The entire apparatus has then the form shown in the following woodcut.

The hydrogen now burns in oxygen, the fusing together of the orifice of the glass tube being prevented by the end of platinum. If, now, the cock of the oxygen reservoir be slowly turned off, and the quantity of oxygen so diminished, the point is soon reached at which the supply becomes insufficient to support the combustion of the hydrogen; the hydrogen flame expands, then disappears for some moments, the flame appears at the oxygen burner, and now without any interruption the oxygen burns in hydrogen. If the oxygen cock be gently opened, the flame withdraws itself to the hydrogen burner, and again hydrogen burns in oxygen. The phenomenon can be repeated as often as desired without extinguishing the flame, provided that the increase or diminution of the stream of oxygen is not made too suddenly.

The experimeut is highly captivating, and it is quite impossible for the observer who has not carefully watched it from the beginning to decide which of the two gases indeed is the combustible one. It shows the reciprocity



of the combustion in the clearest manner. It is evident that while the oxygen is burning an excess of hydrogen issues from the orifice of the large tube and can be ignited there, so that the combustion of hydrogen in the air, and that of oxygen in the hydrogen, can be shown at the same time.

Combustion of Oxygen with a Sooty Flame.

Heavy hydrocarbons, like benzol and oil of turpentine, burn with a very sooty flame; with a very similar flame oxygen also burns in the vapours of these bodies. The experiment is made in the following manner:—Some benzol is warned to the boiling-point in a long-necked flask; the flask is closed by a cork with two holes, through one of which a glass tube of about 1 centimetre bore is passed, and through the other a tube somewhat narrower and bent to one side. When the vapour arrives at the orifice of the wider tube it is lighted, and then a tube, through which a slow stream of oxygen is flowing, is passed down into the flask through the flame. The oxygen tube is bent above, and its mouth is provided with a platinum tube fused into it. A cork upon the oxygen tube closes the wider tube of the flask. The benzol flame is extinguished, and the vapour issues through the side tube, while the oxygen burns in the benzol vapour with a very sooty flame.

Easy Demonstration of Oxidation and Reduction and their Accompanying Change of Weight.

Oxide of copper is rubbed together with some gum-water to a stiff paste, and made into a cylinder of about 1 centimetre in diameter and 3 centimetres in length. It is then dried, ignited, and reduced by hydrogen at a low temperature. The reduced copper has the form of a cylinder, is very porous, but, at the same time, is tenacious

* See last number of the *Sucrerie Indigène* (June 20, 1870), pp. 75—78.
† This practice is pursued in some laboratories.
‡ Translated by Prof. Leeds, from the *Ber. d. Deutsch. Chem. Gesell.* Communicated by the Editors of the *Journal of the Franklin Institute.*

enough not to fall into a powder. It is surrounded with some platinum foil, the end of which is fused into a glass tube. Two tubulated bell-glasses, connected by tubes with gas-holders, are now filled, the one with hydrogen the other with oxygen. The mouth of the hydrogen bell opens below, that of the oxygen above. The gases are now allowed to stream through the bells, the copper cylinder is made somewhat warm without allowing it to attain the point of ignition, and introduced into the oxygen bell. It forthwith ignites and continues to glow until its oxidation is complete. Then it cools off, and the light dies away. When taken out of the oxygen and put in the hydrogen bell it begins to glow fiercely again, much water condenses upon the sides of the bell, while the cylinder is reduced to metallic copper. We have here the highly interesting phenomenon of a body which burns twice in succession, first in oxygen and then in hydrogen, and both times with the same intense light and heat. After the second burning it is the same body as in the beginning. Since the change of weight in such a cylinder amounts to a gramme, the addition and subtraction can very easily be verified by a common balance.

NOTE ON THE ANALYSIS OF CHROME-IRON ORE.

By J. E. STODDART.

In "Select Methods of Chemical Analysis," page 54, is given a method for estimating chromium, by Professor Storer, which is said to be applicable to such compounds as chrome-iron ore.

This method consists in oxidising the chromium compound by means of nitric acid and chlorate of potassium; chromic acid is then formed, and estimated in the form of chromate of barium.

Being engaged in the analysis of chrome-iron ores, and this process seeming to promise good results in a short time, I made trial of it, under the direction of Professor Thorpe.

Half a gramme of the ore, pulverised to an impalpable powder, and then passed through very fine muslin, was heated with nitric acid and chlorate of potassium, in an evaporating dish as directed in the process. After an hour's heating, which is considerably longer than the time given by Professor Storer, there still remained some of the ore undecomposed, which after fusion with carbonate of sodium and nitrate of potassium, and treatment with boiling water, yielded a tolerably large precipitate of chromate of lead, on the addition of excess of acetic acid and acetate of lead.

This result seems to show that even after an hour's treatment with nitric acid and chlorate of potassium there still remains a pretty large amount of undecomposed chrome ore, and that therefore this method does not present any great advantage over the ordinary, although somewhat more tedious, process of fusion.

Laboratory, Andersonian University,
Glasgow.

NOTICES OF BOOKS.

Chemical Note-Book. London: S. Deacon, 30, King Street, Borough, S.E.

This useful work will be found a boon to pupils, as well as to teachers, especially by insuring uniformity in making notes of experiments. Its arrangement and intended use may best be shown by the reproduction, on a smaller scale, of one of its pages, which reads as follows:—

Description of Substance

Deportment of Solution with Reagents

Hydrochloric Acid.	Sulphuretted Hydrogen.	Ammonic Sulphide.	Ammonic Carbonate.	Sodic Phosphate.

Additional Experiments for the Detection of Metals

Experiments for the Discovery of the Non-Metallic Constituents

Name of Substance

Confirmatory Experiments

CORRESPONDENCE.

ATOMIC VOLUMES.

To the Editor of the Chemical News.

Sir,—As you have favoured me by the insertion in your issue of the 26th of May, of some brief remarks upon the doctrine of "atomic volumes," I may, perhaps, be permitted to offer a few additional observations in relation to it.

In further illustration then of my own humble views, I would call attention to marsh-gas, H_4C, as another well-known typical compound, formed of 1 volume of carbon-vapour, C = 12, and 4 volumes of hydrogen, $H_4 = 4$, and, like the compounds to which I have before referred, viz., HCl, H_2O, and H_3N, presenting in the combination of its elements, a double product volume.

In this case, as the single volume of carbon vapour exists in equal proportions in each of the two compound volumes of H_4C, it follows, according to the reasoning in my previous communication, that it must be regarded as *at least* halved in the combination, like the single atomic volume of H and Cl, of O and of N, in the above compounds respectively. The atomic volume combining weight of carbon vapour would, in this point of view, be 6 instead of 12, as generally received. If, however, we consider more closely the constitution of H_4C, and bear in mind that the single volume of C, and the 4 volumes of H are not simply mixed, but *chemically combined*, in the condensed double compound volume, we can, I think, but conclude that an equal proportion of this 1 volume of carbon must be associated with each of the 4 hydrogen volumes; or, otherwise expressed, that the unit volume C is separated or divided into four in the formation of the product volumes H_4C, just as I have ventured previously to hint that the single volume of N is divided into three in H_3N. Should this deduction be correct, it will be necessary still further to divide the present atomic volume weight of C, and represent it by $\frac{12}{4}$, or 3, in lieu of either 12 or 6.

It is evident that a multitude of other compound gases may be viewed in the same light relatively to the *single* elementary volumes that enter into them, and that, accordingly, as I have presumed to infer, the atomic or combining weights of the elements should be regarded as strictly multiples of the true atomic volume weights. This theory is also supported by the fact that, in the case of mercury, Hg, 200, and of cadmium, Cd, 112, the atomic weights are *double* the respective vapour densities of those bodies.

I fear that I have already trespassed upon your valuable pages, and, therefore, will conclude in the words of one of our most distinguished chemists, Dr. Hofmann, "we are liable (speaking of bodies that he refers to) to be misled by analogy: to assume, for example, coincidence

of the *combining* weight with the *volume* weight, whereas, in truth, there may be, as in the cases of phosphorus and arsenic, *no such coincidence*, but an exceptional divergence; the combining weight corresponding to half the volume only, not to the full volume of the gas, or even, perhaps, standing in *some less simple ratio* thereto."*—I am, &c.,

W. H. O.

Mildmay Road, Stoke Newington.
June 9, 1871.

MAGNETIC CURVES.

To the Editor of the Chemical News.

Sir,—As I take great interest in lecture illustrations I was much pleased to read Dr. Mayer's article on the exhibition of magnetic curves. Would you allow me to say that a somewhat similar method to the one Dr. Mayer describes is in use by Mr. W. F. Barrett, of the International College.

Instead of using shellac, Mr. Barrett uses a solution of gum, which is allowed to dry on a piece of glass, and when the figures are obtained, they are fixed by breathing on the gummed glass. I would mention, too, that I have used, at Mr. Barrett's suggestion, the lantern I devised for cohesion figures with very satisfactory results. This lantern allows the slide to be horizontal, so that the actual formation of the curves is seen on the screen when the slide is tapped. One of my pupils, Mr. Anderton, used, some years ago, a similar method to that of Dr. Mayer, only instead of shellac as a cementing material, canada balsam was used.—I am, &c.,

C. J. Woodward, B.Sc.

Midland Institute, Birmingham.
June 10, 1871.

PALM OIL.

To the Editor of the Chemical News.

Sir,—If your space should permit it, will you allow me to append to the article on palm oil (by M. Gaston Tissandier), a few words, which, by preventing misapprehension, may render the information given more useful.

To many users of palm oil, the colour is of small importance, except in so far as it marks freedom from dirt and from some objectionable admixtures. Those who bleach the oil, look, I believe, not so much for an oil of *deep* colour as for one of *bright* colour. The brilliant orange-scarlet of fine Lagos oil is their desideratum. To makers of stearine, the "hardness" of the oil, that is, the proportion of hard fatty acids obtainable, is the important consideration.

I never saw a palm-oil adulterated with "fifty per cent of water." Parcels of oil occasionally come in, which give from ten to eighteen per cent of water, but these are classed with certainty under the head of "wet and dirty oil," and are very commonly valued with much accuracy by the samplers, who use no chemical test of purity, but simply inspect the samples when drawn. I think it would be difficult to induce a good palm-oil sampler to pass as "clean and merchantable" an oil containing ten per cent of water. But a mixture of two parts oil and one part water would need wonderful skill in mixing to render it passable as "clean."

To the notice of the oil, which may be expressed from the kernels of the palm nuts, it is desirable to add, that although it may be of "superior quality" in some points, yet it is quite different in character from the true palm-oil of commerce, which covers the outside of the nut as it grows. The palm-kernel oil is almost or quite useless for many purposes to which palm-oil is applicable. And an addition of the kernel-oil to the palm-oil, so far from raising the value of the latter, would lower this value considerably to almost all ordinary consumers of the article.—I am, &c.,

W. H. H.

Belmont Works, Battersea.

* "Modern Chemistry" p. 97.

MISCELLANEOUS.

Geologists' Association.—On the 2nd inst. an important paper was read at a meeting of this society by M. Hawkins Johnson, Esq., F.G.S., on the "Origin of Flints, and the Process of Silicification in General." After alluding briefly to the different modes in which these substances occur, the author proceeded to show that their formation is due to a chemical process which may be roughly expressed, in technical language, as the substitution of silicon for carbon. He pointed out how a crop of sponges invested with their gelatinous flesh or sarcode, and living at the bottom of a deep ocean, were suddenly buried in a thick stratum of white mud, consisting of the minute shells of *Foraminiferæ* that they then died, and while in the process of decomposition, this interchange of materials took place,—the nascent carbonic acid parting with its carbon in exchange for the silicon of the silicate of soda which sea-water is known to contain. At the close of the paper, the author produced a tadpole, upon which he had experimented, and which he had that afternoon subjected for two hours and a half to the action of nitric acid, without its undergoing any alteration, the inference being irresistible that the animal had become invested with a film of silica of sufficient thickness to protect it from the acid; another tadpole that had not undergone the same preparation having been converted into a brown cloud by immersion in the acid for the same time. The importance of this discovery in connection with the arts can scarcely be over-estimated; for the protection of perishable surfaces from decomposition is a desideratum that forces itself upon our notice daily.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Annalen der Physik und Chemie, von Poggendorff, No. 3, 1871.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Thermo-Chemical Researches.—Dr. J. Thomsen.—The continuation of a very lengthy monograph on this subject, this ninth section treating on the specific heat of aqueous solutions.

Experimental Optical Researches.—Dr. G. Quincke.—The concluding section of a lengthy monograph treating on Newton's coloured rings between surfaces of glass and metal.

Apparatus for Demonstrating the Axiom of the Paralellogram of Forces and the Laws of Equilibrium on an Inclined Plane.—G. Krebs.—A lengthy paper illustrated by several engravings.

Observations on the Theory of Capillary Phenomena.—A. Mousson.—An algebraico-physical essay illustrated with woodcuts.

Three Methods of Estimating the Resistance of Induced Currents.—Dr. F. Kohlrausch.

Reduction of the Second Axiom of the Mechanical Theory of Heat to General Mechanical Principles.—Prof. R. Clausius.

Limits of Applicability of the Law of Lenz-Jacobi.—Dr. A. von Waltenhoven.

Double Chloride of Zinc and Ammonium formed in Leclanché's Manganese Galvanic Element.—E. Priwoznik.

—This paper contains, in the first place, the detailed crystallographical description of the crystals just alluded to, illustrated by a woodcut. The composition of the crystals is, in 100 parts:—Zinc, 38·31; chlorine, 41·71; ammonia, 19·98. Formula, $ZnCl_2(H_3N)_2$. The chemical process by which it is formed in the Leclanché's cell is expressed by $2H_4NCl+Zn+2MnO_2=2H_3N+ZnCl_2+Mn_2O_3+H_2O$.

Determination of the Temperatures of Fusion and Solidification of Fatty Substances.—Dr. Th. Wimmel.—This paper contains a critical review and observations on an essay on the subject just alluded to, published in this periodical (vol. cxl., p. 420) by Dr. F. Rüdorff. The author of this paper states that fats exhibit two different temperatures of solidification (*erstarrungspunkte*)—one a natural, and another which might be termed artificial—the former of which, although not exactly coinciding with the fusion-point, is, at any rate, the most constant, and more readily determinable. The author points out the very great discrepancies existing between the various observations made by different authors on the fusion-point of the same kind of fat, adducing, as an instance, that beef suet, according to some authors, fuses at 37·6°, and, according to others, at 59·6°, and the same holds good for other fatty bodies, inclusive of Japan wax.

Observations on Prof. A. Heller's Memoir on Measuring the Intensity of Sound.—Dr. A. Scebeck.

Reply to Dr. Kenngott on his Remarks upon my Analysis of Chabasite.—Dr. C. Rammelsberg.—A vindication of the author's character against the insinuation made that he falsifies results of analysis.

Derivation (Ableitung) of the Behaviour of Gases towards Heat (Wärmeverhältnisz) under Constant Pressure and Volume $\left(\frac{c}{c'}\right)$ from the Mechanical Theory of Heat.——Dr. F. Mohr.

Description of a New Microscope Constructed by R. Winkel.—Prof. J. B. Listing.—It appears that the instrument here alluded to, made at Göttingen, is a masterpiece of good workmanship, the maker having especially taken care to guard against the deterioration of the lenses by the action of air and moisture, by reducing to a minimum the quantity of oxide of lead used in the composition of the glass the lenses are made of. In other respects, the instrument is not above those made by celebrated London and Paris microscope makers.

The American Journal of Science and Arts, May, 1871.

This number contains the following original papers relating to chemistry and allied sciences:—

Estimation of Phosphoric Acid.—C. E. Munroe.—Reserved for reproduction, an observation also applicable to the following paper, by the same author:—

Use of Porous Cones in Filtration.

Existence of the so-called Compound Ammonium-Amalgams.—By the late Dr. C. M. Wetherill.—This paper contains, in the first place, an account of the results of the experiments made at various periods by different scientific chemists for the purpose of obtaining amalgams with the so-called compound ammoniums. Next follows the minute description of the preparation of methyl-ammonium oxalate by Lea's process; and, lastly, the author states that a portion of fluid sodium-amalgam, the size of a pea, was placed in a small test-tube ($\frac{1}{4}$ inch diameter), and the solution of methyl-ammonium oxalate added, the swelling was observed, both with and without shaking, in the cold and warm. The same experiment was performed with different specimens of methyl-ammonium oxalate; in some instances the swelling was from 8 to 10 times the original volume, which was very much less than the turgescence of the ammonium salt. The methyl-ammonium amalgam presented the same buttery appearance as the ammonium amalgam. When the lump was pressed between two plates of glass, myriads of gas bubbles were apparent; when these were pressed out, the amalgam was at once restored to the condition of mercury. A piece of filter-paper was placed upon a glass plate, then saturated with a strong solution of the re-crystallised methyl-ammonium oxalate; a globule of mercury the size of a small pea was placed upon the paper with the negative poles of twenty Bunsen cells in contact with it, the positive pole touching the paper; the globule of mercury swelled slightly, presented a buttery appearance, attached itself to and amalgamated the blade of a penknife which was in contact with the negative pole, and, upon being pressed under a glass plate, showed innumerable gas bubbles in its substance—in fact, was a metallic froth. It results from these experiments that the compound ammonium examined by this author may form the so-called amalgam.

This number also contains several valuable papers bearing upon natural history and geology.

Neues Jahrbuch für Pharmacie, von Dr. F. Vorwerk, April, 1871.

This number contains the following original papers and memoirs more particularly relating to chemistry:—

A Practical Method for Preparing Lactic Acid.—Dr. C. O. Harz.—This lengthy memoir contains a review of practical applicability of the various methods in use, and prescribed in pharmacopœiæ, for the preparation of lactic acid. The author, after having made a series of experiments on this subject, found that, by using the following prescription, lactate of soda and other lactates may be readily prepared:—Take of milk sugar, 3 parts; water (not distilled, but good spring or river water), 36·0; a flour rich in gluten (the author has successfully employed the well-known *farina hordei præparata*), from 0·50 to 0·75 parts; beer yeast, one or two large-sized table-spoonfuls; crystallised carbonate of soda, 6 parts, or bicarbonate of soda, 3 parts. The essay further treats, at length, on the mode of preparation of various lactates as required in pharmacy.

Action of Carbolic Acid and Creosote upon the Animal Organism.—Dr. Th. Husemann.—A lengthy, but valuable, physiological essay.

As a curiosity, taken from among the secret remedies, the analyses of which are quoted in this number, we copy the following:—

Lemonade for Strengthening the Memory.—G. M. Raufer.—The author puffs up, and sells for three shillings, a quantity of fluid, about 30 grms., containing 15 parts of phosphoric acid, 15 of glycerine, and 70 of water. This stuff is sold in Vienna.

Monatsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin, April, 1871.

This number contains the following original papers relating to chemistry and collateral sciences:—

Subjective Colours of the Double Figures (Doppelbildern) of Coloured Glass Plates, and on the Colours of Thick Plates.—Prof. Dove.—An optico-physical essay which, although of great value, does not admit of any useful abstraction.

Composition of Native Compounds of Niobium and Tantalum in General, and more especially on that of Tantalite, Columbite, and Pyrochlor.—Dr. C. Rammelsberg.—This monograph is divided into the following sections:—History of the discovery and earlier researches of the group of bodies alluded to; brief account of the labours of MM. Rose, Marignac, Blomstrand, Roscoe, and Deville on this subject; tantalite and columbite—older analysis; exhaustive account of the results of analysis of six different specimens of tantalite investigated by the author; analysis of several specimens of pyrochlor and columbite. This monograph is not quite complete; the author states that he will also publish his researches on fergusonite, yttro-tantalite, euxenite, and other members of the tantalum group, and a general retrospective review of all these researches.

Bayerisches Industrie und Gewerbe Blatt, March, 1871.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Most Prominent Objects of the Industrial Exhibition held at Cassel last year, considered in a Technical, as well as Industrial, point of view.—Dr. E. Wiederhold.—The continuation of an excellent descriptive review, copiously illustrated with engravings, of the most useful and best-executed apparatus and tools intended for application in various ways; among these gas apparatus intended for use in localities where no large gas-works exist, or are near enough, occupy a prominent place, especially as regards the manufacture of gas from petroleum distillation, oil residues, and paraffin oil. Attention is also called to lucifer matches prepared entirely without phosphorus, either in the composition or on the paper intended for igniting them by friction. MM. Kalliwoda and Co., at Ortenberg (Grand Duchy of Baden), prepare a composition the essential ingredients of which are chlorate of potassa and hyposulphite of lead; the lucifer matches dipped with this composition ignite readily by friction on any rough surface, and are a little less costly than those containing phosphorus.

Recent Improvements made in the Wine Industry.—Dr. Ph. Neumann.—This lengthy memoir contains an excellent review of all that has been done of late years to improve the modes of making must, fermenting it, and all the novelties introduced to preserve wine by heating it, application of electricity, filtration, &c. We learn from this essay that in the wine-producing parts of Germany the manufacture of cognac from wine has become a profitable business, although the wines employed for this purpose are naturally less rich in alcohol than the French wines.

Annalen der Chemie und Pharmacie, May, 1871.

This number contains the following original memoirs and papers:—

Contribution to our Knowledge of the Sulpho-Nitrogen Acids.—Dr. A. Claus.—The continuation and end of this very lengthy and exhaustive monograph.

Normal Butylic Alcohol.—Drs. A. Lieben and A. Rossi.—Of this exhaustive monograph, the chief results were published in *Comptes Rendus*, vol. lxviii., p. 1561. The following are the headings of the sections contained in this memoir:—Review of the labours of Drs. Wurtz, Chancel, Fayet, Kolbe, and others on this subject; preparation of butyraldehyde; butylic alcohol; oxidation of butylic alcohol; butylic chloride; butylic bromide; butylic iodide; butyl-ethylic ether; butylen; butylic acetate; butylic butyrate; butylic cyanide; butylamine; constitution of the normal butylic alcohol, and general observations. From the latter we quote the following:—If we compare the boiling-points of the now known four isomeric butylic alcohols together we find that the boiling-point of the tertiary butylic alcohol is the lowest, that of the normal butylic alcohol the highest, and that, starting from the first-named, the differences between the cyphers indicating the boiling-point decrease, as is exhibited by the following tabulated review:—

	Normal butylic alcohol.	Fermentation butylic alcohol.	Secondary butylic alcohol.	Tertiary butylic alcohol.
	$C\begin{cases}CH_2.CH_2.CH_3\\H\\H\\OH\end{cases}$	$C\begin{cases}CH(CH_3)_2\\H\\H\\OH\end{cases}$	$C\begin{cases}CH_2.CH_3\\CH_3\\H\\OH\end{cases}$	$C\begin{cases}CH_3\\CH_3\\CH_3\\CH\end{cases}$
Boils at—	116°	109°	99°	82°
Difference—		7°	10°	17°

Normal alcohols may be defined as those which, of all isomeric alcohols, have—(1) The highest boiling-points, and are the most stable; (2) the simple and compound ethers of which, as, also, the amines thereof, have relatively the highest boiling-point, and the haloid combinations of which are least readily split up by the throwing off of CnH_{2n}; (3) when oxidised, these bodies yield acids containing the same number of carbon atoms, and are distinguished from eventually (*eventuellen*) isomeric acids, by having a higher boiling-point and more power of resisting oxidation.

Investigation of a Himalaya Tea.—Dr. P. Zöller.—This paper contains the results of a series of experiments made with a sample of very fine tea, a small quantity of which had been sent to Prof. J. von Liebig by a proprietor of tea plantations. 100 parts of the tea contained 4·95 of water and 5·63 of ash, which, in 100 parts, was found to consist of—Potassa, 39·22; soda, 0·65; magnesia, 6·47; lime, 4·24; peroxide of iron, 4·38; manganoso-manganic oxide, 1·03; phosphoric acid, 14·55; sulphuric acid, a trace; chlorine, 0·81; silica, 4·35; carbonic acid, 24·30. Extract soluble in water of this tea amounted to 36·26 per cent. The nitrogen in the air-dried tea was found to be 5·38 per cent. In addition to 4·94 per cent of theine, this tea was found to contain some theobromine. The quantity of nitrogen in the extract (that is, the portion of the tea soluble in water by infusion), dried at 100°, is 10·09 per cent, while the ash therein amounts to 11·46 per cent. The author concludes by stating that this tea is equal to the best Chinese tea, and that the younger the leaves of the shrub the better the tea they yield.

Decomposition of Acrolein Ammonia by Dry Distillation.—Dr. A. Claus.—The result of the author's researches prove that acrolein ammonia yields, by dry distillation, a volatile base, which is, however, while volatilising, partly decomposed. The crude oily product of the dry distillation yields, when submitted to distillation at from 120° to 150°, a volatile base (a new compound) which has a constant boiling-point, and is picoline.

Further Researches on Propargyl Ether.—Drs. C. Liebermann and O. Kretschmer.

Oxypicric (Styphnic) Acid.—Dr. J. Schreder.—The author, in the first portion of this lengthy memoir, proves the identity of styphnic acid, $C_6H_3N_3O_8$, with the trinitro-resorcin of Weselsky, the quantities of carbon and hydrogen in these substances having been found to amount, respectively, to 29·41 and 29·42 and 1·64 and 1·62. The memoir is further divided into the following sections:—Triamido-resorcin; hydrochlorate of amido-diimido-resorcin.

Basicity of Gluconic and Lactonic Acids.—Prof. H. Hlasiwetz.—The contents of this paper are mainly devoted to a critical review of Dr. Fittig's essay "On the Constitution of the so-called Carbo-Hydrates," separately published under the following German title:—"Festschrift zer Feier des Geburtstages Sr. Maj. des Königs von Würtemberg," Tübingen, L. F. Fues, 1871. However valuable the above-named paper may be, it is not well suited for abstraction, chiefly on account of the very large number of lengthy and complex formulæ which would require to be reproduced, an observation which equally applies to the two following papers:—

Formation of a Methyloisethionic Acid.—Dr. E. Erlenmeyer.

Lactic Acid contained in Muscular Tissue.—Dr. E. Erlenmeyer.

Polytechnisches Journal von Dingler, first number for May, 1871.

This number contains the following original papers and memoirs relating to chemistry and collateral sciences:—

Observations on the Application and Caloric Statistics of Reverberatory Furnaces to Iron-Foundry Purposes.—Dr. E. F. Dürre.—A lengthy monograph of considerable value in reference to reverberatory furnaces in general, and the best mode of constructing them so as to utilise all the available heat of the fuel.

Henderson's Patented Method of Separating Phosphorus from Crude Cast-Iron.—P. Tunner.—Mr. Henderson's method, patented in New York, is fully described in the *Engineer* of the 17th of February last (p. 110). The author does not state anything new on this subject.

M. Kröncke's Method of Amalgamating Silver Ores, as used at Copiapo, Chili.—Dr. L. Eich.—The method alluded to may be summarised in the following manner:—The use of a hot and concentrated solution of protochloride of copper (*kupferchlorür*) and common salt—the latter serving the purpose of keeping a larger quantity of the former salt in solution, and avoiding the formation of basic salts of copper. The use of perfectly dry and very finely ground up ore, so that the solution just alluded to may thoroughly penetrate; wet, or moist ore, moreover, causes the formation of basic salts of copper, and thereby loss of active protochloride of that metal. Use of lead and zinc along with mercury and the protochloride of copper, in such quantities that the reactions take place according to the following formulæ:—

$$3AgS,SbS_3+3Cu_2Cl+NaCl=3AgS+SbCl_3+3Cu_2S+NaCl,$$

and $$AgS+Cu_2Cl+Zn+NaCl=Ag+Cu_2S+ZnCl+NaCl.$$

Lastly, the materials (ores) to be operated upon should always, as much as possible, be of the same richness and quality throughout.

Chemical Analysis of a Cement-Stone.—Dr. E. Reichardt.—The portion soluble in hydrochloric acid, amounting to 89·16 per cent, was found to consist of—CaO,CO_2, 49·84; MgO,CO_2, 26·96; CaO,SO_3, 0·36; Fe_2O_3, 3·83; FeO, 4·17; MnO, 0·22; NaO, 0·40; KaO, 0·35; SiO_3, 2·96; HO, 0·66. The rest, insoluble in hydrochloric acid, consists of—CaO, 0·76; MgO, 0·14; Fe_2O_3, 0·60; Al_2O_3, 1·60; KaO, 0·13; NaO, 0·19; SiO_3, 3·78; sand, 3·26.

Transparent Aniline Lakes and the Colouration of Mica.—F. Springmühl.—This paper treats, at great length, on the preparation of varnishes coloured with various aniline and other similar colours.

Method for Recovering Tartaric and Oxalic Acids from so-called Dischargers, and on the Use of Hypochlorite of Soda (Eau de Labarraque), instead of Bleaching Powder, as Discharge for Turkey-Red Dyed Fabrics.—Dr. A. Müller.

Sugar Refined with Sulphurous Acid according to Dr. Seyfert's Process.—Dr. P. Schulze.—This essay treats, at great length, on the effect of this process upon the refined sugar, as detectable by a peculiar taste and by its becoming yellow, and on the precautions refiners have to take when applying the sulphurous acid gas.

Valuation of the Quantity of Oil contained in Oil-Seeds.—Dr. H. Vohl.—A lengthy memoir, accompanied by the engraving of an ingeniously-contrived apparatus for the accurate estimation of the oil contained in various seeds.

Manufacture of Benzoic Acid from the Urine of Horses and Cattle on the Large Scale.—C. J. Kaufmann.—The author's exact process is not described, but, from 35,000 cwts. of urine, annually 70 cwts. of pure benzoic acid are prepared, which is chiefly consumed in England and France in the manufacture of aniline colours.

Method of Distinguishing Chloroform made from Chloral from Chloroform made from Alcohol and Bleaching-Powder.—Dr. Hager.—In the first place, the former chloroform, known now on the Continent as "English chloroform," has a sp. gr. of only 1·485, and contains, according to the author, from 0·75 to 0·8 per cent of strong alcohol. When, to the chloroform prepared from alcohol and bleaching-powder, strong sulphuric acid is added, it always becomes more or less coloured, which does not happen to be the case with the chloroform prepared from chloral; when, moreover, a few drops of each kind of chloroform are left to evaporate spontaneously upon a watch-glass, the chloroform prepared with alcohol gives off, after a while, a disagreeable smell, while the other kind retains until complete volatilisation its pleasant fruity odour.

Journal für Gasbeleuchtung und Wasserversorgung, No. 8, 1871.

This number contains, besides a series of papers strictly relating to gas and waterworks' engineering, the following information relating to—

Carboxygen Illumination.—Dr. J. Philips.—The carbolin (the fluid used in the lamps invented by the author) costs about the same as petroleum—that is to say about 6d. per Prussian quart (1·145 litres). The hourly consumption of carbolin, amounting, on an average, to 30 grms., will cost about one halfpenny. As regards the price of the oxygen gas (an artificial air containing from 50 to 75 per cent of oxygen), it is stated that this may be prepared on the large scale at a cost of 15 centimes (2½d.) per cubic metre (a little more than 35 English cubic feet). The carboxygen illumination is daily gaining ground in Cologne, Brussels, New York, and other places. This light is also employed now in some large hot-houses where plants and shrubs are exhibited, the colours of leaves and flowers seen by this light being seen exactly as in daylight, while there is no danger of injuring the most tender and delicate plants, as there is with other artificial modes of lighting.

No. 9, 1871.

This number does not contain any other matter except what strictly belongs to gas and waterworks' engineering and management.

Les Mondes, March 23, 1871.

Recent unfortunate occurences have caused a long delay in the transmission of this number, which, among a great variety of other very readable and excellent matter, contains the following, relating more particularly to chemistry and collateral sciences:—

New Plastic Material.—Rev. F. Moigno.—Very pure and very finely-pulverised phosphate of lime (bone-ash) is made into a paste by means of collodium. After having become dry, this substance becomes very hard, and assumes an excellent polish.

Collodium Prepared with Silk.—Rev. F. Moigno.—Pure silk is soluble in hydrochloric acid. If this solution is first neutralised by ammonia, and next evaporated at a gentle heat, the residue yields an organic chloride of ammonium suitable for photographic use.

Chemical Investigation of the Black-Coloured Matter of the Tadgerite.—Dr. S. Meunier.—By tadgerite is understood the stony matter which constitutes the meteorite fallen at Tadgera, near Sétif, Algeria, on the 9th of June, 1869. The author comes to the conclusion that the blackish colouration which obtains in grey-coloured

meteorites by the action of heat is due to the separation, in these substances, of a peculiar compound of peridotic nature; this separation is due to a kind of liquation of the silicates, especially those whereof the composition is akin to that pyroxen and amphibole. The author calls attention to the fact that the heating to redness of a small fragment of meteorite may often give a precious clue to its mineralogical composition; so treated, for instance, the meteorite of Montréjeau exhibits irregular-shaped granules upon a whitish ground, which granules are not rendered manifest by analysis. The Pultusk meteorite exhibits colourless grains, which act strongly upon polarised light and are invisible before the stone is heated to redness.

Bulletin de l'Académie Impériale des Sciences de St. Petersbourg, vol. xvi., No. 1, 1871.

This number only contains the following original paper bearing on chemical science:—

Place of Cerium in the System of Elementary Bodies.—Dr. D. Mendéleyef.—Since the value of this lengthy and learned memoir almost entirely depends upon the reproduction of a series of formulæ, as well as upon that of a tabulated review of the system of the elements, divided and arranged according to the author's views into eight groups, each sub-divided again into ten typical series, it is not possible to give here an abstract of this highly scientific memoir, in which, moreover, several references occur relating to papers published by the author in the Russian language.

NOTES AND QUERIES.

Hydrate of Chloral.—Being anxious to make some pure hydrate of chloral, I shall be glad if you will inform me of the best and simplest method of making it, and purifying it when made?—W. E.

Disulphide of Carbon.—I should be glad if you or any of your correspondents would kindly tell me if the demand for disulphide of carbon is good; also, where the market is for the same, and what the price is per pound.—Sulphur.

Estimation of Sulphur in Dense Coke.—Can any of your readers inform me of a good method for the estimation of sulphur in *very dense* coke? The methods I have tried do not yield very accurate results, on account of the high temperature required for decomposition.—S. Reddrop.

Carbolic Acid.—(Reply to W. G. Holding.)—To obtain the acid in dry state, recourse must be had to digestion with chloride of calcium, followed by a new rectification. If required pure, only that portion which boils at 370° F. must be received; the distillate, by refrigeration, furnishes crystals of the acid, which must be drained, dried, and preserved from the air.

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THE CHEMICAL NEWS.

Vol. XXIII. No. 604.

EXAMPLES FOR PRACTICE IN QUANTITATIVE ANALYSIS.

By ALEXIS A. JULIEN.

The following examples in quantitative analysis are executed in the School of Mines, Columbia College. As the atomic weights employed in calculating the results of analyses have been corrected from time to time by more accurate determinations, I give at the outset the latest numbers.

Table I.—Atomic Weights.

H = 1.

	O=8.	O=16.		O=8.	O=16.
Al	13·7	27·4	Hg	100·0	200·0
Sb	122·0	122·0	Mo	48·0	96·0
As	75·0	75·0	Ni	29·0	58·0
Ba	68·5	137·0	N	14·0	14·0
Bi	210·0	210·0	Os	100·0	200·0
B	11·0	11·0	O	8·0	16·0
Br	80·0	80·0	Pd	53·0	106·0
Cd	56·0	112·0	P	31·0	31·0
Cs	133·0	133·0	Pt	98·7	197·4
Ca	20·0	40·0	K	39·0	39·0
C	6·0	12·0	Rh	52·0	104·0
Ce	45·7	91·4	Rb	85·4	85·4
Cl	35·5	35·5	Ru	52·0	104·0
Cr	26·1	52·2	Se	39·5	79·0
Co	30·0	60·0	Si	14·0	28·0
Cb	94·0	94·0	Ag	108·0	108·0
Cu	31·7	63·4	Na	23·0	23·0
Di	47·5	95·0	Sr	44·0	88·0
E	56·3	112·6	S	16·0	32·0
F	19·0	19·0	Ta	182·0	182·0
Gl	4·6	9·2	Te	128·0	128·0
Au	197·0	197·0	Tb	75·0	75·0
H	1·0	1·0	Tl	204·0	204·0
In	72·0	72·0	Th	119·0	238·0
I	127·0	127·0	Sn	59·0	118·0
Ir	99·0	198·0	Ti	25·0	50·0
Fe	28·0	56·0	W	92·0	184·0
La	46·0	92·0	U	60·0	120·0
Pb	103·5	207·0	V	25·6	51·2
Li	7·0	7·0	Y	30·8	61·6
Mg	12·0	24·0	Zn	32·5	65·0
Mn	27·5	55·0	Zr	44·8	89·6

Strength of Reagents.

The reagents used in the laboratory are all prepared with the strength recommended by Fresenius in his "Manual of Qualitative Chemical Analysis." By the use of the following table, the amount of a reagent required for any precipitation can be readily calculated; though in actual practice it is generally advisable to employ a few more cubic centimetres, and also to add a few drops, first to the supernatant liquid, and afterward to the filtrate:—

Table II.

Dilute sulphuric anhydride,	1 c.c.	will	precipitate	0·231 grm.	Ba
Barium chloride,	,,	,,	,,	0·032 ,,	SO_3
Disodic orthophosphate	,,	,,	,,	0·011 ,,	Mg_2O
Magnesia mixture,	,,	,,	,,	0·024 ,,	P_2O_5
Ammonic molybdate,	,,	,,	,,	0·001 ,,	P_2O_5
Ammonic oxalate,	,,	,,	,,	0·016 ,,	Ca_2O
Argentic nitrate,	,,	,,	,,	0·010 ,,	Cl

Example No. 1.—Analysis of Fine Iron Wire.

Determination of Fe.—Weigh out from 0·3 to 0·5 grm. of the wire (previously rubbed bright with sand-paper). Throw into a large covered beaker, add about 20 c.c. concentrated hydrochloric acid and heat until solution. Then add about 5 c.c. concentrated nitric acid, heat until the colour becomes a deep orange. The solution often becomes previously coloured to a brownish black, from the solution of nitric peroxide in protochloride of iron, but this is removed by longer boiling, and the orange colour finally appears. Rinse off the convex cover, dilute largely, add ammonia in excess, heat to boiling, allow to settle, pass the clear supernatant fluid through a five-inch filter, and wash by decantation several times, boiling the precipitate with distilled water. Finally throw on the filter and wash with boiling water, until a few drops of the filtrate, caught in a test-tube, and acidified with dilute nitric acid, give no reaction for chlorine on addition of a little argentic nitrate. Dry thoroughly, empty the precipitate into a weighed crucible, burn the filter completely on a platinum wire, and add the ashes to the crucible; ignite at first very slowly, and finally very strongly; cool in desiccator, and weigh. Repeat this ignition until the weight remains constant (within one milligramme). To determine the amount of silica in this precipitate derived from the glass beaker, empty the ignited precipitate into a small flask, add 10 c.c. concentrated hydrochloric acid, boil until solution, empty into a casserole, evaporate to dryness, again dissolve in concentrated hydrochloric acid, dilute, filter through a three-inch filter, dry, ignite in a weighed crucible, and weigh. This weight of silica thus found, corrected for the silica of the two filter ashes, is deducted from the previous amount; and, from the weight of Fe_2O_3 thus found, the weight and the percentage of Fe are readily calculated.

Example No. 2.—Analysis of Magnesic Sulphate.*

A. *Determination of* MgO.—Dissolve 1 to 1·5 grms. in cold water, and add one-fifth its volume of ammonium chloride and sufficient disodic orthosphosphate (see Table II.) Be careful not to rub the side of the beaker with the stirring-rod. Cover, and set aside twelve hours. Filter and wash with cold water, to which one-quarter its volume of ammonia has been added, until the filtrate, on being tested for chlorine with argentic nitrate, gives only a slight opacity. Dry and ignite as usual to constant weight. If the precipitate or ash is not perfectly white, moisten with a drop or two of concentrated nitric acid, evaporate and ignite cautiously, and repeat this operation to a constant weight. Result, $Mg_2P_2O_7$.

B. *Determination of* SO_3.—Dissolve 1 to 1·5 grms. in water, acidulate with hydrochloric acid, dilute largely, boil, add barium chloride in slight excess (Table II.); heat for a few minutes, allow to settle, and decant the clear liquid on a filter. Wash several times by decantation with boiling water; throw on the filter, wash thoroughly, dry, and ignite. Empty into a small beaker, digest with very dilute hydrochloric acid, filter, wash thoroughly, dry, and ignite. Result, $BaSO_4$.

C. *Determination of* H_2O.—In a weighed porcelain crucible weigh out 1 to 1·5 grms. Heat very gently at first over a low flame, to avoid loss by the ebullition of the salt, and finally lower the crucible until about an inch above the extremity of the flame. After heatiug thus about an hour, weigh, and repeat the operation until the weight remains constant. A portion of the acid may be expelled if the temperature is too high.

Example No. 3.—Analysis of Barium Chloride.

A. *Determination of* Ba.—Dissolve 1 to 1·5 grms. in water, acidulate with hydrochloric acid, dilute largely, heat to boiling, add dilute sulphuric anhydride in sufficient excess (Table II.), and continue as in Example No. 2, B. Result, $BaSO_4$.

* In order to make the student entirely dependent for accuracy upon his own skill and care, the substance given out for analysis in this and all subsequent cases is intermixed with known proportions (in several varieties) of certain inert salts, &c. So that while none of the determinations are affected by this intermixture, the student is entirely unaware of the percentages to be found.

B. *Determination of* Cl.—Dissolve 1 to 1·5 grms. in cold water; acidulate with nitric acid; add argentic nitrate in sufficient excess (Table II.); stir well; warm gently until the precipitate has completely settled; wash thoroughly with boiling water acidulated with nitric acid, at first by decantation, and afterward on a filter; and dry. Remove the precipitate as completely as possible to a weighed porcelain crucible, brushing the paper with a feather. Burn the filter on the cover and empty the ashes into the crucible. Moisten the whole with a few drops of concentrated nitric acid; digest 5 to 10 minutes; add 2 or 3 drops of concentrated hydrochloric acid; evaporate cautiously, and fuse at as low a temperature as possible. Result, AgCl.

C. *Determination of* H_2O.—Heat 1 to 1·5 grms. in weighed porcelain crucible, exactly as in No. 2, C. A portion of the chlorine may be expelled if the temperature is too high.

Example No. 4.—Analysis of Cupric Sulphate.

A. *Determination of* CuO.—Dissolve 1 to 1·5 grms. in a porcelain casserole, dilute, heat to boiling, add pure potassic hydrate to an alkaline reaction, and boil until the precipitate settles readily on the removal of the gas flame. Decant rapidly the clear fluid through a filter; wash two or three times by decantation with boiling water; transfer to the filter, removing as completely as possible from the casserole; wash thoroughly and dry. Empty the precipitate as completely as possible into a weighed porcelain crucible, burn the filter on the cover, empty the ashes into the crucible, and ignite strongly. As a trace of the precipitate generally adheres so firmly to the casserole that it cannot be rubbed off, dissolve it now in a drop or two of warm dilute nitric acid, and moisten the whole precipitate and filter ashes in the crucible (which may contain a little reduced copper), with this solution, and again evaporate and ignite cautiously, and at last strongly, to a constant weight. In this precipitate determine the silica derived from the casserole exactly as in Example No. 1. Result, CuO.

Concentrate the filtrate and washings from the original precipitate of hydrated oxide of copper, acidify with hydrochloric acid, and test with sulphuretted hydrogen water.

B. *Determination of* SO_3.—Proceed exactly as in Example No. 2, B.

C. *Determination of* H_2O.—Heat 1 to 1·5 grms. in a weighed porcelain crucible at 140° C., until the weight remains constant. Loss = 4 equivalents of water.

Heat again over the flame, as in Example No. 2, C, until the salt becomes white and the weight remains constant. Second loss = last equivalent of water. If the temperature be too high during the last heating a portion of the acid may be expelled: in which case a dark tinge, due to CuO, may be observed near the sides and bottom of the crucible.

Example No. 5.—Analysis of Silver Coin.

Clean with potassic hydrate, wash, dry, and weigh.

A. *Determination of* Au.—Dissolve in a little pure concentrated nitric acid, dilute, filter, wash thoroughly, dry, ignite in a porcelain crucible, and weigh. Result, Au + AgS. Wrap in a little pure silver, heat in cupel to fusion, cool, dissolve in nitric acid, filter, wash, dry, ignite, and weigh. Result, Au.

B. *Determination of* Ag.—Heat the first filtrate to boiling, add dilute hydrochloric acid in excess, and continue as in Example No. 3, B. Result, AgCl.

C. *Determination of* Pb.—To filtrate from B add 10 c.c. dilute sulphuric anhydride, evaporate to dryness, dissolve in warm water, filter, wash, dry, ignite in a porcelain crucible, and weigh. Result, $PbSO_4$.

D. *Determination of* Cu.—Heat to boiling the filtrate from C. in a casserole, add pure potassic hydrate in excess, and continue exactly as in Example No. 4, A. Result, CuO.—*American Chemist.*

THE NATURE OF DIFFERENT GUMS.

Dr. Sacc, of Neuenberg, Switzerland, has made an extensive inquiry into the nature of different resins. We condense from it the following results:—The resins spoken of are copal, amber, dammar, common resin, shellac, elemi, sandarach, mastic, and Caramba wax. All these resins can be reduced to powder.

The following will become pasty before melting:—Amber, shellac, elemi, sandarach, and mastic; the others will become liquid at once.

In boiling water, Caramba wax will melt; common resin will form a semi-fluid mass; dammar, shellac, elemi, and mastic will become sticky; while copal, amber, and sandarach will remain unchanged.

Dammar and amber do not dissolve in alcohol; copal becomes pasty; elemi and Caramba wax dissolve with difficulty; while resin, shellac, sandarach, and mastic dissolve easily.

Acetic acid makes common resin swell; on all the others it has no effect.

Caustic soda dissolves shellac readily, resin partly; but has no influence on the others.

Amber and shellac do not dissolve in sulphide of carbon; copal becomes soft and expands; elemi, sandarach, mastic, and Caramba wax dissolve slowly; while resin and dammar dissolve easily.

Oil of turpentine dissolves neither amber nor shellac, but swells copal; dissolves dammar, resin, elemi, sandarach, and Caramba wax easily, and mastic very easily.

Boiling linseed oil has no effect on copal, amber, and Caramba wax; shellac, elemi, and sandarach dissolve in it slowly, while dammar, resin, and mastic dissolve easily.

Benzol does not dissolve copal, amber, and shellac, but does elemi and sandarach to a limited extent, and Caramba wax more easily; while dammar, resin, and mastic offer no difficulty.

Petroleum ether has no effect on copal, amber, and shellac; it is a poor solvent for resin, elemi, sandarach, and Caramba wax, and a good one for dammar and mastic.

Concentrated sulphuric acid is indifferent to Caramba wax; it dissolves all resins, imparting to them a dark brown colour, excepting dammar, which takes a brilliant red tint.

Nitric acid imparts to Caramba wax a straw colour; to elemi, a dirty yellow; to mastic and sandarach, a light brown; it does not affect the others.

Ammonia is indifferent to amber, dammar, shellac, elemi, and Caramba wax; copal, sandarach, and mastic become soft, and finally dissolve; while resin will dissolve at once.

It is not difficult, by means of these reactions, to test the different resins for their purity.—*Dingler, Polytech. Journal.*

ON THE EMPLOYMENT OF POTASSIUM FERRICYANIDE AS A TEST FOR COBALT, NICKEL, AND MANGANESE.

By ALFRED H. ALLEN, F.C.S.

In a former number of the Chemical News a correspondent called attention to the possibility of distinguishing cobalt from nickel by potassium ferricyanide, which he directed to be added to a solution of the metal after addition of tartaric acid and excess of ammonia. My former pupil, Mr. J. Spear Parker, while in my laboratory showed that the tartaric acid might be replaced by citric acid, or omitted altogether, any ammoniacal salt answering the purpose. Cobalt occasioned a deep red colouration, while with nickel no change occurred if sufficient ammonium chloride

were present. The reaction is delicate and trustworthy. I find, however, that a solution of nickel to which ammonium chloride, ammonia, and excess of potassium ferricyanide have been added, becomes turbid on boiling, and in a short time deposits the whole of the nickel as a copper red precipitate, which often adheres to the sides of the vessel. Thus the presence of nickel is readily detected by a simple test, which answers perfectly well even in the presence of a large excess of cobalt,—the latter metal being indicated at the same time by the red colour of the solution.

The red precipitate I at first supposed to be nickel ferricyanide, and to have the same composition as the gelatinous reddish brown precipitate produced by potassium ferricyanide on addition to nickel nitrate. This was rendered the more probable from a similarity of decomposition by boiling with sodium hydrate, which readily converts the nickel into a black precipitate (probably Ni_2O_3) with production of sodium ferro- and ferricyanides.

The brick-red precipitate does not contain sufficient nickel to correspond to either $Ni_2''(C6N_6Fe)''''$, or $Ni_3''(C_6N_6Fe)_2''''$, and it evolves ammonia during the action of the soda.

I intend to examine this precipitate further, and to try and utilise it for the quantitative estimation of nickel, as whatever its composition may be, it is easily produced, easily washed, and readily decomposed into a definite compound by alkalies.

When a solution of manganous sulphate is treated with ammonium chloride and ammonia, and potassium ferricyanide gradually added to the solution so obtained, a whitish precipitate is first formed, which changes with the addition of more ferricyanide to a dark brown colour. This reaction is used in my laboratory to distinguish manganese from cobalt and nickel. The solution filtered from the precipitate contains ferrocyanide and ferricyanide, thus proving that the manganese has suffered oxidation at the expense of the ferricyanide. It is absolutely free from manganese, no green colour being developed by evaporation and fusion with alkaline carbonate and nitre. If the precipitate be boiled in the liquid containing excess of potassium ferricyanide, and then filtered from the solution, it is found to consist of a mixture of a higher oxide of manganese with manganous ferrocyanide; it yields by boiling with sodium hydrate, a black oxide of manganese and sodium ferrocyanide. The oxide of manganese thus obtained is entirely soluble in hot dilute HCl, with evolution of chlorine. There is a process described in the last edition of Fresenius where manganese is estimated by boiling the solution, in presence of iron, with potash and potassium ferricyanide, and then estimating the resultant ferrocyanide by standard permanganate.

I have employed the reaction of ferricyanide with an ammoniacal solution of manganese for the quantitative estimation of the metal, but do not think it possesses any advantage over the precipitation by bromine, if it be as good.

Manganese kept in solution by means of tartaric acid gives a similar reaction to the ammonium chloride solution, but the precipitation does not occur completely until the liquid is boiled.

In my practice it is frequently required to estimate the *real* manganese present in manganese ores—not the chlorine producing power—and also in spiegeleisens, &c., and this is often a tedious process, as the iron has always to be first separated, and the manganese requires to be very accurately determined.

In the above reaction there seemed a means of throwing down the manganese completely even in presence of iron, without the latter metal going with it, and with this view I mixed manganous sulphate with ferric chloride, added ammonium chloride, tartaric acid, and excess of ammonia, and then potassium ferricyanide. The liquid was then boiled for some time, but without any precipitate being formed, so the presence of iron is apparently fatal to the process. With a smaller proportion of iron the manganese was partially precipitated, and the use of soda or potash instead of ammonia succeeded no better.

On treating a solution of zinc sulphate with excess of ammonium chloride and ammonia, and adding potassium ferricyanide, no change occurs either in the cold or on boiling, but if the heat be continued *very* long a slight white turbidity is occasioned, probably in consequence of the formation of a small quantity of ferrocyanide. On adding potassium ferrocyanide to either the hot or cold liquid, the zinc is thrown down as white zinc ferrocyanide, which is a far better and more delicate method of detecting it than the precipitation as sulphide.

Thus either one of the four metals, nickel, cobalt, manganese, or zinc can be readily distinguished from the others by adding first ammonium chloride and excess of ammonia, and then excess of potassium ferricyanide, when one of the following reactions will take place:—

(1). A brown precipitate = manganese.
(2). A deep red solution = cobalt.
(3). No change in the cold – a copper red precipitate on boiling = nickel.
(4). No change in either hot or cold liquid. Add potassium ferrocyanide – white precipitate = zinc.

As these four metals frequently occur in the same analytical "group," and in presence of ammoniacal salts, the above method of distinguishing them will, I think, be found useful, the only objection being that the explanations of the reactions are somewhat difficult, but I propose to study this question further.

I have not tried the reactions in the presence of any other metal except copper, which does *not* interfere with the test for nickel, and only by the colour with the reaction for cobalt.

The presence of manganese or zinc interferes with the tests for nickel and cobalt, which may prevent the reaction being used for the estimation of nickel in German silver, but I am still examining this question.

ON THE CAPABILITY OF CERTAIN SULPHIDES TO FORM THE NEGATIVE POLE OF A GALVANIC CIRCUIT OR BATTERY.*

By W. SKEY,
Analyst to the Geological Survey of New Zealand.

When a piece of massive galena is placed in voltaic contact with amalgamated zinc, and immersed along with it in weak sulphuric acid, to within an inch or two of the point of contact, a galvanic current is at once established, gas in quantity being given off at the surfaces of the galena, while the zinc is rapidly oxidised.

Three or four such pairs when connected among themselves, intensity fashion, afford a current of electricity strong enough to decompose acidulated water, and manifest all the phenomena of a small galvanic battery. Such an arrangement may properly be termed a *pyritous battery*—in accordance with the custom hitherto observed, of designating new forms of batteries after some distinguishing or novel feature in them.

Several other metallic sulphides manifest similar phenomena when coupled in this manner with zinc, amongst which are the following:—Zinc blende, copper pyrites, vitreous copper stibnite, proto-sulphide of iron, and the sulphides of silver, mercury, platinum, and gold.

The only sulphide I have yet found any difficulty with in setting up this action, under these conditions with zinc, is mundic, or the bi-sulphide of iron; but if the point of contact between it and the zinc is submerged in the acid, gas is evolved at this point, and the area of evolution rapidly spreads around from this, until the whole surface of the specimen becomes active.

* Read before the Wellington Philosophical Society.

The gas given off from the surfaces of these sulphides in the foregoing experiments was *sulphuretted hydrogen*. The effects, therefore, upon these sulphides when thus made to form the negative pole of a galvanic pair, is to desulphurise them; in some cases the ultimate effect is to reduce the metal to the metallic state. At any rate this obtains for the sulphides of mercury, lead, silver, platinum, and gold. With common yellow copper pyrites, a beautiful iridescence is communicated to it in a few seconds after the liberation of gas commences, owing, of course, to desulphurisation.

Saline solutions appear to produce the same effects in these instances as sulphuric acid, but they take place much slower.

The fact is thus directly established, that several of the metallic sulphides are capable of performing all the functions of the negative pole of a galvanic pair. From this, and the manner in which these sulphides have been connected with each other, it is clearly demonstrated that they are pretty good conductors of electricity. To a certain extent, indeed, all bodies are conductors of electricity, the terms conductor and non-conductor being only expressive, as Faraday affirms, "of extreme degrees of one common condition," there being no complete conductor, nor any absolute non-conductor; but these results show I think, these sulphides are conductors to a degree not before recognised—a circumstance which renders a comparison of their conducting power with that of other conductors necessary.

Indirectly it has appeared, that nascent hydrogen is able to decompose these sulphides at common temperatures, by combining directly with their sulphur, thus accomplishing at a low temperature that which would require a very high one, in case it (the hydrogen) were presented to the sulphide in its ordinary form.

In relation to the amalgamating processes used for the extraction of gold at our batteries, these results prove that zinc amalgam, in contact with acid solution, has precisely the same effect in decomposing the sulphides of gold, mercury, iron, &c., as sodium amalgam; like this amalgam, therefore, it keeps perfectly bright and mobile in presence of sulphides, or the products of their natural metamorphoses.

Whether or no such an amalgam could ever be profitably substituted for sodium amalgam in our gold batteries has yet to be determined. I am afraid that the continual addition of sulphuric acid to the water conveying the auriferous stuff to the plates, &c., which the use of this amalgam necessitates, would increase so much the cost of extraction as to render it unprofitable; still, as the water need only be very slightly acidified, it may be well to keep the fact thus arrived at in mind, when projecting intended improvements in the amalgamation of the gold from auriferous reefs.

In conclusion, I will only now remark, that the kind of phenomena just described appear to have some relation to the formation and decomposition of metalliferous lodes.

It is pretty certain, analogically considered,* that these sulphides should be able to form among themselves a series of voltaic pairs in presence of saline solutions, as they differ from each other in respect to their affinities for oxygen. Galena and copper pyrites, for instance, should form a voltaic pair, in which the galena would be the negative element; sulphide of silver and galena again should furnish another pair, in which the galena would have its function reversed, and so on for the rest, according to their relative proneness to change.

In a natural way, therefore, the contact of dissimilar sulphides generally should set up galvanic action and chemical decomposition. and by setting up this action, we might have a sulphide decomposed by saline solutions, which it would be able to resist if it stood alone; or, on the other hand, we might have an easily decomposible sulphide preserved by the association with it of one still more ready to decompose.

* Since determined to be the case.

Since the results just detailed were arrived at, I have been referred by Dr. Hector to a paper by Mr. Robert Hunt, entitled, "Researches on the Influence of Magnetism and Voltaic Electricity on Crystallisation and Conditions of Matter," given in "Memoirs of Geological Survey of Great Britain," vol. i.

The subject of that paper is similar in part to this under consideration, but I do not see that the author has anticipated any portion of the results stated here; he certainly does not demonstrate the actual and continuous production of electricity by the contact of sulphides with positive bodies in saline solutions; nor does he show that copper pyrites conducts electricity, the water-line being, as you will observe by reference to his diagram 14, page 457, above the point of contact between the pyrites and the battery, so that the change or decomposition of the ore need not involve the necessity of conducting power in the pyrites, as in the case of that connected with the positive end of the battery, the conducting power necessary for this decomposition might well have progressed around, from this point or line of contact by the liberation of copper; while, in case of the other piece of pyrites, all the conducting power necessary for the production of the phenomena described may, with propriety, be referred to the wire bound around it.

I would also state, that in repeating this experiment of Mr. Hunt's I find that, different to his own observations as stated, both the pieces of pyrites are chemically affected, while it is not that in contact with the copper of the battery which displays such marked iridescence, but that communicating with the zinc; and it passes into this state, not by an oxidation process, but by a desulphurising one, brought about by the liberation of hydrogen upon its surfaces; this gas, when freshly liberated, having a desulphurising effect upon sulphides generally, as I have clearly shown above.

PRELIMINARY NOTICE ON THE AMIDO-DERIVATIVES OF ORCIN.

By JOHN STENHOUSE, LL.D., F.R.S.

In a paper I published on March 1st, 1871,* I mentioned that I had made some experiments on the action of reducing agents on trinitro-orcinic acid and trinitro-resorcinic acid, but as Dr. Schreder has recently announced† that trinitro-resorcinic acid is identical with oxypicric or styphnic acid, and has investigated the amido-derivatives of it, I have not pursued my investigations farther in that direction, but have confined them to trinitro-orcinic acid.

Triamido-orcin, $C_7H_5(NH_2)_3O_2$.—When trinitro-orcinic acid is treated with reducing agents the NO_2 is replaced by amidogen, giving rise to amido-compounds. For the production of the triamido-orcin I find sodium amalgam to be the best reagent; as when nitro-orcin is agitated with water and sodium amalgam it rapidly becomes brown and ultimately colourless, giving rise to the triamido-orcin. The same substance is formed when trinitro-orcin is boiled with tin and hydrochloric acid, or with zinc and dilute hydrochloric acid. Intermediate amido-compounds appear to be formed, but I have not yet succeeded in isolating them.

Amido-diimido-orcin, $C_7H_5(NH_2)(NH)_2O_2$.—This substance is formed by the oxidation of triamido-orcin. When the colourless alkaline solution of triamido-orcin, obtained by treating nitro-orcin with sodium amalgam in the way already mentioned, is exposed to the air, it rapidly absorbs oxygen and acquires a magnificent blue colour, due to the formation of amido-diimido-orcin. The addition of an excess of sulphuric acid to this solution precipitates the

* *Poc. Roy. Soc.*, vol. xix., p. 410. Preliminary notice, Chemical News, vol. xxii., p. 98.

† *Ann. Chem, Pharm.*, vol. clviii., p. 244.

amido-diimido-orcin sulphate in dark purple brilliant scales, which can be obtained of great beauty by re-crystallisation from water, in which they are but slightly soluble. The addition of an acid renders them still more insoluble. By substituting hydrochloric acid for sulphuric acid in precipitating the blue solution the *amido-diimido-orcin hydrochlorate* is obtained in thin silky needles of a dark brown colour, which are much more soluble in water than the corresponding sulphate. The *nitrate* forms purple scales much resembling the sulphate, but more soluble, and is readily decomposed by excess of nitric acid. The *oxalate* is but slightly soluble, and crystallises in purple scales. The *picrate* forms iridescent green needles and plates, which are but slightly soluble. The *acetate* is much more soluble. The base itself, *amido-diimido-orcin*, can be obtained by treating the hydrochlorate with ammonia, or by exactly neutralising the blue alkaline solution before mentioned with hydrochloric acid, when the base separates in opaque needles having a green iridescent lustre. The base is but slightly soluble in dilute ammonia, and almost insoluble in water, but it dissolves readily in solution of sodic hydrate, with a fine deep blue colour, which gradually disappears on boiling, ammonia being evolved at the same time.

Nitric acid acts readily upon amido-diimido-orcin, forming an acid which gives crystalline salts the nature of which I have not yet determined.

A NEW METHOD FOR THE DETERMINATION OF AVAILABLE CHLORINE IN "BLEACHING POWDER."

By J. B. F. HERRESHOFF,
Assistant in Brown University Laboratory, Providence, R.I.

Having had occasion to estimate the available chlorine in several samples of bleaching powder, and finding the methods now in general use inconvenient, and, unless executed with the utmost care, liable to much inaccuracy, I have of late devised a method which for accuracy, ease of execution, convenience, and economy of time, surpasses those in present use.

It is based upon the fact that when "bleaching powder" is made to act on an excess of stannous chloride in strongly acid solution, stannic chloride is formed at the expense of all available chlorine in the former. The reaction is represented by the following:—

$$[CaCl_2O_2+CaCl_2]+4HCl+2SnCl_2=2CaCl_2+2H_2O+2SnCl_4.$$

Any excess of stannous chloride now remaining in solution is estimated by means of a standard solution of potassic dichromate, and, deducted from the former amount used, gives the weight of stannous chloride changed into stannic chloride by the "bleaching powder" used. The following represents the reaction:—

$$3SnCl_2+K_2Cr_2O_7+14HCl=3SnCl_4+2KCl+Cr_2Cl_6+7H_2O.$$

The process is conducted in the following manner:—

Weigh accurately 13·8738 grms. of pure potassic dichromate, dissolve in water, and dilute the solution to one litre. Each cubic centimetre of this solution has oxidising power equivalent to 0·01 milligrm. of chlorine.

Weigh about 30 grms. of stannous chloride and dissolve in water, to which chlorhydric acid is freely added, and dilute the solution to one litre.

Fill two burettes with these solutions, run off about 10 c.c. of the stannous solution, dilute somewhat with water, and add about 5 c.c. of chlorhydric acid and a few drops of solution of starch, and potassic iodide. Now run in the solution of potassic dichromate, until a last drop gives to the solution a blue colour.

If it requires 20 c.c. of the standard dichromate solution, then 1 c.c. of the stannous solution will equal 2 c.c. of the former, and also 0·02 milligrm. of chlorine.

Again, supposing the tin solution to be of the above strength, measure off 20 c.c. of the now standardised tin solution, dilute with water, as before, and add 1 grm. of the "bleaching powder" in question, well triturated with water, and then 10 or 12 c.c. of chlorhydric acid.

The reaction is energetic and complete, and, if sufficient amount of chlorhydric is introduced, the solution will become perfectly clear.

Add a few drops of solution of starch and potassic iodide, and then run in the potassic dichromate solution until the deep blue colour of iodide of starch remains permanent. Supposing 10 c.c. are required, then 10 c.c. ÷ 2 c.c. = 5 c.c.; 20 c.c. − 5 c.c. = 15 c.c. As 1 c.c. of the stannous solution equals 0·02 Cl and 15 c.c. are required for 1 grm. of the "bleaching powder," then $15 \times 0{\cdot}02Cl = 0{\cdot}30Cl$, or 30 per cent.

Penot's arsenic method, the one now in general use, has disadvantages.

Arsenious acid is with difficulty purified, and there is no convenient method by which to test its purity.

The standard solution of sodic arsenite is tedious to prepare, from the fact that it requires a long time for complete solution of the required amount of arsenic in sodic carbonate solution.

This solution will not keep with certainty for any length of time unchanged, and is newly standardised only with difficulty, which arises from the fact that trouble is experienced in preparing perfectly pure iodine, caused mainly by its property of retaining moisture, and its volatility. This latter property is a source of much injury to the balance on which it is weighed, unless conducted in a closed vessel.

If the solution of iodine be of the correct standard no trouble is experienced in accurately standardising the arsenious solution, provided the iodine solution be run into the latter, in which starch and an excess of sodic carbonate is present.

One great advantage in favour of the new method is that the standard potassic dichromate solution will remain for any length of time unchanged, and although the tin solution undergoes change, yet it is a matter of only a few moments to accurately re-standardise it by means of the dichromate solution.

The correctness of the results obtained by this method will be seen to depend upon the purity of potassic dichromate—a substance, though not always to be found of perfect purity, yet the impurities, which consist mainly of potassic sulphate, are so small as not to materially affect the final results.

This method gives accurately corresponding results. In four analyses made on a sample of "bleaching powder," the following results were obtained:—

	Cl.
1	26·42 per cent.
2	26·50 ,, ,,
3	26·56 ,, ,,
4	26·48 ,, ,,

—*American Chemist.*

ON THE ACTION OF SULPHUROUS ACID ON METALS.

By P. SCHWEITZER, Ph.D.

Zinc and Sulphurous Acid.—If we dissolve metallic zinc by means of a mineral acid, and add a few drops of a solution of sulphurous acid, we observe, according to the strength of the sulphurous and hydrochloric acid per cent, a more or less rapid separation of sulphur and development of the odour of sulphuretted hydrogen gas. This is a reaction we resort to daily in our laboratories for the detection of sulphurous acid in hydrochloric, acetic, and other acids, and is so delicate a test for the presence of

this impurity, that the minutest traces of it may be discovered in this way without difficulty.

The reaction according to which this decomposition takes place is exceedingly complicated, and we look in vain to any of our text-books for information. Many false statements, too, will be encountered on the subject, which only a close study will reveal as such. In the following will be found the results of some experiments, undertaken with a view of settling this question :—

A strong solution of sulphurous acid was prepared, so as to be perfectly free from sulphuric acid, sulphuretted hydrogen gas, free sulphur, and oxygen, which, by the way, requires a great deal of care. In this acid, care being taken to exclude all atmospheric air, pure zinc was dissolved. No generation of gas could be observed, and the fluid, after deepening gradually in colour so as to become at last quite brown, like old sulphide of ammonium, commenced to separate sulphur, regaining its original colourless state after some time.*

While brown, and before sulphur had commenced to separate, part of the fluid was saturated with caustic potassa and tested for sulphur (as sulphide of potassium with nitroprusside of sodium), which was proved to be absent. After repeatedly shaking the fluid, which had become milky from separated sulphur, the solution itself being quite colourless, and allowing it to stand for several hours, a small amount of it was taken and tested in several ways. It contained still an excess of sulphurous acid, which, I may mention here, does not disappear even after standing for several weeks with metallic zinc, owing to the zinc becoming covered with a coating of sulphide of zinc, a trifling amount of which is always formed in this operation ; part of it may be seen floating about in the liquid. Upon acidulating a part of the solution in question with hydrochloric acid and heating, sulphur separated in large quantities and of a yellow colour, while sulphurous acid was given off; another portion was acidulated with the same acid and left standing; in a few minutes sulphur separated, increasing in quantity while standing, and sulphurous acid being given off again. This established the presence of hyposulphurous acid, as no other known acid of sulphur is decomposed in the cold by a mineral acid into sulphur and sulphurous acid. Another portion of the fluid was then taken, diluted with water, acidulated with hydrochloric acid, and chloride of barium added. No precipitate whatever formed, proving the absence of sulphuric acid. This test has to be done quite rapidly, as after a few seconds sulphur begins to be separated, recognisable when in small quantity by the peculiar opalescent appearance it presents. A third portion of the fluid was acidulated with hydrochloric acid, boiled so as to decompose all polysulphur acids, which in the case of hyposulphurous acid cannot be accomplished in less time than two or three weeks. As the precipitated sulphur does not settle very quickly, the fluid was shaken with a small piece of bright metallic copper which rendered it clear almost immediately, so that it could be filtered and tested for sulphuric acid. A heavy precipitate was obtained with chloride of barium, proving the presence of this acid in large quantity and beyond doubt. This sulphuric acid, though not present in the original fluid, was produced therefore by the decomposition of a sulphur acid, and judging from the synthesis of these acids, this must have been trithionic acid.

$$2S_2O_2+3SO_2=2S_3O_5+S \text{ (preparation).}$$
$$S_3O_5=SO_3+SO_2+S \text{ (decomposition).}$$

We have therefore present in the fluid, after acting with sulphurous acid on metallic zinc, SO_2, S_2O_2, S_3O_5, S, ZnO, ZnS, a trace. I may mention here that a solution of hyposulphite of soda treated with sulphurous acid turns, especially upon heating, first yellow, then brown, sulphur separates, and trithionic acid is formed. The change of colour in the original experiments therefore indicates the point when trithionic acid is beginning to be formed. The reaction may be represented in the following way :—

(1). $2Zn+3O_2=ZnO,SO_2+ZnO,S_2O_2$.
(2). $2S_2O_2+3SO_2=2S_3O_5+S$.
(3). $SO_2+S=S_2O_2$.
(4). $5SO_2+S=2S_3O_5$.

The result being $ZnO.SO_2$; $ZnO.S_2O_2$; $ZnO.S_3O_5$; besides sulphur, sulphide of zinc, and the undissolved metal. As regards the reaction under 3, I am perfectly aware that sulphur under ordinary conditions does unite with sulphurous acid, but if separated in the fluids containing it, its more active state may very likely favour such combination ; the reaction under 4 is a fact, as it is a method for the preparation of S_3O_5.

This fluid does not contain tetra- or penta-thionic acid, for on saturating it with caustic potassa and dissolving by an excess of alkali the first precipitated oxide of zinc, no sulphur separated to form sulphide of potassium on boiling. Both of these acids are decomposed, by boiling with alkalies, into sulphur and trithionic acid ; this latter being decomposed in its turn again, but very much slower, into sulphurous and hyposulphurous acids.

Upon treating now sulphurous acid with metallic zinc in the presence of hydrochloric acid, we obtain a milky liquid, and the odour of sulphuretted hydrogen becomes more or less perceptible. The reaction here is more rapid and complicated. Zinc and hydrochloric acid generate hydrogen, as is well known, and the question may arise whether the hydrogen, being in the nascent state, will directly reduce sulphurous acid to water and sulphuretted hydrogen gas. Many metals decompose water, oxide of the metal being formed, while hydrogen is set free. On filling a little flask with sulphurous acid solution, inverting it and letting its mouth dip under sulphurous acid, contained in a porcelain dish, and introducing into the flask a piece of sodium, a vivid separation of hydrogen took place, while the sulphurous acid remained clear, and no trace of hydrosulphuric acid could be detected in the escaping gas. The same was observed when a piece of aluminium was substituted, in which case the fluid had to be heated to accelerate decomposition. The nascent hydrogen here was therefore without action upon the sulphurous acid, and it is not likely that, in the case of zinc and hydrochloric acid, the hydrogen should reduce sulphurous acid to water and sulphuretted hydrogen gas.

However, another experiment was tried, the result of which presents a still stronger reason for not considering hydrogen the reducing agent of sulphurous acid. In treating sulphurous acid with zinc and hydrochloric acid in the cold, until the reaction ceased, freeing the sulphur from the liquid in the way indicated, and adding to it more hydrochloric acid, and heating, a precipitate of sulphur was obtained, indicative of the presence of a polysulphur acid ; but as no sulphuric acid could be found in the fluid, this must have been hyposulphurous acid. In order to succeed in this experiment much sulphurous and little hydrochloric acid must be taken; the separated sulphur there being also yellow. As therefore nascent hydrogen was without action upon sulphurous acid, but hyposulphurous acid and sulphur were separated, the probability is that sulphuretted hydrogen gas is formed by the direct combination of sulphur with hydrogen. In the moment of separation of both hydrogen as well as sulphur, we obtain all hydrogen and all sulphur as sulphuretted

* The few bubbles of gas escaping at the beginning are very probably carbonic acid. Zinc, like many other metals, oxidises in moist air, becoming thereby converted into basic carbonate. A stick of zinc, which had been cleaned beforehand with a file, so as to present a pure metallic surface, did not show the phenomenon of generating gas. Ordinary granulated zinc (which is always coated with a whitish oxycarbonate) shaken with water imparts to it an alkaline reaction, on account of the carbonate dissolving to a perceptible extent, and which may be easily tested with solution of cochineal. The amount of carbonic acid which all metals absorb, when exposed to the oxidising influence of moist air, is accounted for by the fact that it dissolves to a much larger extent in moist air than would correspond to 1·25 of a per cent, and that it very likely exists in this solution as carbonic acid and not as anhydride.

l ydrogen ; so in the case of treating sulphide of sodium with an acid.

$$Fe+HCl=FeCl+H.$$
$$FeS+HCl=FeCl+HS.$$
$$Fe+HSO_4=FeSO_4+H.$$
$$FeS+HSO_4=FeSO_4+HS.$$

But even one of these two elements, while in the nascent state, will combine with the other. We know that nascent hydrogen will combine directly with phosphorus, forming phosphuretted hydrogen ; and, though sulphur, presenting so many strange and peculiar anomalies in its different states, may not unite as flowers of sulphur with hydrogen, it is very likely that sulphur, just separated from the decomposing hyposulphurous acid, will form sulphuretted gas, with nascent hydrogen. The sulphur certainly must have retained in this case some active principle, as it will directly combine upon shaking with metallic copper, and, as we will see hereafter, readily with metallic cadmium. Besides, sulphuretted hydrogen gas is only perceived in this reaction after sulphur has commenced to separate.

Cadmium and Sulphurous Acid.—In treating a piece of metallic cadmium, which had been rolled out to a very thin and long strip, with the same sulphurous acid, the reaction took place in a similar way as that with zinc. The fluid turned first yellow, but still remained clear, which required, however, a much longer time than with zinc, then darkened, separated a little white sulphur, and precipitated finally a large amount of sulphide of cadmium. This is insoluble in weak sulphurous acid, and therefore precipitated, while sulphide of zinc, being soluble, is consequently prevented from forming. We have a clear case here of separating sulphur combining directly with a metal. But this separating sulphur seems to have even the power of depriving salts like sulphide or hyposulphite of cadmium of its metal, forming thereby sulphide of cadmium and sulphuric acid. For while in the liquid resulting from the action of sulphurous acid upon cadmium, and which has been in contact with the metal for only three or four hours, no sulphuric acid can be detected before boiling, its quantity will be perceptible after standing in contact three or four days, and will increase by standing several weeks. This sulphuric acid derives its origin from the oxidation of the sulphurous acid by the oxygen of the metallic oxide, which is decomposed by the separating sulphur, The experiment can be easily made by adding a solution of either nitrate or sulphate of cadmium to hyposulphite of soda. Upon boiling, a slight opalescence only will be observed, as cadmium salts are generally acid, but no precipitate of either sulphur or sulphide of cadmium. Upon adding, however, a little hydrochloric acid, a heavy precipitate of sulphide of cadmium will be obtained immediately. The sulphur, therefore, from the decomposition of the hyposulphurous acid, was decidedly active.

The original fluid treated in the same way, viz., boiling with a little hydrochloric acid, immediately gave a precipitate of sulphide of cadmium. Before, however, any marked precipitate of CdS was observed, and after the fluid had remained in contact with the cadmium for about four hours, no sulphuric acid was found to be present, as has been already mentioned. Upon acidulating and boiling this solution, filtering out the separated sulphur, and testing the filtrate, much sulphuric acid was found, originating, as in the case of zinc, from the decomposition of S_3O_5. The total reaction may be thus expressed :—

$$2Cd+3SO_2=CdO.SO_2+CdO.S_2O_2.$$
$$2S_2O_2+3SO_2=2S_3O_5+S.$$
$$(SO_2+S=S_2O_2).$$
$$(5SO_2+S=2S_3O_5).$$
$$Cd+S=CdS.$$
$$CdO.SO_2+S=CdS+SO_3.$$

It is hardly necessary to say that the sulphuric acid will combine with oxide of cadmium, liberating either SO_2 or S_2O_2. The fluid, after a sufficient length of time, will therefore contain CdO,SO_3 ; CdO,SO_2 ; CdO,S_2O_3 ; CdO,S_3O_5 ; CdS ; and the undissolved metal. All sulphur will have disappeared, and entered into combination with cadmium, so that a portion of the fluid, turbid and yellow from suspended sulphide of cadmium, will become clear upon acidulating with hydrochloric acid ; no odour of sulphuretted hydrogen will make its appearance, as it is decomposed by the sulphurous acid. After filtering and washing, the precipitate, may, however, be readily recognised as CdS.

Nickel and Sulphurous Acid.—Nickel in form of the well-known cubes was treated with sulphurous acid in a bottle ; the action proceeded similarly to that of the two former metals, but was very much slower even than that of cadmium. After several hours a darkening and green colouration of the fluid occurred, but a day elapsed before a separation of sulphur took place, and then only small specks of it were seen floating about. In this stage of the operation the fluid contained NiO,SO_2; NiO,S_2O_2 ; some sulphur, but no S_3O_5 or SO_3. After several weeks' standing all sulphur had disappeared and combined with nickel as NiS, which in this case was certainly formed directly from the metal, as it presented the appearance of a black, heavy, scaly mass ; this mass amounted to a considerable quantity, much more than could have formed from the little precipitated sulphur, and as much sulphuric acid was then to be found in the solution, we must conclude that either hyposulphite of nickel is not decomposed by sulphurous acid like the cadmium or zinc salt, but, under the influence and in contact with the metal and sulphurous acid, is converted into sulphuric acid, separating thereby sulphur, which combines with the nickel, thus :—

$$2(NiO.S_2O_2)+SO_2=2(NiO.SO_3)+3S ;$$

or we have to admit a secondary reaction of the sulphurous acid upon the metallic nickel, thus :—

$$2Ni+2(SO_2)=NiO.SO_3+NiS.$$

I should decidedly favour the former view, which does not appear to be so very strange, if we remember that sulphurous and hyposulphurous acids are decomposed differently when combined with different oxides. I only recall the preparation of thithionic acid either from bisulphite of potassa with flowers of sulphur, or from hyposulphite of potassa by sulphurous acid. We do not get trithionic acid if we take in these cases soda instead of potassa salts.

However, we have in the fluid, after allowing the reaction to proceed for a considerable length of time, NiO,SO_3 ; NiO,SO_2 ; NiO,S_2O_2 ; NiS.

Aluminium and Magnesium with Sulphurous Acid.—These two metals dissolve readily, the first in cold, the second in hot sulphurous acid. No sulphur is separated in the fluid, and no sulphuretted hydrogen gas is observed, as has been already mentioned. But upon analysing the clear solution, we find in both cases SO_2 ; SO_3 ; S_3O_5 ; S_2O_2.

The presence of SO_3 and S_3O_5, and the non-appearance of S, forces us to admit two or three reactions taking place simultaneously and independently of each other. The first, the solution of the metal by decomposing water, liberating hydrogen, and dissolving as oxide in the sulphurous acid ; the second, the action of sulphurous acid upon the metal by forming a sulphide and hyposulphite ; and the third, by forming a sulphate, hyposulphite, and trithionate.

(1). $Mg+HO+SO_2=MgO.SO_2+H.$
(2). $2Mg+3SO_2=MgO.SO_2+MgO.S_2O_2.$
(3). $4Mg+8SO_2=MgO.SO_3+MgO.S_3O_5+2(MgO.S_2O_2).$

These reactions seem to take place according to concentration, and depending in general upon conditions, which a future and closer study may reveal, and in the investigation of which, I hope, other chemists will participate.—*American Chemist.*

ON COLOURS FROM COAL-TAR.*

By HARRY N. DRAPER, F.G.S.

A few evenings since, when I was thinking how I might best place before you this subject of Colours from Coal-Tar in an instructive, and at the same time, if possible, an interesting manner, I chanced, in a very charming book, upon a passage which, with your permission, I will read to you.

"Yesterday forenoon, writes a friend, I went to the South Kensington Museum. It is really an absurd collection—a great deal of valuable material, and a great deal of perfect rubbish. The analyses are even worse than I was led to suppose. There is an *analysis of a man*. First a man contains so much albumen, and there is the albumen; so much phosphate of lime, fat, hæmatin, fibrine, salt, &c., &c.; then in the next case, so much carbon, so much phosphorus, a bottle with sticks of phosphorus, so much potassium; and there is a bottle with potassium, calcium, &c. They have not bottles of oxygen, hydrogen, and chlorine, but they have cubical pieces of wood on which is written, 'The quantity of oxygen in the human body would occupy the space of 170 cubes of the size of this.' What earthly good can this do any one?"—Brown, *Horæ Subsecivæ*.

Now, when I tell you that, did I not stand in such close proximity to the admirable museum of this Society, and in awe of the unfavourable opinion of some friends of mine whom I see here, and whose opinion I respect, I should be much disposed to say "that, very little good indeed;" you will understand that it is not without diffidence that I ask your attention to the somewhat formidable array of specimens which you see upon the table, or to the diagrams which hang against the wall. It is not that I have large sympathies with those, who, thinking the best part of a scientific lecture to be the experimental one, are ever on the alert for brilliant coruscations of light, and for explosions, but that I realise most fully the great difficulty of conveying anything like an accurate idea of complex chemical changes, with the opportunities and within the limits of a single lecture; and when we have, too, in the same hour, to occupy ourselves with the practical results of an important industry like this. However, as I have introduced the diagrams to your notice, it is but fair that I should be their apologist; and while, I think, I may say that they need not be a cause of uneasiness to us, I hope that when (as is but too probable) I come to use hard words, they will be a positive help, and give us some notion of what the hard words mean. And, as regards the specimens, I think when I say that each of them represents a fact, and often a very beautiful fact in chemistry, that each one of them has its own peculiar application in the arts or manufactures, and that they are, without exception, products of that wonderful fossil flora which we call Coal, you may be disposed to view them indulgently. I shall hope, too, to give them new interest in your eyes as we proceed; and it will be well if, at the outset, we get a clear idea of the object before us. We propose to trace the *useful*, as represented by this black, unsightly coal, in its progress towards the *beautiful*, as exemplified by the brilliant dyes with which we have been familiar during the past fourteen years. The mind, even when accustomed to chemical changes, and trained by experience to expect what would otherwise be startling results, does not easily grasp the conception that the millions of tons of coal which, in these islands, await the hands of the miner, represent a vast magazine, not only of light and heat, but of colour-giving substances, which vie in splendour with the tints of the most gorgeous exotics or the varied plumage of tropical birds.

The connection of colour with light is inseparable. We cannot imagine its independent existence. It is not only when, as you have seen in some of the earlier lectures of this course, a ray of light is intercepted by a prism, that its velocity is altered, and that magnificent spectrum whose tints never weary us extends itself upon the screen; but nearly all bodies in nature split up white light, and in effect these differ only from prisms in that they absorb some rays and reflect others. That this is so we know; but though we may scarcely adopt the fanciful hypothesis of Darwin, whose "Ethereal powers"

> "Cling round the aërial bow, with prisms bright,
> And, pleased, untwist the sevenfold threads of light;
> Eve's silken couch with gorgeous tints adorn,
> And fire the arrowy throne of rising morn,"

we really know little more than we might thus express, and cannot predict, either from the shape, physical condition, or chemical constitution of a body, whether it shall appear to our eyes red, yellow, or blue. But grant white light and anything which will absorb a part of it, and you have colour. But does light, upon which so marked a change is effected when this partial absorption takes place, itself effect no change when it is not partially but wholly absorbed by the bodies upon which it falls? All nature gives an affirmative reply. The snow-drop will flourish in the most secluded corner of the garden, and the wood-sorrel, we know, puts forth its pale blossoms in the shadiest dell; but it is in the full blaze of the tropical sunlight that cactus and aloë assume their vivid tints and the orchis takes its rainbow hues. Shall we, then, allow imagination too much rein if we think that centuries ago, when that which is now a coal-field was a waving forest, the light drunk in by every leaf did something more than clothe the copse in sombre green. May we not suppose that just as a logwood tree or an indigo plant of to-day returns in colouring matter the work done by the light it absorbs, these dark tracts of pines no less stored up their colour-giving substances but withheld them for a future age. And if we turn from the domain of fancy to that of fact, we see, as has been well pointed out by Hofmann, how much the practical science of to-day tends to turn to the vast storehouses of raw material which the mineral kingdom affords, and to form from inorganic bodies the most rare and costly products of organic life. Already, as he says, we have found the potash salts, which were hitherto obtained only from plants, in that great magazine of potash, the ocean; and paraffin and allied fatty bodies obtained from coal have already supplanted, to a very large extent, the fats and oils for which we were accustomed to look to animal and vegetable sources alone. So, in the same way, we have now some other things which, as flavours and perfumes, appeal to our senses of taste and smell, derived from mineral sources. It is not, then, after all, so very strange that we should seek and find *colour* in *coal*.

"Coal and Iron," it has been well said, "are kings of the earth;" and as we traverse to-day the desert of hard names and abstract fact which separates coal-tar from colour—a desert by the way in which there are a few pleasant oases—we shall do well to remember how much wealth from this source alone lies stored up in the coal-fields of Great Britain; and, perhaps, there are few cases we could select in which the contrast between the value of two substances is greater than it is between coal-tar and say, for example, purple dye; the first a fit type, if not of complete uselessness, at least of offensiveness; and the last so prized that a pound of wool dyed with the Tyrian purple, which, as we know, was obtained from a shell fish—the *murex*—sold at the time when Augustus reigned in Rome for a sum equivalent to £30 of our money. As I have mentioned this, it is, perhaps, but fair to say, that the purple of Tyre had an advantage which, in some degrees, compensated for its enormous price. We read that, when the Greeks sacked the treasury of Darius, they found a quantity of cloth dyed with this murex purple, which had retained all its brilliancy of colour after the lapse of 100 years. It was, therefore, essentially what ladies call a "fast" colour. It may not be out of place

* A Lecture delivered before the Royal Dublin Society.

to note, that nearly all the dyes with which we are familiar are of comparatively recent introduction. We never hear of woad now, but this favourite dye of our barbaric ancestors was only replaced by indigo in the reign of Elizabeth. And now, in our own day, indigo is being slowly but surely supplanted by the dyes which form a part of the wonderful series of "Colours from Coal-Tar."

And now we will endeavour, if you please, to follow the progress of this industry. All who hear me know, that in the manufacture of gas, coal is distilled in large retorts, made either of fire-clay or iron, from each of which a vertical tube rises. A number of these retorts are arranged together in a furnace; the retorts are made red-hot, and distillation at once commences.

Both the gas and the oily products of the coal pass up a vertical pipe, and (as this bends on itself) down again into what is called the "hydraulic main." Here all which is not gas condenses, and as the condensed liquid accumulates, it flows into a tank prepared for it. Coke alone remains in the retort. The condensed liquid is *coal-tar*—the body which interests us to-day. Now, this coal tar is, by distillation at a lower temperature, itself separable into two bodies—*pitch* and *tar oil*. The pitch is, as we know, used for the formation of the artificial asphalt, employed for footways, and in the manufacture of coarse black paints or varnishes. In this and other ways all the pitch which it was worth while to make found an outlet; but, because there was almost no other use for the tar oil than "creosoting" timber, it was almost without value. A glance at the diagram which we have here will show us that coal-tar oil is by no means a simple body. Hofmann, indeed, very properly speaks of it as "one of the most wonderful productions in the whole range of modern chemistry." We must not forget that not one of all these substances tabulated here exists *as such* in the coal itself. Indeed, if we were not aware what remarkable changes the simple application of heat is capable of making in most bodies, we should not be prepared for such a splitting up of what, at first sight, seems a very simple body indeed, into all these compounds. This action of heat sometimes shows itself in a very remarkable way. Here is a body: it is a compound of cyanogen and sulphur; and you see, that, as we have it here, it consists of small white lumps of a loosely-aggregated powder. Now, if I apply heat to this body, it changes its form in a very remarkable manner; and, having seen this change of form, you will readily understand that its chemical properties and chemical composition have also undergone complete change. So it is with coal: the mere application of heat, to a certain degree, not only changes its form, but resolves it into all these bodies, each of which has some important chemical difference from its neighbour.

Perhaps, if we only knew how to go to work, we should discover that there are very few things in nature from which we could not obtain colour. But the tendency of these component parts of coal-tar to form coloured compounds is very remarkable. I might show you this by more or less elaborate methods, with almost any one of them; but here is one of the simplest examples. I mix a few drops of this body, which, as we shall have more to say to it presently, I will not describe now. I mix it with the water in this jar, and then add a solution of a persalt of iron. You see we have a violet colouration at once developed. But there are *five* constituents of tar, in which the tendency to give colour is so very marked that all the interest of coal-tar, as a practicable source of colour, centres in them. These five bodies are benzol, toluol, phenol, napthalin, and anthracen; and as it is from these alone that the coal dyes are obtained, we have arranged them here on a separate diagram, in which each is shown, with the principal colours derived from it. You will see, also, specimens of each of these bodies on the table, and will observe some characteristic points of difference, even in their appearance.

Now, in separating these substances from coal-tar oil, advantage is taken, firstly, of differences of boiling point and, secondly, of the various behaviour of these bodies with acids and alkalies. Glancing at the diagram, we see, for instance, that *benzol* boils at 84°; *toluol* at 114°; and *phenol* at 188°; so that, if we subject a mixture of these three bodies to distillation, we shall obtain—first, the benzol; then, as the temperature increases, the toluol; and, finally, when the thermometer reaches 188°, the phenol.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Annual Meeting, April 18th, 1871.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Dr. Joule, F.R.S., drew attention to the remarkable atmospheric phenomenon which had been seen by several persons in Derbyshire and elsewhere, on the evening of Good Friday, April 7th, and stated that he had witnessed a similar appearance near Glasgow, on the day before it was observed in this neighbourhood. The perpendicular ray extended upwards from the sun to an altitude of 30°, and was very clearly defined. It was observed from half an hour before until after the sun had set. The phenomenon was also witnessed, at the same time, by Professor J. Thomson, who was sailing on the Firth of Clyde.

The following Report of the Council was read by one of the secretaries:—

The Council have the satisfaction to report that the past year has been one of steady progress for the Society. On the 31st of March, 1870, the general balance of the Treasurer's account was £268 1s. 2d., and on the 31st of March last it had increased to £287 19s. 1½d.

The number of ordinary members on the roll of the Society on the 1st of April, 1870, was 161, and 14 new members have been since elected. The losses during the year have been—Deaths, 1; resignations, 3; and defaulters 2. The number of the roll on the 1st of April instant was therefore 169.

In the last annual Report it was stated that in consequence of the rapid increase of the library of the Society it would be necessary to provide additional accommodation. This has since been done, a new bookcase having been fitted up in the Council room in place of a smaller one which has been removed into the tea room.

The Council refer with pleasure to the fact that the principal result of the recent solar eclipse expedition is due to the energy, intelligence, and skill of a member of the Society, Mr. Alfred Brothers, F.R.A.S., whose beautiful photograph of a solar corona is now generally regarded as having settled the long disputed question as to the real nature of this remarkable phenomenon.

The printing of the fourth volume of the Society's third series of Memoirs has been completed, and a new volume has been commenced.

During the session now ending an important alteration has been made in the terms of admission of Sectional Associates, the annual subscription having been reduced from £1 to 10s., except for those Associates who wish to make use of the Society's library, who will still continue to pay a subscription of £1 per annum.

The librarian reports that he has not been able to send any books to be bound in the course of the past year. The Society continues to receive the publications of those societies to whom our own "Memoirs" and "Proceedings" are sent, but the recent war between France and Germany has to some extent interrupted the transmission of the works of many continental societies.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:—

President—Edward William Binney, F.R.S., F.G.S.

Vice-Presidents—James Prescott Joule, D.C.L., LL.D., F.R.S., F.C.S., &c.; Edward Schunck, Ph.D., F.R.S., F.C.S.; Robert Angus Smith, Ph.D., F.R.S., F.C.S.; Rev. William Gaskell, M.A.

Secretaries—Henry Enfield Roscoe, B.A., Ph.D., F.R.S., F.C.S.; Joseph Baxendell, F.R.A.S.

Treasurer—Thomas Carrick.

Librarian—Charles Bailey.

Other Members of the Council—Peter Spence, F.C.S., M.S.A.; William Leeson Dickinson; Henry Wilde; Robert Dukinfield Darbishire, B.A., F.G.S.; Osborne Reynolds, M.A.; William Boyd Dawkins, M.A., F.R.S.

CORRESPONDENCE.

THE PHOSPHATE SEWAGE PROCESS.

To the Editor of the Chemical News.

Sir,—Some time since you were good enough to insert in the Chemical News a letter from myself, asking Dr. Voelcker a question regarding the analysis of the "dried sewage deposit" (prepared according to Dr. Forbes's "patent phosphate process for the utilisation of sewage,") which appeared in No. 589.

The deposit was found by Dr. Voelcker to contain 28·52 per cent of phosphoric acid; 29·95 per cent of alumina and oxide of iron, sufficient to combine with all the phosphoric acid present; also a quantity of lime, insufficient to combine with more than about half the phosphoric acid.

Being considerably interested in phosphatic manures, I was anxious to know how much of the phosphoric acid Dr. Voelcker would consider as being in combination with the alumina, and how much with the lime, for upon this agriculturalists and many manufacturers at present believe the value of the deposit as a manure to entirely depend, the more so because Dr. Voelcker, in his analysis, stated the phosphoric acid to be equal to a certain quantity of phosphate of lime, which, on first reading, may be taken to mean that he considers it to be really in combination with the lime, and not with the alumina and iron as one would suppose from the well known chemical affinity of the acid for these bases.

I looked forward to Dr. Voelcker's reply, in the hope of having my scruples set at rest; but not receiving one, I procured some native phosphate of alumina and made the following experiment. I dissolved the mineral in sulphuric acid, and to the clear saturated solution added milk of lime in slight excess. The precipitate was collected on a filter, well washed, and analysed. I found that the whole of the phosphoric acid which it contained was combined with alumina and a little oxide of iron, mere traces only being soluble in acetic acid, showing the almost complete absence of phosphate of lime.

Now, assuming that all the phosphoric acid present existed as the soluble phosphate of lime, the value of the deposit as a manure would not greatly exceed the value which Dr. Voelcker assigns to it, viz., £7 7s. per ton; but if (as I must infer from my experiment), nearly the whole of the phosphoric acid is combined with alumina and iron, of what value will such a manure be as a fertilising agent? If, indeed, phosphate of alumina be the excellent manure which Dr. Voelcker appears to consider it, from the value he attaches to this "sewage deposit," how is it that Messrs. Lawson and Son do not convert the native mineral phosphate directly into a manure by the ordinary method of treatment with acid, instead of doing so by the very roundabout plan known as Forbes's patent process?—I am, &c.,

Superphosphate.

Royal College of Chemistry, London,
June 20, 1871.

MISCELLANEOUS.

The Abbé Moigno.—As will be seen from our Chemical Notices from Foreign Sources, the Abbé Moigno has again resumed the publication of *Les Mondes*. Subscribers' names and subscriptions will be received at our office. The learned editor was unable to continue his duties during the disasters which attended and followed the late war. He has now, however, resumed them with renewed energy and with the determination to make the journal even more useful than before.

A New Brass.—The difficulty of uniting iron to brass is created by the unequal rate of expansion in the two metals, which destroys the unity when the temperature is changed. A new alloy of copper is announced, and the inventor claims that its expansion by heat is so similar to that of iron and steel, that the surfaces may be regarded, when joined, as permanently united, for all practical purposes. The formula (recently published in the *Journal of Applied Chemistry*) is as follows:—Tin, 3 parts; copper, 39½ parts; zinc, 7½ parts. Any of our readers who have occasion to join iron and brass, can easily try this new "metal," and we shall be glad to hear of the practical value of an idea of such high technical importance.—*Scientific American*.

London Institution.—The managers of the London Institution, in accordance with the recommendation of the annual meeting of proprietors, have resolved to afford opportunities during the ensuing season for the reading and discussion of communications on subjects of special interest in science, literature, commerce, and the arts, provided they receive such offers as will ensure an adequate succession of suitable papers. It is believed that this proposed extension of the use of the Lecture Theatre in Finsbury Circus will produce a series of attractive meetings similar in character to those of the Society of Arts, but representing more directly the business and thought of the city. The managers do not intend to restrict the reading and discussion of papers to the proprietors of the institution, or to limit the range of subjects otherwise than by the provisions of the charter, which preclude politics and theology.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences, No. 1, January 2, 1871.

The only paper relating to chemistry and collateral sciences is on—

A Process for Purifying Fatty Substances.—A. Boillot.—The process alluded to is the treatment of the fat—be it suet or any other—with lime-water (*aqua calcis*), heat being applied at the same time. For 1 kilo. of the fatty substance, 2 litres of lime-water are taken. After cooling, the fatty substance is placed in a linen or flannel bag, and submitted to a gentle, but gradually-increased pressure. The fat left in the bag is next melted in hot water to which some sulphuric acid has been added, and after being cooled and well washed with cold water is ready for domestic use,—since it appears from the context of the paper rather that this process is intended to be applied to render fatty substances, otherwise used for soap and candle making, fit for culinary use, the reader being reminded of the condition Paris was in when this paper was published.

No. 2, January 9, 1871.

Historical Account of the Scientific Researches on Gelatine.—E. Chevreul.—The third instalment of this very exhaustive and valuable monograph; not suited, however, for any useful abstraction.

Colza Oil.—A. Wurtz and E. Willm.—The authors state, that when into this oil a current of steam is passed having a temperature of about 120°, there is removed from the oil, which is not thereby to any extent saponified, an acrid principle; and after a washing with a hot solution of carbonate of soda, the oil so purified, after washing, of course, with cold water, may be used for culinary purposes.

The other papers relating to the matters above named bear upon the same or similar subjects, and are rather to be considered as tentative suggestions brought out under peculiar circumstances, and therefore not of general value.

No. 3, January 16, 1871.

This number opens with a valuable essay on—

Dwelling-House Chimneys.—General Morin.

Composition of Milk, and on the Preparation of an Obsidional Milk.—Dr. Dubrunfaut.—The contents of this paper bear only upon the late exceptional condition of Paris, and the scarcity of milk there experienced.

Plan for Utilising the Sewage of Paris.—M. Durand-Claye.—Illustrated by a woodcut.

Nos. 4 and 5, January 23 and 30, 1871.

Do not contain any papers relating to chemistry and collateral sciences.

No. 6, February 6, 1871.

Discovery of Avic Acid in an Albatross.—E. Chevreul.—The eminent author relates at length how, on the 19th of January last, he accidentally made the discovery that the feathers of the well-known large bird called the albatross contain a volatile fatty acid, identical with that which the author has found in the suint of sheep's wool. No further particulars, nor any analysis, of the acid alluded to is here quoted, the author having simply relied on his sense of smell, thereby recognising the well-known odour of the fatty acid alluded to.

Preservation of Potatoes by the Aid of Sulphurous Acid Gas.—V. Labarre.—The author proposes to cause a current of sulphurous acid gas to be passed among the tubers alluded to, either while packed in casks or placed in cellars, since by the use of this gas the fermentation and decay of the potatoes is checked.

Nos. 7, 8, and 9, February 13, 20, and 27; and No. 10, March 6, 1871.

These numbers do not contain papers relating to chemistry and collateral sciences.

No. 11, March 13, 1871.

This number contains the following original paper relating to chemistry:—

Researches on Propylic Bromide and on Butylic Bromide.—I. Pierre and E. Puchot.—The authors describe, at length, the mode of preparation of propylic bromide, C_6H_7Br, by pouring into a long-necked flask 100 parts of pure propylic alcohol, and next, alternately, and by small quantities at the time, 15 parts of phosphorus and from 140 to 145 of bromine, care being taken to keep the phosphorus constantly in excess. After purification of the crude product, a clear, colourless, ethereal liquid is obtained, which boils at 72°, and has at 0° a sp. gr. of 1·349. By contact with moist air, this liquid gradually becomes somewhat coloured. Butylic bromide, C_8H_9Br, is prepared by a similar process to that just alluded to, the only difference being that 120 parts of pure butylic alcohol are taken. The rectified butylic bromide boils at 90·5°, and has at 0° a sp. gr. of 1·249; when freshly prepared, this liquid is colourless, but it soon becomes coloured by a partial decomposition.

No. 12, March 20, 1871.

Preparation of Two Organic Acids obtained by the Reaction of Alkalies upon Silk and Wool.—P. Champion.—*Sericic Acid.*—Raw silk, having been first purified by boiling alcohol, ether, and acetic acid, is treated with boiling baryta-water, whereby a portion of the substance is dissolved; the excess of baryta is removed by carbonic acid from the liquid kept at 80°. After filtration, the filtrate is treated with nitrate of lead, whereby an abundant precipitate is obtained, which, having been collected on a filter and thoroughly washed, is suspended in water and decomposed with sulphuretted hydrogen, the liquid yielding, after filtration and evaporation on a water-bath, an amorphous, slightly yellow-coloured, translucent, deliquescent mass, soluble in alcohol and acetic acid. The baryta salt of this substance, having the same physical properties, was, on analysis, found to yield results leading to the formula $C_{30}H_{29}N_4O_{14}Ba$. *Lanugic acid*, obtained from previously well purified wool by the same process, and yielding a body of similar physical properties, was, on analysis of the baryta salt, found to lead to the formula $C_{38}H_{29}N_5O_{20}Ba$.

No. 13, March 27, and Nos. 14, 15, and 16, April 3, 10, and 17, 1871.

Do not contain any papers relating to chemistry.

No. 17, April 24, 1871.

The only paper relating to chemistry in this number treats on the—

Saccharate of Chloride of Sodium.—Dr. E. J. Maumené.—The author describes, at great length, a series of experiments made with the view to ascertain the precise constitution of the sugar contained in some well-defined large colourless prismatic ortho-rhombic crystals, consisting of 13·295 per cent of chloride of sodium, the rest being sugar, which latter the author's experiments prove to be pure cane sugar; so that thus the compound just alluded to is really a saccharate of chloride of sodium.

No. 18, May 1, 1871.

This number does not contain any papers relating to chemistry.

No. 19, May 8, 1871.

Contains only the following paper bearing on chemistry:—

New Blue Colouring Matter Derived from Eserine.—A. Petit.—Eserine (a very strong base) is carefully saturated with sulphuric acid, and next ammonia in excess is added; the liquid is then evaporated to dryness upon a water-bath, leaving a solid magnificently blue-coloured substance, soluble in alcohol and water, crystallising, and colouring silk blue without the aid of a mordant. The action of acids renders the colour beautifully violet. When eserine is directly treated with ammonia, without having been first saturated with sulphuric acid, there is obtained, on evaporation, a greenish coloured mass, less soluble than that alluded to, and turning wine-red when acted upon by acids. The composition, properties, and mode of formation of the blue pigment alluded to have not been ascertained as yet by the author, who is engaged in further experiments on this subject.

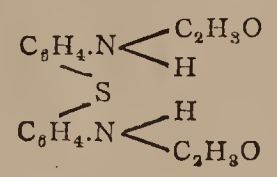
Nos. 20 and 21, May 15 and 29.

The Academy did not meet on May 22. These numbers do not contain any papers relating to chemistry.

Zeitschrift für Chemie von Beilstein, No. 6, 1871.

The only original memoir contained in this number is—

Thio-Aniline and Thio-Toluidine.—V. Merz and W. Weith.—The authors describe, at length, a series of experiments made by them on the action of sulphur upon aniline and toluidine, aided by heat, and best, also, assisted by the simultaneous action of litharge, which greatly accelerates the sulphurisation of the aniline and toluidine, and to some extent prevents the formation of resinous by-products. The result is the formation of thio-aniline, $C_{12}H_{12}N_2S$, a body difficultly soluble in both cold and hot water; but readily so in alcohol, ether, and best in hot benzol, from which it is deposited, on cooling, in crystalline state. Thio-aniline is devoid of smell, has a burning taste, exhibits a neutral reaction to test-paper, and fuses at 105° (when heated under water, it fuses even below 100°); by dry distillation, it at first gives off a very irritating vapour, next chiefly aniline and sulphuretted hydrogen, leaving a large quantity of carbonaceous residue. Thio-aniline behaves as a diamine, fixing 2 equivalents of acid without neutralising it; the salts are generally crystalline. The authors describe a series of these salts, among which the following:—Thio-aniline-chlorhydrate, $C_{12}H_{12}N_2S.2HCl+2H_2O$.—Insoluble in ether, alcohol, and cold concentrated hydrochloric acid; readily soluble in water. Thio-aniline-platin-chloride, $C_{12}H_{12}N_2S.2HCl.PtCl_4$. Thio-aniline-sulphate, $C_{12}H_{12}N_2S.SO_4+H_2O$.—Difficultly soluble in cold water; more readily so in hot water and very dilute sulphuric acid; almost insoluble in alcohol and ether; crystalline. Thio-aniline-oxalate, $C_{12}H_{12}N_2S.H_2C_2O_4$.—Most of the salts of thio-aniline exhibit an acid reaction; even their very dilute solutions impart to fir-wood a beautiful orange colour; with chloride of iron, an intensely blue violet colouration is produced. Thio-aniline is, like the haloid aniline compounds, a very stable body; treated with chloracetyl, the benzol solution of thio-aniline yields thio-acetanilide—



This substance is readily soluble in boiling alcohol, but difficultly so in ether and water; it is a crystalline body which, instead of being prepared as just stated, is more readily obtained by boiling the thio base for several hours with concentrated glacial acetic acid, the only difference between these two preparations being the point of fusion, which, for the chloracetyl preparation, is 213·5°, for the other 215°. Thio-sulpho-carbanilide is obtained by the action of alcoholic solutions of sulphide of carbon and of thio-aniline upon each other. The authors next treat on the resinous products of the sulphurisation of aniline; the purified resinous matter, dried at 120°, yielded 20·87 per cent of sulphur. Thio-toluidine, $C_{14}H_{16}N_2S$, a crystalline substance, almost insoluble in water, very readily so in ether, and tolerably so in alcohol; devoid of taste and smell; reaction neutral; fuses at 103°. This base forms salts with acids, but they are decomposed by a large quantity of water; the salts exhibit a strongly acid reaction. Toluidine-chlorhydrate, $C_{14}H_{16}N_2S.2HCl$, a crystalline salt, yielding, with platinum-chloride, a double salt, $C_{14}H_{16}N_2S.2HCl+PtCl_4$. Thio-toluidine-sulphate, $C_{14}H_{16}N_2S.H_2SO_4$, contains 28·66 per cent of sulphuric acid, is decomposed by boiling water, insoluble in alcohol, and soluble in very dilute sulphuric acid. The authors briefly mention a series of experiments made with the view of the sulphurisation of benzol, but none of these experiments led to the desired result. The passing of vapours of benzol over black sulphuret of antimony heated to redness caused the benzol to be converted into diphenyl.

Bulletin de l'Academie Royale des Sciences, des Lettres et des Beaux Arts de Belgique, No. 4, 1871.

This number contains the following original memoirs and papers relating to chemical and collateral sciences:—

Steam-Boiler Explosions.—Prof. Melsens.—The first instalment of a very important memoir on this highly interesting subject.

Second Paper on the Action of Iodate of Potassium upon the Animal System.—Prof. Melsens.—The contents of this paper belong rather to the domain of experimental physiology than to that of chemistry. The poisonous action of even comparatively small doses of iodate of potassium cannot, as is well observed by the author, be simply due to chemical action, that is to say, to a kind of combustion, whereby organic matter present in the body of animals is destroyed. It appears that the centres of the nervous system are acted upon in a peculiar manner, and that the organised structure of the blood is destroyed, that liquid itself becoming chemically altered at the same time. The author continues his researches on this subject.

Observations on the Constitution of the Carboniferous Limestone of Belgium.—E. Dupont.—An important geological essay illustrated by woodcuts.

Spots on the Sun, as seen and observed at the Astronomical Observatory at Leuven (Louvain) during the years 1864 to 1867 inclusive.—Dr. F. Terby.—Illustrated by a series of engravings.

Annales de Chimie et de Physique, October, 1870.

This number contains the following original papers and essays:—

Experimental Researches on the Sound Produced by Conically-Shaped Organ Pipes.—Dr. J. F. Boutet.—This lengthy essay, illustrated by a series of woodcuts, is divided into the following sections:—General description of the method of executing the experiments; exposition of the calculation of M. Duhamel; experimental verification of the laws set forth by mathematical analysis and applied to conical pipes; comparative and completing observations on cylindrically-shaped organ pipes.

Memoir on the Acetic Derivatives of the Carbo-Hydrates of Mannite and Substances Isomeric therewith, and of some other Vegetable Substances.—Dr. P. Schützenberger.—This is the first portion of a lengthy monograph on this subject. However valuable and interesting in a scientific point of view, it is not possible to quote here more than the headings of the various chapters of this essay—Cellulose and acetic anhydride; amylaceous matter and acetic anhydride; glycogene and acetic anhydride; gum arabic, inuline, glucose, cane sugar, and lactic anhydride with acetic anhydride.

Journal für Praktische Chemie, No. 7, 1871.

This number opens with the continuation of the very lengthy and exhaustive monograph on the—

Dissociation Phenomena Exhibited by Aqueous Solutions of Chloride of Iron.—Dr. F. W. Krecke.—However valuable this work may be, it is not suited for useful abstraction, since the value of the essay is in a great measure owing to the exhaustive tabulated results of a large number of experiments.

Leucine from the Proteine Compounds of Plants.—Drs. H. Ritthausen and U. Kreussler.—This essay contains the following sections:—Preparation and composition of leucine; combustion of leucine with soda-lime; behaviour of the leucine towards the nitrates of mercury; leucin-cupric oxide.

Dispersion of Asparaginic and Glutaminic Acid met with among the Products of the Decomposition of Proteine Compounds.—Drs. H. Ritthausen and U. Kreussler.—100 parts of the following proteine compounds of vegetable origin were found to yield, of asparaginic acid, the following quantities per cent:—Fibrine from Indian corn, 1·4; a mixture of vegetable glue, mucedine, and fibrine, 1·1; gluten-caseine, 0·33; conglutine, 2·0; legumine, 3·5. And of glutaminic acid, per cent—Mucedine, 25; fibrine from Indian corn, 10; mixture of vegetable glue, mucedine, and fibrine, 8·8; gluten-caseine, 5·3; conglutine, 3·5; legumine, 1·5. Proteine compounds of animal origin do not yield any glutaminic acid among the products of their decomposition, but, as the same compounds of vegetable origin, yield asparaginic acid.

Chemical Change Undergone by Silver which had been Buried for a considerable length of time.—Dr. A. Schertel.—The contents of this paper are, inclusive even of the results of analysis, strikingly in accordance with Professor Church's paper on brittle silver (see Chemical News, vol. xxiii., p. 243). The paper of the author named above relates to some treasure trove near Hildesheim.

Bayerisches Industrie und Gewerbe Blatt, April, 1871.

This number contains the following original matter relating to chemistry and allied sciences:—

Prussian Association for the Promotion of Industry at Berlin.—From the programme containing the questions to which the Council of this Society desires to receive answers, the following relate to chemistry:—The gold medal of the Association, or the value thereof in money, and in addition thereto a sum of £150, will be given to the author who gives a thoroughly reliable and satisfactory method, readily executable, of determining the quantity as well as the various compounds met with in the commercial aniline (aniline oil, as it is also termed), and also determines the influence which the diversity of these compounds exercises upon the manufacture, as well as upon the yield of fuchsine; it being also desired that the author should examine and plainly explain under what conditions aniline yields the largest quantity of colouring matter. The silver medal, or money-value thereof, and a sum of £45, will be given for the preparation of an opaque red enamel on gold, silver, copper, or bronze. A donation of £37 10s. will be given to the author of a concise and critical work especially bearing upon the wants of industry in relation to the composition of cements. The silver medal, or the money-value thereof, and, in addition thereto, a sum of £22 10s. will be given to the author of an exhaustive essay on the industrial manufacture, mode of formation, and chemical constitution of corallin (also known as aurin, rosolic acid, and peonin), and on the blue pigment, azuline, derived from it, The silver medal, or its value in money, and, moreover, £75, will be given to the inventor of a yellow-coloured solder which possesses the qualities and properties of the ordinary tinman's solder. The aim of this solder should be to join therewith brass, or similar alloys, in such a manner as to hide the joints from being visible by the different colour exhibited by the solder.

Contribution to the Industrial Statistics of Bavaria.—Dr. M. Haushofer.—A very valuable and exhaustive essay bearing upon the science of political economy in general, and especially exhibiting the high industrial standing of Bavaria.

Analysis of the Water of an Artesian Well Recently Bored at the Brewery of G. Sedlmayr, at Münich.—Dr. H. Schilling.—The well has a depth of 56 metres, and yields, without interruption, 10,000 litres of water per hour, the temperature of which is +9·2° R. (= about 12° C.). 1 litre of this water contains 0·3264 grms. of solid constituents, the salts being calculated as bicarbonates. The percentage composition of this water (it should be borne in mind that the city of Münich is situated at an elevation of 538 metres above sea level) is as follows:—Silica, 4·47; sulphate of potassa, 2·94; sulphate of soda, 0·95; chloride of sodium, 0·15; nitrate of soda, 6·04; bicarbonate of soda, 17·65; bicarbonate of lime, 36·79; bicarbonate of magnesia, 23·80; oxide of iron and trace of alumina, 0·31; organic matter, 6·90. As compared with the ordinary pump-water at Münich, the latter contains 0·848 grms. of solid matter to the litre, including 15·55 per cent of nitrate of lime and 8·85 per cent of organic matter.

NOTES AND QUERIES.

Cheap Electricity.—Mr. Highton informs us that he has discovered the way to consume carbon as the positive in the battery. Further details will shortly be given.

Hydrate of Chloral.—(Reply to "W. E.")—You will find all the particulars you require in vol. xxi., of the Chemical News, pp. 22, 167, 179, and 227; also vol. xxii., pp. 23 and 46.

Disulphide of Carbon.—(Reply to "Sulphur.")—This fluid is largely made in this country and on the continent; it is chiefly used in the caoutchouc and gutta-percha works, and very extensively, also, in the extraction of oils and fats. The price per pound, wholesale, will depend very much upon the quantity to be delivered at once, and also upon the purity.

Disulphide of Carbon.—(Reply to "Sulphur.")—I believe the demand for this in England to be good. It is largely used by varnish-makers, manufacturers of vulcanised india rubber, and for the extraction of fatty matters from shoddy and from greasy refuse of various kinds. The price paid by the writer is about £28 per ton; as far as his knowledge extends, there are only three or four makers in England. —Shoddy.

Estimation of Sulphur in Dense Coke.—(Reply to S. Reddrop.)—Take care to reduce the coke to an impalpable powder, by grinding it in an agate mortar to begin with; next, procure perfectly pure nitrate, or chlorate of potassa free from sulphuric acid. Fuse an excess of either of these salts in a capacious platinum crucible, and, while in fusion, add, with a clean platinum spatula, gradually, your weighed quantity of coke to the fused mass, taking care to cover the crucible with the lid immediately after every addition of a small quantity of the coke-powder at the time; you will, in this way, and with a good strong heat of the crucible—to be distinctly visibly red in daylight—burn off carbon and sulphur, the latter forming sulphate of potassa. After having quite cooled the crucible, treat first with hot water, so as to dissolve the contents, next add, gradually and carefully, to the liquid an excess of hydrochloric acid, and estimate the sulphuric acid in the well-known way with baryta. From the sulphate obtained, calculate the quantity of sulphur.—Dr. A. A.

TO CORRESPONDENTS.

J. W.—The preparation of nitro-prusside of sodium is given in most manuals of chemistry.

Digamma.—The secretary will give you all the information you require.

M. M. P. M.—We cannot well give the names of makers of chloral; our advertisement columns should give you the information.

J. Charlton.—The process is given in every work treating on water analysis.

W. Russell.—The second edition has not yet appeared.

W. Tate.—The action you refer to has, we should think, been observed by every student of chemistry; hence our reason for not inserting your former note.

C. B. Bidwell (Middletown, Conn.).—Received, with thanks.

Prof. Fittig (Tübingen).—Our publisher will comply with your request as far as he is able.

E. W. Prevost (Heidelberg).—Duplicates of the numbers have been forwarded to you.

THE CHEMICAL NEWS.

VOL. XXIII. No. 605.

SUGGESTIONS FOR THE IMPROVEMENT OF THE METHODS EMPLOYED IN QUALITATIVE ANALYSIS.

By A. H. ALLEN, F.C.S.

IT is not supposed that the following remarks contain much that is new, or that experienced chemists would have any difficulty in avoiding error, but I am anxious to call the attention of teachers to the existence of the defects, and the means I would suggest for their removal.

When imparting instruction in qualitative analysis, I have frequently found that the pupils have a difficulty in ascertaining that the substance under investigation is a metallic oxide or hydrate. In my opinion, there are few works on qualitative analysis which give anything like sufficient prominence to these important classes of compounds. Ordinarily, the student is directed to search first for the metal and then for the "acid," and there is generally some short note or observation to the effect that "if no acid can be detected, the substance is probably an oxide or hydrate." I have always found it preferable to test for these classes of compounds before proceeding to the method for the detection of other salt radicles. It is a curious fact that in two recent works on qualitative analysis little or no notice is taken of the solubility of various compounds. In my opinion, the solubility is a most important aid in the analysis of both simple and complex substances, and the manuscript "tables of solubilities" used by my pupils have the solvents for all the important classes of salts, with the addition of the *colour* when this is peculiar and characteristic, and notes in certain cases.

When the metal is ascertained, and the solvent for the substance known, it is very easy to look at this table and learn from it whether the solubility and colour are in accordance with those of any oxide or hydrate of the metal detected. If not, the ordinary method is in no way lengthened; but should the properties agree it is much better to try a few tests to ascertain if the substance be really an oxide or hydrate, than to wade through a tedious method for the detection of salt radicles, and then get the same result in the end "by difference."

Again, a method for the analysis of simple salts ought to be applicable to any ordinary inorganic solid or liquid likely to occur in a laboratory in a single bottle. Thus, if many analytical systems be followed, potassium chromate can only be analysed by a method for mixtures, though it is essentially a "simple substance containing one base and one acid." Ferro- and ferricyanides are not of more frequent occurrence than the permanganates, yet few text-books give any hint of the properties of the latter salts, or contain any note of the abnormal reactions their presence would occasion. Sodium aluminate is, to all intents and purposes, a simple salt, and it is tolerably frequently met with, but if the student examine it by almost any of the ordinary text-books he will fail to ascertain its composition, and trying in vain for an "acid" will begin to think he has met with a soluble oxide or hydrate of aluminium. Sodium stannate, arsenite, and arseniate, are other instances of the same difficulty. Of course it may be said that the student is not supposed to work alone, and that these difficulties will be explained away as they arise; but why should the majority of text-books take no notice of them, or only refer to them in another part of the book instead of calling attention to a possible result in the place where the difficulty would occur.

For these reasons, I direct my pupils to detect the metal first, and then to refer to a table containing descriptions of the chief oxides, hydrates, and anomalous compounds of each metal, while at the same time he is warned that every mentioned property and reaction of the oxide, &c., must be realised before deciding that it is really that compound of the metal. The classes of salts most liable to be mistaken for oxides or hydrates are the sulphides and carbonates, and in some cases the phosphates, arseniates, and oxychlorides. Possibly, a few examples may make my meaning clearer.

(1.) The substance under examination is supposed to be simple, and the metal has been proved to be magnesium. The substance is white, and insoluble in water, but readily soluble in HCl. Instead of going through the whole method for the detection of salt radicles, and coming at last to the conclusion that the substance is MgO, the analyst looks at the table of oxides, &c., and finds the follow-particulars under the head "Magnesium."

"*Magnesium.*	Oxide, $Mg''O$.	White, insoluble in water, soluble in HCl without sensible effervescence. Turns yellow when boiled with $HgCl_2$. Turns wet red litmus-paper blue. The hydrate gives off water at a red heat."
	Hydrate, $Mg''Ho_2$.	

These few reactions are sufficient to distinguish the oxide or hydrate from any other compound of magnesium.

(2.) The substance is black, insoluble in water, soluble in HCl, and has been found to contain copper. Of course, an advanced student would immediately suspect that it was CuO, especially as Cu_2S and CuS are only dissolved by HCl with great difficulty. But the student with a limited knowledge and a short memory would probably wander through the whole acid method, while he might at once detect the nature of the substance by looking at such a table as I describe, where he would read—

"*Copper.*	Cuprous oxide, $Cu_2'O$. Red.	Soluble in HCl, without effervescence or smell of H_2S.
	Cuprous hydrate. Yellow.	
	Cupric oxide, $Cu''O$.	Black. Soluble in HCl, without effervescence or smell of H_2S.
	Cupric hydrate, $Cu''Ho_2$.	Bluish. Turns to black CuO when boiled in water. Soluble in HCl, without effervescence or smell of H_2S."

One more instance will suffice. (3.) The substance is white, soluble in water, and has been found to contain barium. The Table says—

"*Barium.*	Barium oxide, $Ba''O$.	White or grey. Soluble in water; solution alkaline to litmus, and gives brown Ag_2O with $AgNO_3$. Aqueous solution does not effervesce, or evolve H_2S with HCl. The hydrate gives off water when heated to redness with dry SiO_2.
	Barium hydrate, $Ba''Ho_2$.	
	Barium dioxide, BaO_2.	White. Insoluble in water; soluble in HCl. The solution bleaches and liberates I from KI."

This plan is not merely more speedy and accurate than that usually followed, but it familiarises the student with the properties of the various oxides, and gives them their

proper position of precedence. The method for detecting sulphides is not always clearly laid down, and I think might be improved.

Another fault that is common in works on qualitative analysis is the omission of any systematic means of detecting the salts of hydrogen. Considering how important and common are many of the compounds of this "metal," it should have a definite place in the analytical method. Sourness, and the property of reddening litmus-paper, are general properties of the salts of hydrogen, which should no more cause it to be slurred over than the sweet taste of lead compounds, the blue or green colour of salts of copper, or the tendency of silver compounds to blacken in the light, are reasons for the exclusion of these metals from the general method. Of course, hydrogen will be grouped with potassium, sodium, and ammonium, and the students must be taught to ascertain whether the *original* substance is an acid or anhydride. A few simple tests will generally decide this question, a table of the properties of the principal acids, anhydrides, and salt radicles being employed for the purpose.

In many handbooks there is to be found a direction somewhat to the following effect. "Before passing H_2S, if arsenic has been found in the preliminary examination, the solution must be boiled with sulphurous acid."

Now, this renders it *necessary* to make a "preliminary examination," which is not always convenient, especially with a liquid, and in the hands of beginners the dry test for arsenic is very liable to failure when applied to the sulphides of arsenic and the arseniates. Suppose sodium arseniate is under examination, this direction would make it necessary to examine the substance as a "mixture," while no difficulty will be met with if the following plan be employed. Omit the reduction with sulphurous acid, and, in all probability, the arsenic will not be thrown down by H_2S, and the analyst will find the metal to be sodium. The method for the detection of salt radicles will then readily reduce the question to the phosphate or arseniate, which, of course, are distinguished by silver nitrate. If H_2S does produce a precipitate, arsenic will be detected, and, on reference to the above-mentioned table of oxides and anomalous substances the analyst will learn that the brown Ag_3AsO_4 produced with silver nitrate indicates an arseniate, its incomplete volatility shows that it is not the hydrogen or ammonium salt (or arsenic pentoxide), and a reference to the table of solubilities will tell him that the only other arseniates soluble in water are those of potassium and sodium. The same plan would be preferable if arseniate of calcium or iron were under examination, the arseniate being mistaken for phosphate up to a certain point, and then readily distinguished from the latter.

I am engaged on a work on analysis in which the above suggestions are incorporated, and I shall be obliged to anyone who will assist me in reducing the chances of error for beginners in analysis.

ON THE ACTION OF HYDROBROMIC ACID ON CODEIA AND ITS DERIVATIVES.*

By C. R. A. WRIGHT, D.Sc.,
Lecturer on Chemistry in St. Mary's Hospital Medical School.

IT has been shown in Part I. of this research† that the action of hydrobromic acid on codeia gives rise, without evolution of methyl bromide, first to bromocodide, and secondly to two other new bases termed respectively deoxycodeia and bromotetracodeia, the latter of which, under the influence of hydrochloric acid, exchanges bromine for chlorine, yielding a corresponding chlorinated base, chlorotetracodeia; when, however, the action of hydrobromic acid is prolonged, methyl bromide is evolved in some little quantity. By digesting codeia with three or four times its weight of 48 per cent acid for five or six hours on the water-bath, vapours were evolved which, condensed by the application of a freezing mixture to a colourless mobile liquid, the boiling-point of which was found to be 10·5° to 11·5°, and the vapour of which burnt with a yellow-edged flame, exploded violently with oxygen, forming carbonic and hydrobromic acids; it becomes, therefore, of interest to examine in detail the action of hydrobromic acid on each of the three bodies produced from codeia under its influence.

1. *Action of Hydrobromic Acid on Bromotetracodeia.*

When bromotetracodeia hydrobromate is heated in a sealed tube to 100° with four or five times its weight of 48 per cent hydrobromic acid for from six to ten hours, methyl bromide is found as a thin layer on the top of the tarry contents of the tube after cooling; by dissolving this tarry substance in water and fractionally precipitating the liquid by strong hydrobromic acid several times successively, nearly white amorphous flakes are ultimately obtained, resembling in all their physical and chemical properties the bromotetracodeia hydrobromate originally employed. After desiccation, first over SO_4H_2, and finally at 100°, there were obtained numbers which correspond with those required for a base bearing the same relation to morphia that bromotetracodeia does to codeia; it is, therefore, provisionally named bromotetramorphia.*

Hence the action of hydrobromic acid on bromotetracodeia is

$$\underbrace{C_{22}H_{83}BrN_4O_{12}}_{\text{Bromotetracodeia.}} + 4HBr = 4CH_3Br + \underbrace{C_{68}H_{75}BrN_4O_{12}}_{\text{Bromotetramorphia.}}.$$

Carbonate of soda throws down from the solution of the hydrobromate a nearly white precipitate which rapidly oxidises, and appears identical in all its physical properties and chemical reactions with bromotetracodeia.

When crude bromotetramorphia hydrobromate is precipitated by carbonate of soda, and the precipitate (after filtration and washing) re-dissolved in hydrochloric acid and fractionally precipitated twice or thrice by strong hydrochloric acid, white flakes free from bromine are ultimately obtained; these are the hydrochlorate of the corresponding chlorinated base, which is therefore termed chlorotetramorphia. After drying at 100°, numbers were obtained which led to the formula $C_{60}H_{75}ClN_4O_{12},4HCl$.

Converted into platinum-salt and dried at 100°:—

0·4235 grm. gave 0·0840 Pt = 19·83 per cent.

The formula $C_{68}H_{75}ClN_4O_{12},4HCl,2PtCl_4$ requires 19·72 per cent.

When codeia is heated on the water-bath with three parts of 48 per cent hydrobromic acid for five hours, and the portion of the precipitate thrown down by carbonate of soda and insoluble in ether is dissolved in hydrochloric acid, and fractionally precipitated several times by excess of stronger acid, flakes are obtained which, on drying at 100°, yield numbers intermediate between those required for chlorotetracodeia and chlorotetramorphia, leading to the formula $C_{70}H_{79}ClN_4O_{12},4HCl$.

Converted into platinum-salt, and dried at 100°:—

0·4830 grm. gave 0·0935 Pt = 19·36 per cent.

The formula $C_{70}H_{79}ClN_4O_{12},4HCl,2PtCl_4$ requires 19·40 per cent.

Whether this is only a mixture of chlorotetracodeia and chlorotetramorphia hydrochlorates, or is one compound, is open to doubt; assuming that it is not a mixture, the name chloro-dicodeia-dimorphia might be applied to the base. It appears *à priori* probable that the following double series of bases should be obtainable by successive methyl eliminations:—

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* Read before the Royal Society, June 15, 1871.

† *Proc. Roy. Soc.*, vol. xxi., p. 317.

* All combustions given in this paper were made by lead chromate and oxygen; except where otherwise stated, chlorine and bromine were determined by boiling with silver nitrate and nitric acid.

Bromotetracodeia	$C_{72}H_{83}BrN_4O_{12}$
	$C_{71}H_{81}BrN_4O_{12}$
	$C_{70}H_{79}BrN_4O_{12}$
	$C_{69}H_{77}BrN_4O_{12}$
Bromotetramorphia	$C_{68}H_{75}BrN_4O_{12}$
Chlorotetracodeia	$C_{72}H_{83}ClN_4O_{12}$
	$C_{71}H_{81}ClN_4O_{12}$
Chloro-dicodeia-dimorphia	$C_{70}H_{79}ClN_4O_{12}$
	$C_{69}H_{77}ClN_4O_{12}$
Chlorotetramorphia	$C_{68}H_{75}ClN_4O_{12}$

Out of these ten bases four have been prepared, and a substance corresponding in composition with a fifth (chloro-dicodeia-dimorphia) has also been obtained; but from the great similarity in properties between all the five substances and their high formulæ, it is clear that no certainty as to the purity of the missing intermediate bodies could exist, and therefore it was not thought advisable to attempt their formation.

2. *Action of Hydrobromic Acid on Bromocodide.*

When bromocodide hydrobromate (prepared by two hours' digestion of codeia with three times its weight of 48 per cent HBr, precipitation by sodium carbonate, and extraction with ether, &c.) is heated with four to six parts of the same acid to 100° for five or six hours either in a sealed tube or in an open flask, methyl bromide is copiously evolved; the tarry product, dissolved in warm water and precipitated by sodium carbonate, is for the most part insoluble in ether, the insoluble portion having all the properties of bromotetramorphia; the ethereal extract shaken with HCl or HBr yields a viscid liquid, which, on standing, becomes filled with crystals consisting apparently of a mixture of the hydrochlorates, or hydrobromates, of deoxycodeia and a lower homologue, the latter predominating when the digestion is performed in an open flask. Attempts to prevent the formation of the lower homologue by continuing the digestion with HBr for only two or three hours did not succeed, as the large quantity of unaltered bromocodide in the ether extract obtained prevents the separation of the crystalline hydrochlorate or hydrobromate of deoxycodeia, and hitherto no method of separating the deoxycodeia salt from its lower homologue has been arrived at.

The following numbers were obtained by the analysis of these crystals after re-crystallisation from hot water to free them from adhering bromocodide-salt:—

Specimen A, prepared in sealed tubes, digested six hours at 100°:—

0·3185 grm. gave 0·7850 CO_2 and 0·1970 H_2O.
0·2200 grm. gave 0·1025 AgCl.

Specimen B, prepared in an open flask, digested six hours at 100°:—

0·2825 grm. gave 0·6920 CO_2 and 0·1660 H_2O.
0·2260 grm. gave 0·1085 AgCl.

	Calculated.				Found.	
Deoxycodeia.			Deoxymorphia.			
C_{18}	216	67·60	C_{17}	204	66·77	67·22 66·80
H_{22}	22	6·89	H_{20}	20	6·55	6·87 6·54
N	14	4·39	N	14	4·59	
O_2	32	10·01	O_2	32	10·47	
Cl	35·5	11·11	Cl	35·5	11·62	11·53 11·88
	319·5	100·00		305·5	100·00	
$C_{18}H_{21}NO_2$,HCl			$C_7H_{19}NO_2$,HCl			

The hydrobromate, prepared from the same batch as specimen B above, gave the following numbers after drying at 100°:—

0·3260 grm. gave 0·7010 CO_2 and 0·1720 H_2O.
0·2730 grm. gave 0·1465 AgBr.

	Calculated.				Found.	
Deoxycodeia.			Deoxymorphia.			
C_{18}	216	59·34	C_{17}	204	58·29	58·64
H_{22}	22	6·05	H_{20}	20	5·71	5·86
N	14	3·84	N	14	4·00	
O_2	32	8·79	O_2	32	9·14	
Br	80	21·89	Br	80	22·86	22·83
$C_{18}H_{21}NO_2$,HBr	364	100·00	$C_{17}H_{19}NO_2$,HBr	350	100·00	

From the above numbers, and more especially from the percentages of H, Cl, and Br found, it appears that while specimen A may have contained some little quantity of deoxycodeia, specimen B must have consisted almost wholly of the lower homologue; to this body the name deoxymorphia may appropriately be given (provisionally). to indicate that its composition bears the same relation to that of morphia as deoxycodeia to codeia.

The numbers required for apomorphia salts are very close to those actually obtained above, viz., for hydrochlorate C = 67·22, H = 5·93, Cl = 11·70; and for hydrobromate C = 58·62, H = 5·17; Br = 22·99; but the entire absence of emetic properties in all these specimens, as observed by Dr. Michael Foster, conclusively proves that this base could not have been present.

Further research is required before it can be decided with certainty which of the three oxygen atoms in codeia is removed in the formation of deoxycodeia; the production of deoxymorphia with simultaneous evolution of methyl bromide, however, indicates that the oxygen that links the methyl group to the rest of the codeia residue is still present in deoxycodeia and deoxymorphia, while the production of both from bromocodide renders the following formulæ probable:—

Codeia. $C_{17}H_{17}NO\begin{cases}OH\\O.CH_3,\end{cases}$ Bromocodide. $C_{17}H_{17}NO\begin{cases}Br\\O.CH_3,\end{cases}$

Deoxycodeia. $C_{17}H_{17}NO\begin{cases}H\\O.CH_3,\end{cases}$ Deoxymorphia. $C_{17}H_{17}NO\begin{cases}H\\OH;\end{cases}$

so that deoxycodeia probably bears to codeia the same relation as free hydrogen, H_2, to water, H.OH; or as acetic acid, $\frac{CH_3}{CO.OH}$, to glycollic, $\frac{CH_2.OH}{CO.OH}$; bromocodide corresponding similarly to hydrobromic acid, HBr, or to bromacetic acid, $\frac{CH_2Br}{CO.OH}$.

Experiments are in progress to gain further insight into the structure of the group $C_{17}H_{17}NO$. By the action of hydriodic acid on codeia methyl is eliminated as iodide, and the elements of free hydrogen and those of HI are added on to this group; from which, as well as from the easy polymerisation to form tetracodeia bases, it appears probable that some at least of the 17 carbon atoms are connected together somewhat after the fashion of ethylene or acrylic acid, which unite readily with HI, HBr, H_2, &c. Again, the oxidising action of $AgNO_3$ on chlorotetramorphia is accompanied by the production of CO_2, which renders it not improbable that the third oxygen atom exists either as the group [CH(OH)]″ or as CO″.

On carefully examining, side by side, the qualitative reactions of the hydrochlorate B and those of a specimen of pure deoxycodeia-salt from codeia (without evolution of methyl bromide), not the slightest difference was discernible between the two; in their physiological actions, too, as observed by Dr. Michael Foster, the two bodies seem perfectly alike, both being utterly dissimilar from apomorphia, from which in all other respects (qualitative reactions, percentage composition, &c.) they differ either not at all, or extremely little.

The portion insoluble in ether of the batch from which hydrochlorate B was obtained was treated with HCl and fractionally precipitated by strong acid several times successively; this mode of treatment was adopted rather than that with HBr, as a much larger yield is obtained thus, the hydrochlorates of chlorotetracodeia and chlorotetramorphia being much less soluble in dilute HCl than the corresponding brominated bodies are in dilute HBr. Finally, nearly white flakes were obtained, presenting all the characters of chlorotetramorphia hydrochlorate, and yielding the following numbers after drying at 100°:—

0·2480 grm. gave 0·5610 CO_2 and 0·1410 H_2O
0·1390 grm. gave 0·0755 AgCl.

	Calculated.		Found.	
C_{68}	816·0	61·79	61·70	
H_{79}	79·0	5·98	6·32	
Cl_5..	177·5	13·44	..	13·45
N_4..	56·0	4·24		
O_{12}	192·0	14·55		
$C_{68}H_{75}ClN_4O_{12},4HCl$	1320·5	100·00		

Hence the portion insoluble in ether must have been bromotetramorphia.

The simultaneous formation of bromotetramorphia and deoxymorphia from bromocodide is explainable in two ways: either

(I.) $$\underbrace{5C_{18}H_{20}BrNO_2}_{\text{Bromocodide.}}+4H_2O=$$
$$=\underbrace{C_{72}H_{83}BrN_4O_{12}}_{\text{Bromotetracodeia.}}+\underbrace{C_{18}H_{21}NO_2}_{\text{Deoxycodeia.}}+4HBr.$$

(II.) $$\underbrace{C_{72}H_{83}BrN_4O_{12}}_{\text{Bromotetracodeia.}}+4HBr=$$
$$=\underbrace{C_{68}H_{75}BrN_4O_{12}}_{\text{Bromotetramorphia.}}+4CH_3Br.$$

(III.) $$\underbrace{C_{18}H_{21}NO_2}_{\text{Deoxycodeia.}}+HBr=\underbrace{C_{17}H_{19}NO_2}_{\text{Deoxymorphia.}}+CH_3Br\text{ ; or}$$

(IV.) $$\underbrace{C_{18}H_{20}BrNO_2}_{\text{Bromocodide.}}+HBr=\underbrace{C_{17}H_{18}BrNO_2}_{\text{Bromomorphide.}}+CH_3Br.$$

(V.) $$\underbrace{5C_{17}H_{18}BrNO_2}_{\text{Bromomorphide.}}+4H_2O=$$
$$=\underbrace{C_{68}H_{75}BrN_4O_{12}}_{\text{Bromotetramorphia.}}+\underbrace{C_{17}H_{19}NO_2}_{\text{Deoxymorphia.}}+4HBr.$$

Of these two views, the first involves only known substances and reactions similar to those already known in the codeia series of derivatives, and is, moreover, probable from the circumstance that the numbers obtained in some instances indicate the presence of deoxycodeia as well as deoxymorphia; whilst the second view involves the not improbable existence of bromomorphide, $C_{17}H_{18}BrNO_2$; on the other hand, it will be shown in the next section that equation (III.) represents a reaction which does not readily take place with deoxycodeia, when not in the nascent condition at any rate.

Whichever view be adopted, the ultimate formation of bromotetramorphia requires the action of water on a brominated body, substituting hydroxyl for bromine by a reaction perfectly parallel to that whereby codeia is regenerated from chlorocodide by the action of water,* viz.:—

$$C_{18}H_{20}ClNO_2+H_2O=HCl+C_{18}H_{21}NO_3.$$

3. *Action of Hydrobromic Acid on Deoxycodeia.*

In the hope that this action would give rise to methyl bromide and deoxymorphia, deoxycodeia hydrobromate was heated to 100° for two hours with about five parts of 48 per cent HBr; no change whatever took place, no methyl bromide being found on opening the tube in which the digestion was carried on after complete cooling. After an hour's additional exposure to a temperature of 120°—130°, the contents of the tube were found to have become black and tarry, while a small quantity of methyl bromide floated on the top. Precipitated by sodium carbonate, a very dirty substance was obtained, which was almost insoluble in ether; the ethereal extract, shaken with HBr, gave a small quantity of a tarry hydrobromate, of which 0·1330 grm. gave 0·0790 AgBr, or Br=25·20 per cent, deoxymorphia hydrobromate requiring only 22·86 per cent.

Nothing fit for analysis could be obtained from the portion insoluble in ether, and the minute yield of pure deoxycodeia from codeia precluded a repetition of the experiment.

* Matthiessen and Wright, *Proc. Roy. Soc.*, vol. xviii., p. 88.

4. *On the Physiological Action of the Foregoing Codeia Derivatives.* By Michael Foster, M.A., M.D.

The hydrochlorate of chlorotetracodeia and the hydrobromate of bromotetramorphia, in doses of a decigramme by subcutaneous injection or by the mouth, produced in adult cats in a very few minutes a condition of great excitement, almost amounting to delirium, accompanied by a copious flow of saliva and great dilatation of the pupils. Micturation and defæcation occurred in some instances, and vomiting was observed on two occasions with the morphia-salt, but was very slight. The excitement was very peculiar, being apparently due partly to increased sensitiveness to noises, and partly to an impulse to rush about.

The same doses of the morphia-salt given to a young kitten produced the same flow of saliva, dilatation of pupils, and excitement (without vomiting); but the stage of excitement, which in adult cats passed gradually off in a few hours, was followed by a condition marked by a want of co-ordination of muscular movements, and presenting the most grotesque resemblance to certain stages of alcoholic intoxication. This stage was followed in turn by sleepiness and stupor, in which the kitten was left at night; in the morning it was found dead.

Two observations have shown these salts paralyse (in dogs and cats) the inhibitory fibres of the pneumogastric; they also seem to lower the internal tension, but want of material has prevented from ascertaining how this is brought about.

On *rabbits* neither salt, even in doses of a decigramme, seems to have any effect, except perhaps a slight excitement. There is no dilatation of the pupils, no flow of saliva, and, if one observation can be trusted, no paralysis of the inhibitory fibres of the pneumogastric.

No marked difference was observable between the two salts, except that the morphia salts seemed rather more potent than the corresponding codeia bodies.

The salts of deoxycodeia and deoxymorphia given by mouth or by subcutaneous injection in doses of a decigramme, produced in adult cats, almost immediately after exhibition, a series of convulsions much more epileptic in character than tetanic. In one case there was a distinct rotatory movement.

In a few minutes these convulsions passed away, leaving the animal exhausted and frightened. Then followed a stage of excitement with dilated pupils and flow of saliva, very similar to the effects of the tetracodeia and tetramorphia salts, but less marked.

Doses of half a decigramme given to adult cats produced the stage of excitement only, without the convulsions.

In no case, with any specimen of product, has vomiting been witnessed.

Trials with rabbits gave only negative results. Like the tetracodeia and tetramorphia products, the deoxycodeia and deoxymorphia salts appear to paralyse the inhibitory fibres of the pneumogastric.

No marked differences could be observed between the hydrochlorates and hybromates of deoxycodeia or deoxymorphia.

CHEMICAL AND PHYSICAL ANALYSIS OF SUGAR.*

M. Payen's Process.

This process is based on the insolubility of sugar crystals in alcohol saturated with pure sugar, whilst many foreign matters are soluble in this liquid. The following is the mode of operating:—

An average sample of sugar to be analysed is rubbed lightly in a mortar, so as to break the lumps without

* *Saccharimetrie Optique, Chimique, et Melassesimetrique.* By L'Abbé Moigno.

injuring the crystals. 10 grms. of this sugar are weighed, and placed in a tube about 15 m.m. in diameter, and 30 centims. in length; then about 10 centims. of absolute alcohol are added, to eliminate the 2 or 3 per cent of water which the raw sugar may contain. After shaking the tube it is allowed to settle, and the alcohol decanted. There is then poured into the tube 50 centims. of the test-liquor, which is prepared in the following manner:—

To 1 litre of alcohol of 88° is added 50 c.c. of acetic acid of 7°; in this liquid there is dissolved 50 grms. of dry powdered white sugar. This quantity will saturate the liquid at a temperature of 59° F.; but in order that it may remain saturated in changes of temperature, a string of white sugar-candy crystals is suspended in the vessel which contains the liquor. The solution thus prepared is able to dissolve the uncrystallisable sugar and molasses, to decompose and dissolve sucrate of lime, without dissolving the crystals of pure sugar, because it is saturated with the latter.

(The saturated liquor was not quite all that could be wished in some changes of exterior temperature. M. Numa Grar has rendered it more exact by powdering sugar-candy in a mortar, mixing it with the test-liquor, and filtering it; then, on immediate use, it gave comparable results in the valuation of sugar to within a half per cent in the proportion of pure crystallised sugar extracted from the raw sample).

To continue: 50 centims. of this test-liquor are poured into the tube containing the raw sugar which has been washed with absolute alcohol and rapidly drained. The tube is shaken, and allowed to stand, and when the liquor has become clear it is decanted, and a fresh quantity of test-liquor poured in as first; it is again allowed to settle, and again the clear liquor is decanted. It is needful to make a last washing with alcohol of 96° or 98° to remove the saturated liquid interposed between the crystals. There only remains to place the sugar on a filter to dry, and weigh it. The difference between the weight of the sample submitted to analysis and that of the sugar remaining after the process indicates the foreign soluble substances which are mixed with the raw sugar. If the sugar contains insoluble substances, their quantity may be ascertained by dissolving all the remaining sugar in alcohol of 60°, filtering and weighing; the residue remaining on the filter will be the insoluble substances which the sugar contains.

M. Dumas has modified his process, and has rendered it more rapid and more sensitive by directly estimating the foreign soluble matters—the glucose and salts—by the excess of density which the test-liquor presents after the sugar has been washed in it above the density of the saturated solution used in the process. In the practice of this process an ordinary *alcoomètre* is all that is needed. The other parts of the process are nearly the same as M. Payen's.

The process is as under: To a litre of alcohol of 85° are added 50 grms. of acetic acid of 80°. To this liquid is added as much pure sugar as it will dissolve; it then marks 74° on the *alcoomètre*. On mixing a decilitre of this liquor with 50 grms. of sugar to be analysed, and filtering the liquid, to finish the valuation of the sugar it is only needful to plunge the *alcoomètre* into the filtered liquid; if it marks 74° again the sugar is pure; if it descends to 69° the pure sugar is 95 per cent; if it goes down to 64° the pure sugar is 90 per cent. Each degree lost on the *alcoomètre* corresponds to a degree of diminution in the richness of the sugar.

In very low-priced sugars, the variable nature of the impurities renders this latter analysis a little uncertain; but for sugars 87° to 100° (of saccharine value), which form the bulk of the sugars of commerce, the results accord with those of the polariscope. If the sugar contains sand or other insoluble matters, they must be taken account of.

In an important refinery in Paris, at Lille, in the most competent hands, in the Customs' Laboratory, and in that of the Sorbonne, this process has given similar results to the polariscope. In an average of twenty analyses made to determine the richness of a large delivery of sugars of three classes, the difference did not exceed one-thousandth, that is to say, it was *nil*. In a single trial it rarely varies 1 per cent, which is of no account, as we know that the sugars of commerce classed in the same type often differ as much as 8 or 10 per cent in richness.

M. Clerget acknowledges that the old process of M. Payen, although it does not indicate exactly the proportion of pure sugar contained in the raw sugar, gives useful results. He also allows that the process of M. Dumas is very important, not only from its exactitude, but still more so from the simplicity and promptitude of the method; at the same time, he calls attention to this important fact.

It is not sufficient to value the return in crystallisable sugar which may be obtained from saccharine juices—to prove the amount of pure sugar ($C_{12}H_{11}O_{11}$) which the juice contains—because, as is well known, up to the present time it has been found impossible to extract the whole of this, or even a quantity proportionately constant, because of other soluble substances contained in the juice.

Barreswill's Process.

We reproduce this process, although it has been considerably improved, because M. Barreswill was one of the first to attack the difficult problem of sugar analysis. His saccharimetrical process is founded on the following facts, discovered by M. Frommers:—

(1). That crystallisable sugar does not reduce the oxide of copper contained in an alkaline fluid.

(2). That it becomes able to reduce this oxide when treated with sulphuric acid, which, by a few minutes' boiling, transforms it entirely into glucose.

(3). The quantity of binoxide reduced is in proportion to the quantity of sugar employed.

When it is required to find the quantity of crystallisable sugar which exists in a liquid, to the exclusion of every other organic product, a standard alkaline solution of oxide of copper must be prepared. By bringing together sulphate of copper, neutral tartrate of potash, and caustic potash, a liquid of an intense blue colour is obtained, which, being filtered, will remain clear and limpid a long time. This solution is the test-liquor, the strength of which must next be determined by ascertaining how much of a solution made with a certain weight of pure dry sugar-candy, with the addition of some drops of sulphuric acid, is needed to decolour exactly a determined volume of it. The test-liquor being thus carefully standardised, a certain quantity of it is placed in a porcelain or glass capsule, and there is then added to it some very concentrated caustic potash, the object of which is to increase the density of the liquid, and render the after precipitation of the oxide of copper more rapid. Then, by means of a graduated burette, the acidified sugar solution is allowed to fall drop by drop into the heated (boiling) test-liquor. As soon as the two liquids are in contact, a yellow precipitate of hydrate of copper appears, which becomes red, and settles to the bottom of the capsule when it has reached the temperature of the liquid in which it is formed. In proportion as the operation advances, the colour of the test-liquor decreases in intensity, whilst the copper is precipitated in a state of protoxide. It is terminated when the solution is entirely decoloured. Then by reading on the burette the number of divisions which it has required to arrive at this limit, we obtain by means of a calculation the quantity of sugar contained in the liquid under examination.

The delicate part of the operation is to seize the point when the precipitation of the oxide of copper is complete. We may arrive at this (if the sugar solution is itself uncoloured) as well by the decolouration of the test-liquor as by the cessation of the formation of the yellow-coloured precipitate, which precedes the deposit of the oxide of copper. The latter is the only test when the liquid to be tested is itself coloured. An excess of sugar solution

added to the test-liquor after the separation of the oxide of copper is complete gives to the solution a brown colour, easily recognised, which results from the reaction of the hydrated alkalies on the glucose.

In cases where the sugar liquid of which the composition is required contains both crystallisable sugar and glucose, the proportion of the latter substance is determined by first making trial with a portion of the liquid brought to a known volume, before it has been submitted to the action of sulphuric acid ; the glucose alone reduces the copper solution, which pure sugar leaves intact.

Another portion of the sugar liquid is afterwards boiled with sulphuric acid, so as to convert all the sugar into glucose ; by means of a second assay made with the liquor thus modified we have the total weight of the glucose which it then contains ; and deducting the weight of the pre-existent glucose obtained by the first operation, we have in the difference the amount of crystallisable sugar contained in the sugar solution (or rather the quantity of glucose which the crystallisable sugar was able to furnish by treatment with sulphuric acid).

When the sugar solution contains (before treatment with acid) only crystallisable sugar, the amount may be ascertained by this method in a quarter of an hour, within 2 or 3 per cent.

The chief fault of this process is that it is only applicable in cases of simple solutions of pure sugar, or of a mixture of pure sugar and glucose. If the sugar to be tested contains tartaric acid, dextrine, or sugar of milk, these substances will act nearly in the same way as crystallisable sugar, and may consequently be confounded with it. On the other hand, there is no doubt of the existence of some organic substances which will reduce the alkaline copper solution in the same manner as glucose itself; in consequence of which this process cannot be employed with absolute certainty, unless by previous experiments it has been proved that other organic substances do not exist in the sugar to be analysed, or that these foreign matters have been properly separated by some efficient method.

Fehling's Copper Liquor.

The improvements mentioned as having been made in Barreswill's process are chiefly in the preparation of the test-liquor, which is called after the inventor. It is prepared as follows :—

" Dissolve 40 grms. of crystallised sulphate of copper in 160 c.c. of distilled water ; in a separate vessel which will hold 1 litre place 160 grms. of cream of tartar, 130 grms. of caustic soda, and 500 to 600 grms. of distilled water at the boiling point. The hot alkaline liquor thus obtained dissolves the tartrate of potash very rapidly. The solution of sulphate of copper is then added little by little, when all this salt is dissolved ; the litre bottle is filled up with distilled water. The liquor thus obtained is kept in a ground-stoppered bottle."

To standardise the copper liquor :—

" Having provided the liquor as above, the exact standard of it is then determined—*i.e.*, the quantity of sugar which corresponds to a certain volume of this liquor ; 10 grms. of pure sugar are dissolved in 80 to 100 grms. of water ; 2 or 3 grms. of hydrochloric acid are then added, and by four or five minutes' boiling the sugar is completely converted into glucose. This solution is thrown into a litre bottle, which is filled with distilled water to the gauging point.

Each c.c. of this liquor represents 1 centigrm. of pure inverted sugar.

Place 25 c.c. of the copper liquor in a capsule, and raise to the boiling point, when by means of the graduated burette the normal sugar solution falls drop by drop into the boiling copper liquor, until a red precipitate of oxide of copper is formed. Suppose that 15 c.c. of the normal sugar solution have decoloured the tartrate of copper, as each c.c. represents 1 centigramme of sugar, it needs 15 centigrammes of inverted sugar to decolour 25 c.c. of Fehling's test-liquor. The standard is then represented by 0·15 grm. The process is thus applied to colonial sugar ; 10 grms. of sugar are placed in a flask holding 100 c.c., which is then filled up with distilled water. Each c.c. of this liquid represents 0·1 grm. of raw sugar. This liquid is allowed to fall drop by drop, as before, into the vessel containing 25 c.c. of the boiling copper liquor. Suppose that it requires 30 c.c. of the sugar solution, representing 3 grms. of sugar, to decolourise the liquor ; the standard of the copper liquor being 0·15 grm. the glucose is found by the following calculation :—

$$3 \text{ grms.} : 0{\cdot}15 \text{ grm.} : : 100 : x$$
$$x = 5 \text{ grms.}$$

There is then 5 per cent of glucose in the raw sugar.

Chemical Analysis in its Application to the Sugar Industry.—By M. Feltz.

We are induced to reproduce this clear and practical note because it brings to our notice two instruments used in the sugar industry of Germany—the saccharometer of Balling and the areometer of Brix, with which it is worth while to become acquainted.

When it is wished to value the yield of sugar in a saccharine substance, the ordinary process has been to determine first the amount of crystallisable sugar it contains by the saccharometer. Another weight of the saccharine substance is then burnt, and this furnishes the quantity of ashes. These two known, the return is calculated according to the saline method.

The saccharimetrical method does not require much time. The weighing of a sample is easy and rapid, because there is no need to weigh to a centigramme.*

It is not the same in the determination of the ash. Three-quarters of an hour must be allowed for the incineration of solid saccharine substances, and at least one hour and a half for the evaporation and incineration of a juice or a syrup with the necessary care. Further, incineration, to be carried out with facility, requires apparatus of particular nicety. For an easy method of analysis in the factory it must therefore be renounced.

The saline method is founded on this principle—that the quantity of molasses produced in the manufacture of beet-sugar is proportionate to the amount of salts existing in the saccharine substance.

Another method practised for some time in German sugar factories is based on this—that the quantity of molasses produced is sensibly proportionate to the sum total of the foreign matters contained in the substance, deduction made for the water and the sugar. I shall speak particularly of this process, the result of which is called *the co-efficient of purity*.

Balling graduated an areometer in such a manner that each degree marked upon it corresponded to one per cent in weight of pure sugar dissolved in water. When the areometer known in Germany as Balling's saccharometer,† or Brix's areometer, marks, for example, 15° in a solution of pure sugar, it signifies that in 100 parts of the solution there are 15 of pure sugar.

Juice or syrup from the factory is always a solution of sugar and foreign matters in water. When Brix's areometer is plunged in this solution, this instrument marks a certain number of degrees corresponding both to the sugar and foreign matters in the solution. If, now, by an observation on the polariscope, or by Barreswill's copper method, it is determined how much pure sugar is contained in the solution, it is easy to calculate the weight of the impurities. Suppose that a sugar solution marked 15° on the areometer of Brix, and that saccharimetrical observation indicates 12 per cent of sugar, to obtain the quantity of foreign matters which are found in 100 parts

* 16·350 grms. of pure sugar, combined with 100 c.c. of pure water, gives in Duboscq's instrument a rotation of 100° ; therefore 1 centigramme will give the rotation of 0·60 c. ; in fact, an inappreciable quantity.

† M. Brix corrected the scale on Balling's instrument, and it is his areometer that is universally used in Germany.

in weight of the solution, it will be sufficient to deduct 12 from 15. Thus of 15 parts of solid matter, 12 are sugar; in 100 parts there will be $\frac{1200}{15} = 80$ parts of sugar, and the co-efficient of purity of this solution is 80. We may remark that by subtracting 15 from 100 we shall have the quantity of water in the sugar liquid.

Thus, two determinations, both quickly made, are sufficient to determine the sugar, the water, and the foreign matters. The determination of the foreign matters is, however, not quite perfect. We have supposed that a degree of Balling or Brix corresponds *exactly* to 1 per cent of any substance whatever, which is not strictly correct. It will be found that the weight of foreign substances is always *less* than that indicated; but practically this is of no importance. It is, besides, possible, by a series of experiments, to determine the co-efficient of correction for the areometer in this respect.—*The Sugar Cane.*

ON COLOURS FROM COAL-TAR.*

By HARRY N. DRAPER, F.G.S.

(Continued from p. 297).

To illustrate the manner in which the chemical method of separation is applied, we have arranged here some glasses in which benzol, aniline, and phenol have each been shaken with an acid and an alkali. You will observe that, in the case of benzol, it floats indifferently upon the surface of both acid and alkali; but that the aniline, while untouched by the alkali, is dissolved by and mixes with the acid. The behaviour of phenol is exactly the reverse of this, for the alkali dissolves, while the acid is without action on it. You see in this experiment a general outline of the method by which coal-tar is made to give up the different bodies which it contains. We must, however, pass with some rapidity into detail: and bearing in mind that we have here to do with but five constituents of coal-tar, speak of them one by one as they come before us in their aspect of sources of colour.

By far the most important of these is that which we have here, benzol. To obtain it, the coal-tar oil is distilled by steam heat, and all those portions which boil under 90° on the centigrade scale are collected. Pure benzol is a very volatile, inflammable fluid, of a peculiar odour, forcibly recalling that of coal-gas. It boils at 81°C. It is very much used in the arts as a solvent of india-rubber; and, under the name of *benzine collas*, we are familiar with its employment for the removal of grease stains. When cooled to about the freezing-point of water, it forms very beautiful crystals. This fact is sometimes taken advantage of to effect its separation from the other bodies which accompany it in coal-tar oil. I must ask you to remember, as this is the point from which all we have to say about the colour-giving matters of coal-tar diverges, that benzol was discovered in 1825 by Faraday, who found it as a product of the action of a high temperature on benzoic acid. It is, however, to Hofmann that we owe its recognition as one of the products of the distillation of coal. This discovery was made in 1845.

Much lower down in the diagram you will see the name of another body, aniline, and to this substance I musk ask your particular attention. Aniline was discovered in 1826, by Undervorben, who found that indigo gave it as one of the products of its distillation. It has been successively known by the names of "crystalline" and "kyanol," and received its present name from Fritzsche, who, obtaining it by a process only slightly differing from that of Undervorben, named it from *anil*, the Portuguese name of indigo. Aniline, as you see by the diagram, is one of the tar oil constituents; but, as it exists there in very small quantities indeed, it is fortunate that we are not dependent upon this source of it. It is to Zinin that we owe the knowledge of the simple relations between benzol and aniline, and the fact that the former can be converted into the latter with the utmost readiness.

On referring again to the diagram, we observe that the chemical composition of benzol is thus represented:—

$$C_6H_6.$$

Now, when benzol is mixed with strong nitric acid, a very violent action takes place, and an orange-coloured liquid is formed. When the orange liquid is poured into water, a yellow oil separates and falls to the bottom. This is nitro-benzol. The following equation represents the change which takes place:—

$$\underbrace{C_6H_6}_{\text{Benzol.}} + \underbrace{HNO_3}_{\text{Nitric acid.}} = \underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + \underbrace{H_2O}_{\text{Water.}}.$$

Nitrobenzol differs remarkably from benzol in its properties. It is, as you see, heavier than water, and its odour is remarkably fragrant, resembling to a considerable extent that of oil of bitter almonds. It is, indeed, known in commerce as artificial bitter almond oil, and is used as a perfume for soap. But it is merely as a step in the process of change which benzol undergoes in becoming aniline that nitrobenzol now interests us. When it is submitted to the action of hydrogen in that condition which chemists call *nascent*, that is just at the instant of its liberation, all the oxygen of the nitrobenzol is removed in the form of water, and, two atoms of hydrogen being introduced, we get *aniline*.

$$\underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + H_6 = \underbrace{C_6H_7N}_{\text{Aniline.}} + 2H_2O.$$

The method of effecting this change, now universally adopted, is that of Béchamp, which consists in distilling the nitrobenzol with iron filings and acetic acid.

The fact that, under certain circumstances, and more particularly when heated with any substance capable of affording oxygen to it, aniline gave rise to colour, was long known; but no one seems to have attempted to fix any of these colours. It was Runge who first noticed the action of chloride of lime upon aniline; and as this was, perhaps, the earliest experiment in which aniline was seen to give colour, we may repeat it. Here is a solution of a salt of aniline, and you will see that when we add to it a weak solution of chloride of lime, a blue colouration is at once produced. It is important not to add too much of the chloride, for the very agent which produces the colour is most active in destroying it. But though a patent has, indeed, been taken out for making a colouring matter in this way, the process has, I believe, been abandoned, as it is not very practicable.

It is to Mr. W. H. Perkin that we owe entirely the first method by which dye was obtained from tar on an industrial scale. Mr. Perkin was, at the time—in 1856—a pupil of Dr. Hofmann, and was engaged in the endeavour to form, by artificial means, the valuable medicine, quinine. I need not stop to detail here the details of this investigation, but may just say that he was acting upon a compound of toluidine with bichromate of potash. He was, as he tells us himself, surprised to find that, instead of getting quinine, he obtained only a dirty reddish-brown precipitate.

Being, however, desirous to know something more of the nature of this, he tried another base, and was fortunate enough to select aniline. I do not think that unless Mr. Perkin had been a very persevering chemist, we should have been any nearer to coal-tar colours to-day than we were in 1856; for in this case the precipitate was of quite as unpromising appearance as the first; but when it was more carefully examined, he found it to contain a beautiful purple-colouring matter. Here was the first appearance of *mauve*. Now this action of bichromate of potash on aniline will, perhaps, be clearer to you if we make the experiment. Here is a solution of aniline, and we will add to it some bichromate of potash solution. This is, somewhat curiously, the very process which is even now adopted

* A Lecture delivered before the Royal Dublin Society.

in making mauve. This dirty greenish-black precipitate is collected, and dried; it used to be washed with coal-tar naphtha, which removed a troublesome, resinous substance, with which it is contaminated; but this has been given up, and the powder is now treated at once with dilute methylated spirit, which dissolves out the dye; next the spirit is distilled off, and the dye which remains in watery solution is precipitated by the addition of caustic soda; it is then collected on a filter, washed, drained, and dried. Here is some of this aniline purple; it is a solid mass, brittle, and has a very beautiful bronze-like lustre.

We shall have to speak of this property of some of the coal-tar dyes again, so we will not stop to notice it now. Mauve is not very soluble in water, but very readily indeed in alcohol. Its intensity of colour is very remarkable, as I shall be able to show you. Here is a large vessel of water, and we will add to it some mauve, in the proportion of only 1-10th of a grain to a gallon. You see, even with this small quantity, the colour is not only distinct but deep.

This beautiful colour—here is a piece of paper, on which we have fixed it by means of albumen—is so much used now that many of my hearers know, probably even better than I do, that it is one of the "fastest" of the coal dyes, bearing the action of light remarkably well.

Now we have so much before us, that, as regards the chemical constitution of mauve I must only stop to point out to you that it is really a product of the oxidation of aniline; the bichromate of potash giving its oxygen to the base, and itself becoming reduced to sesquioxide of chromium. But no sooner was it ascertained that the action did depend upon oxidation, than the patent office was flooded with specifications, in which it was sought to supplant that of Perkin by effecting the same end by different means. It would be only interesting to chemists were I to read the list of substances which appeared in these specifications, to frame which it would almost seem as if the text-books had been ransacked for bodies capable of giving up their oxygen to aniline. Among those which we may single out, as really practicable, is the process of Dale and Caro, in which chloride of copper is used instead of bichromate of potash.

It was, however, soon discovered that, though a substance might oxidise aniline, it would not necessarily produce mauve; and it was found that there were some reagents, as, for example, nitrate of mercury, tetrachloride of carbon, chloride of tin, &c., which, instead of a purple, gave a crimson colour with aniline.

The discovery of this fact was the earliest step in the history of what is now known as magenta.

Magenta is formed when oxidising agents of a less energetic character than chloride of lime or bichromate of potash act upon commercial aniline. I say, on commercial aniline, because it has been found that pure aniline does not give the colour at all; and it has been found that the impurity—if we may so call it—which is necessary to a successful result, is another body, the name of which we have here on the diagram, and which bears exactly the same relation to the coal-tar product toluol as aniline does to benzol. This toluidine is not put into the aniline for this purpose, but the benzol used for making aniline is selected at such a boiling-point as will ensure the presence of toluol—the mixture being then first formed into a nitro-compound, and finally heated with iron and acetic acid, furnishes the mixture of aniline and toluidine which constitutes the aniline of commerce.

The process which is invariably followed in this country for the manufacture of magenta is that of Medlock. It consists in heating the aniline for some hours with a saturated solution of arsenic acid. This process is effected in a large closed iron vessel set in a furnace and provided with a stirrer. Heat is applied, and the mass is examined from time to time, until the maximum of colour is produced. Water is then added, and steam blown into the apparatus to dissolve the crude dye. The solution is filtered, and common salt is added to it. This throws down the colouring matter, which is collected again, dissolved in hot water, and set aside to crystallise. The result is in the form of these beautiful crystals, which I owe to the kindness of Messrs. Brooke, Simpson, and Spiller.

I think I can show you the formation of magenta from aniline, if we use, instead of arsenic, another substance, which does not require so much time. Here we have some aniline, and if we mix with it a little chloride of mercury, or corrosive sublimate, it forms a somewhat thick, sticky liquid, which as I heat it slightly, becomes, first, thicker and pasty, and then melts. As soon as it begins to boil the colour comes, and goes on increasing in intensity. It has now become very deep indeed; and you will see this better if I add some alcohol to it, and then pour a few drops into this large jar of spirit. Observe how all the liquid is coloured in a moment.

Magenta, from a chemical point of view, is a salt of a base called rosaniline; and it is a very curious thing, that, while the base is as nearly as possible colourless, the salts are colours of such intensity. Here, for example, is an ammoniacal solution of this rosaniline, and if we add to it an acid—some acetic acid—you see how rapidly it becomes red. Even the carbonic acid of the air can effect this change. So soon as the ammonia evaporates from the liquid, which I have here upon a piece of white paper, we have the crimson colour produced. Or, like most experiments, this may be pleasantly varied if we substitute this white flower for the paper, and, having immersed it in the solution, warm it gently over the gas flame. See how quickly the rose of York becomes that of Lancaster. It has been shown by Dr. Hofmann that when magenta is what chemists call reduced, that is, deprived of part of its oxygen, it is converted into this brown substance, called *leucaniline*. I must not dwell upon this, but may show you that, though acids do not, in the case of rosaniline, develope colour with this substance, it is at once transformed into rosaniline if we give it back its oxygen. Here is a solution of leucaniline, and I will add to it some bichromate of potash solution, which is a very convenient way of getting oxygen into bodies. As we heat the mixture, you will observe the red colour of magenta is at once developed.

You are all as familiar as I am with the colour of magenta; but you see it looks very pretty here, as we have it fixed upon a surface of albumenised paper.

Magenta is now manufactured on an enormous scale, being, indeed, considered rather as a raw material from which other dyes are made than as a dye itself, though, of course, there is a large consumption for it in this way too. As an example of the extent of this manufacture, I may say that, in going over the works of Messrs. Brooke, Simpson, and Spiller, a few weeks since, I was informed that they make 12 tons of aniline weekly; and at the same factory I saw magenta crystallising from a tank which contained 30,000 gallons of its solution. This was certainly the largest tank in the place, but there were some twenty others of smaller dimensions.

(To be continued.)

NOTICES OF BOOKS.

Light Science for Leisure Hours. By Richard Proctor, B.A. Camb., F.R.A.S., Author of "The Sun," "Other Worlds than Ours," "Saturn," &c. London: Longmans, Green, and Co.

Our newspaper press is each year becoming more scientific, supplying its numerous readers with explanations of the unaccustomed phenomena of Nature, or elucidating technical terms or processes pertaining to some matter of general interest. Mr. Proctor's latest work is a collection of several essays contributed by him from time to time to the various periodicals and the daily press. The main subjects are astronomical or mathematical, but apart from

their intrinsic interest, the articles will well repay perusal on the ground of their bearings politically and historically. Perhaps the most important papers are the first two, in which the phenomena of the aurora, and the variation of the magnetic needle, with their mutual remarkable occurrences, are considered. Here the author's style is very happy; the language is perfectly suited to the general reader, who is gradually led on to understand why spectroscopists have come to the conclusion that the light of the aurora is due to vapour rendered luminous by the passage through it of electrical discharges; while the various speculations as to the nature of the vapour with its peculiar, and, as yet, isolated spectrum, are carefully analysed. The nature of this spectrum is perhaps one of the most interesting scientific questions that have yet to be answered, because its elucidation will tell us also the substance of the zodiacal light, and of the phosphorescence which illuminates at times the nocturnal skies, all these yielding the same spectrum. Of his own views, Mr. Proctor says:—"When once we have reason—as in the case of the aurora we undoubtedly have—to associate electricity wirh any particular form of luminosity, we seem clearly justified in extending the explanation to the same form of luminosity wherever it may appear. I believe that the key to the whole series of phenomena lies in the existence of myriads of meteoric bodies travelling separately or in systems around the sun. They are consumed in thousands daily by our own atmosphere; they probably pour in countless millions upon the solar atmosphere; and, from what we know of their numbers in our own neighbourhood, and of the probability of their being infinitely more numerous in the neighbourhood of the sun, we have excellent reasons for believing that to them principally is due the appearance of the zodiacal light and the solar corona."

But it is not with such difficult questions that the reader has continually to grapple, for he can, if he pleases, study the fallacies and chances of the odds in the next or latest race, or the peculiarities and rules of the Oxford and Cambridge rowing styles; or again, if his taste be classical, spend an hour with his Homer in considering the genuineness of the description of the shield of Achilles, and find that here, too, has been deep reading, which has given rise to clear thought and happy expression.

CORRESPONDENCE.

THE PHOSPHATE SEWAGE QUESTION.

To the Editor of the Chemical News.

Sir,—If "Superphosphate" were practically acquainted with the manufacture of superphosphate of lime, he would scarcely ask why the native mineral phosphate of alumina is not directly converted into manure, for a solution of it in sulphuric acid does not set and become friable, but remains pasty—a dry pulverulent condition being a *sine qua non* in making artificial manures. The practice with some makers is to use an excess of sulphuric acid in dissolving calcium phosphates in order to ensure a perfect solution, and to dry up the excess of acid with gypsum, but, as "Superphosphate" has learnt from his own experiments, that lime precipitates the phosphates of iron and alumina, this *modus operandi* would be useless as applied to phosphates of alumina, and I cannot see otherwise than that it is an error in manufacturing superphosphates from coprolites (which contain phosphates of Al and iron), to so dry them up, for if the proper quantity of acid is employed, and the coprolites properly ground, the whole of the available phosphates will be dissolved, and the manure be in a good condition; whereas, in an excess of acid, we obtain a pasty product entailing much labour in drying with gypsum, besides the all but inevitable certainty of reducing some of the soluble phosphate. The condition, too, is never so fine as it should be, and where the superphosphate is sold on an analysis there is a loss of acid and labour which is not met by the higher (if any) results.

My opinion, with regard to phosphates of alumina is that if they were ground to as fine a degree as possible, and mixed with a small percentage of well-dissolved animal charcoal, they might be so distributed in the field with beneficial results. I have always found that superphosphate made from coprolites, with the addition of a small quantity of animal charcoal, are more friable, give better results, both on analysis and in the field, and do not become so "reduced" as those made from coprolites alone.—I am, &c.,

John Cox.

Nottingham Mills Company.

MISCELLANEOUS.

Liebig on Germany and France.—At a meeting of the Royal Bavarian Academy of Sciences, on the 28th of March, Baron Liebig spoke thus of the future relations between Germany and France:—"The Academy seizes this moment to declare openly that there exists no national hatred between the German and Latin races. The peculiar character of the Germans, their knowledge of languages, their acquaintance with foreign people, the past and present state of their civilisation, all tend to make them just toward other peoples, even at the risk of often becoming unjust toward their own, and thus it is that we recognise how much we owe to the great philosophers, mathematicians, and naturalists of France, who have been in so many departments our masters and our models. I went forty-eight years ago to Paris to study chemistry; a fortuitous circumstance drew upon me the attention of Alexander Von Humboldt, and a single word of recommendation from him caused M. Gay-Lussac, one of the greatest chemists and physicists of his time, to make to me a young man of twenty, the proposal to continue and finish with his co-operation an analysis which I had commenced; he introduced me as a pupil into his laboratory, my career was fixed after this. Never shall I forget the kindness with which Arago and Thenard received the German student, and how many compatriots, physicians, and others, could I not name who, like myself, gratefully remember the efficacious assistance afforded to them by French men of science in finishing their studies. An ardent sympathy for all that is noble and grand, as well as a disinterested hospitality form some of the most noble traits of the French character."

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Under this heading will be found an encyclopædic list of chemical papers published abroad during the past week, with abstracts of all susceptible of advantageous abridgment. The two half-yearly volumes of the Chemical News, *with their copious indices, will, therefore, be equivalent to an English edition of the "Jahresberichte."*

Note. All degrees of temperature are Centigrade, unless otherwise expressed.

Polytechnisches Journal von Dingler, second number for May, 1871.

This number contains the following original papers and memoirs relating to chemistry and allied sciences:—

Contamination of Iodine with Cyanide of Iodine.—Dr. G. C. Wittstein.—The author first refers to the researches on this subject made by MM. Scanlan, Meyer, and Klobach, the latter of whom found, in a sample of iodine, a quantity of cyanide of iodine amounting to 15 per cent. We further meet here with the results of the anal of two samples, *a* and *b*—the first being iodine made in the

way, by M. J. Sticht, at New York; the second, another sample of iodine, from which the maker had tried to separate the iodide of cyanogen by means of sublimation. Of sample *a*, 10 grains were weighed off and rubbed up in a mortar, after addition of a few drops of alcohol, with 16 grs. of metallic mercury, until all the free iodine was combined with the metal; the mass was next treated with 1½ fluid ounces of water, and the residue insoluble therein collected on a previously-weighed filter, and this next gently dried at 50°. The result was that the material thereon weighed 23·125 grains; consequently the loss (iodide of cyanogen) was 2·875 grains (since 26−23·125=2·875); the quantity per cent of iodide of cyanogen in this sample, 28·75. The amount of iodide of cyanogen found in sample *b*, which was tested in the same manner, was found to amount to 56·875. The author observes that Dr. Herzog has found that when iodine, which contains iodide of cyanogen, is treated with metallic iron and water, there is formed a liquid which contains, in addition to proto-iodide of iron, also proto-cyanide of iron. When carbonate of potassa is added to this liquid, all the iron and cyanogen are precipitated and eliminated; and the iodide of potassium thus prepared is quite free from any cyanide of potassium, which, it need hardly be said, is a substance the presence of which should be most scrupulously avoided in a salt like iodide of potassium, which is internally used in large quantities as a medicament.

Presence of Baryta in Native Silicates.—Dr. G. C. Wittstein.—Many years ago, the late Professor Mitscherlich, while analysing several specimens of feldspar, found therein baryta, amounting in many cases, to 2⅓ per cent. The author relates that he analysed some fifty samples of native silicates, among which were ten specimens of feldspar (all minerals met with in Bavaria), six of which were found to contain baryta; the highest percentage of that base present amounting to 2·518 per cent, the lowest to 0·3222 per cent, whereby we should observe that in two of the six samples only traces of baryta occurred.

Adulteration of Fuchsine.—Dr. A. Ungerer.—There was given to the author a sample of fuchsine which, having been found insoluble in alcohol, was considered unfit for use. It turned out, on investigation, that this sample of fuchsine consisted of coarsely-ground-up sugar-candy soaked in a very strong solution of fuchsine, so as to exhibit the external physical appearance of that body.

Changes which Flour Undergoes when Kept for a Length of Time.—Dr. Poleck.—The author has instituted a series of researches with the view of ascertaining the cause of the difference exhibited by flour preserved in casks, and that kept in bags, the former of which often obtains a peculiar smell. The author found that the gluten of flour exhibiting that peculiar smell had been, in a great measure, converted into a soluble modification, whereby the flour had lost the capability of being converted into a good dough, the flour also having assumed a sour reaction. The author ascribes the cause of this change to want of a sufficient circulation of air through the mass of the flour kept in casks, the innermost flour in which was most sour and gave off the peculiar somewhat mouldy smell. It was also found that, with this change in the flour, coincides an increase of the albuminous compounds therein soluble in water.

American Journal of Pharmacy, June, 1871.

Omitting the papers and memoirs more particularly relating to pharmacy and pharmacognosy, this number contains the following original papers and essays relating to chemistry:—

Culture of Hops in the United States.—E. Kannal.—Among other particulars more especially bearing upon the amount of production and cultivation of this important article, the author states that he obtained, from 1 lb. of fair commercial hops, 1¼ ozs. of lupulin, from 1 lb. of fresh New York hops 1 oz, and from fresh Philadelphia hops ¾ oz., averaging 1 oz. from the lb., or 6¼ per cent. The smaller yield in the last two cases was due to the fresh condition of the hops. When packed in bales, hops are sometimes adulterated—the outside consisting of good hops, while the interior is filled with hops deprived of the lupulin, and sprinkled over with lycopodium and powdered resin to hide the fraud.

Note on Hydrocyanate of Morphia.—Dr. J. M. Maisch.—After first referring to the fact that, among the descriptions of morphia salts, as furnished by various chemists, the hydrocyanate is not enumerated; in Gmelin's "Chemistry," some double hydrocyanates are mentioned, but not the simple morphia salt, of which, as far as the author knows, nothing is known of its formation or properties. The following contain the results of the experiments as thus far obtained:—(1) A neutral solution of a morphia salt, even if diluted to the proportion of 1 : 1500 (1 grain in 3½ ozs.), yields, with a neutral soluble cyanide, a crystalline precipitate consisting of hydrocyanate of morphia. (2) After the crystals have separated, the filtrate, acidulated with nitric acid, yields no precipitate with iodo-hydrargyrate of potassium; the morphia hydrocyanate, therefore, if soluble at all, dissolves but very sparingly in water. (3) The solubility of morphia hydrocyanate appears not to be increased by an excess of the precipitant. (4) The precipitate is readily dissolved if the liquid is slightly acidulated by a mineral acid; it is likewise soluble in acetic acid. Hydrocyanic acid does not precipitate a neutral solution.

Detection of Turmeric in Powdered Rhubarb and Yellow Mustard.—Dr. J. M. Maish.—A small quantity of the suspected rhubarb is agitated for a minute or two with strong alcohol, and then filtered, chrysophanic acid being sparingly soluble in this menstruum. The brown yellow colour of the filtrate is due to the resinous principles of rhubarb mainly; if adulterated with turmeric, the tincture will be of a brighter yellow shade; a strong solution of borax produces, in both tinctures, a deep red-brown colour. If now pure hydrochloric acid be added in large excess, the tincture of pure rhubarb will instantly assume a light yellow colour, while the tincture of the adulterated powder will change merely to a lighter shade of brown red. The test is a very delicate one, and is based on the liberation of boracic acid, which imparts to curcumin a colour similar to that produced by alkalies, while all the principles of rhubarb soluble in strong alcohol yield pale yellow solutions in acid liquids. The same test, applied in the same manner, is also applicable to ground mustard seed. The seeds of *Sinapis alba* yield a powder of a yellow-grey colour, entirely distinct from the colour of yellow mustard met with in the trade. Agitated with alcohol, and filtered, a turbid solution is obtained, which assumes a bright yellow colour on the addition of the borax solution, and becomes colourless or whitish again on being supersaturated with hydrochloric acid. If the mustard be coloured with turmeric, the filtrate has a yellow tint, becomes brown-red by borax, and retains the colour on addition of hydrochloric acid. All the so-called yellow mustard of commerce which the author has had occasion to examine, whether ground in England or in the United States, contains turmeric, a practice which ought to be discountenanced; for, under the yellow colour imparted by turmeric, adulteration of mustard may be carried on to an almost indefinite extent, if strength be supplied by the addition of a little capsicum.

Testing Cochineal.—J. M. Merrick, jun., S.B.—The author takes 2½ grms. of the drug, previously ground to powder, and boils that quantity in a capacious narrow-necked flask, with 750 c.c. of distilled water, for one hour. The liquid is immediately filtered through dry paper-filters, and tested when cold in the following manner:—50 c.c. are measured off and poured into another flask of about 200 c.c. cubic capacity, and the measuring vessel rinsed with a definite quantity of water, say 10 to 15 c.c.; a weak solution of permanganate of potassa is then run in from a burette with a glass cock, the flask being well shaken after the addition of every 10 c.c. of the permanganate solution (this does not, as does solution of bleaching-powder, precipitate the colouring matter of the cochineal), so much is added as to change the colour of the cochineal solution to a very faint pink shading on yellow, but never reaching a full yellow; this pink shade should be persistent, that is to say, it should not turn yellow after standing for fifteen minutes. The principal points in this practically very good method are—The use of a weak solution of permanganate; to have a very faint pink colour as a standard of comparison; to let the liquids remain, after agitation, together ten to fifteen minutes before comparing them. The author concludes his paper by observing that very little can be told of the value of a sample of cochineal by a mere physical examination; while the frequent inconsistency between value and price is equally surprising, the author having known samples to differ 30 per cent in colouring power and only one or two cents per pound in price.

Revue des Cours Scientifiques de la France et de l'Etranger (double number), June 8, 1871.

This number does not contain any original papers bearing upon chemistry or allied sciences.

Berichte der Deutschen Chemischen Gesellschaft zu Berlin, No. 9, 1871.

This number contains the following original papers and memoirs:—

Derivation of the Relation of the Heat of Gases under Permanent Pressure and Volume from the Mechanical Theory of Heat.—Dr. F. Mohr.—This paper, and the following lengthy essay by the same author—

Doctrine of Molecules—Are not suited for useful abstraction.

Determination of what might be not inaptly termed Chemical Longitude and Latitude—viz., the Exact Situation and Locality occupied by the CH_3 in Aromatic Compounds.—G. Gräbe.—Illustrated with diagrams.

Contribution to our Knowledge on the Sulpho-Nitrogen Acids.—A. Claus.—This lengthy paper contains the preliminary record of the results of the author's experiment on the mode of preparation and some of the properties of a compound which appears to have the formula $SO_2.NO_2K$. This is obtained by mixing together freshly-prepared solutions of sulphurous acid in strong alcohol and a concentrated solution of nitrite of potassa in water (prepared by passing a current of nitrous acid gas into a concentrated solution of carbonate of potassa). Since the compound, the formula of which is above-mentioned, is insoluble in alcohol, it is thrown down, along, however, with some nitrite of potassa, from which it is to be freed by cautious washing with a little cold water. The compound is very prone to decomposition also by heat, even below 100°. When treated with some water and peroxide of lead at a temperature not exceeding 40° to 45°, an intensely violet-coloured solution of trisulphoxy-azotate of potassa, $[NO.3(SO_3K)]$, is formed, which salt is obtained in solid state by cooling the solution down to below 0°.

New Series of Aromatic Hydrocarbons.—Th. Zincke.—The second instalment of a monograph on this subject, treating on the oxidation of diphenyl-methan and benzyl-toluol. Diphenyl-methan or benzyl-benzol, $C_6H_5---CH_2---C_6H_5$, is difficultly oxidised, and yields mainly benzo-phenon, $C_6H_5---CO---C_6H_5$. Benzyl-toluol, structural formula $C_6H_5---CH_2---C_6H_4---CH_3$, yields, by strong oxidation, among other products, an acid of the formula $C_{14}H_{10}O_3$; when the oxidation is less violent, an acid, $C_{14}H_{12}O_2$. The greatest portion of this part of the essay is devoted to the enumeration and description of a large number of salts of these acids.

Action of Sodium upon the Two Isomeric Monobromotoluols.—W. Longuinine.—The contents of this paper are essentially, by the author's own confession, parallel to those, and almost identical with the contents of a paper on this subject, recently published by Dr. Zincke. The main products of the action of sodium upon monobromotoluols are ditolyl, and a fluid body boiling at between 277° and 282°, $C_{14}H_{14}$.

Aceto-Sulpho Acid.—C. Bender.—The acid just named is, in free state, a colourless, syrupy liquid. The author describes a series of salts of this acid, among which we quote the following:—Aceto-sulphate of baryta, $(CH_3.CO.CH_2SO_3)_2Ba+H_2O$, crystallises, forming nacreous shining lamellæ; loses its water at 100°. Lead-salt, $(CH_3.CO.CH_2SO_3)_2Pb+H_2O$, has an acid reaction, crystallises, fuses at 140°, and is decomposed at 170°. Copper salt—

$$3[(CH_3.CO.CH_2SO_3)_2Cu]+4H_2O,$$

a bluish green-coloured salt, crystalline, and, like the other salts, soluble in water and dilute alcohol.

Quantitative Spectrum Analysis.—K. Vierordt.—An algebraico-physical paper.

Preliminary Communication on the Electrolysis of the Substituted Derivatives of Acetic Acid.—G. E. Moore.—The author represents the decomposition of pure cyan-acetate of potassium by electrolysis by the following formula:—

$$2\left\{\begin{matrix}CH_2CN\\|\\|\\COOH\end{matrix}\right\}+H_2O=\left\{\begin{matrix}CH_2CN\\|\\|\\CH_2CN\end{matrix}\right\}+2CO_2+H_2O+H_2.$$

The author is engaged in more accurately investigating the various products of this decomposition.

Short Notice on Isomeric Iodobenzoic Acids.—P. Griess.—The author communicates that some five years ago he prepared iod-salylic acid, obtained by the action of hydriodic acid upon sulphuric diazo-salylic acid. The new acid alluded to is a crystalline body, difficultly soluble in water, but readily so in alcohol and ether, and fusing at 152°.

Test for the Detection of Small Quantities of Sulphur present in Coal-Gas.—Dr. V. Wartha.—The author forms, first, before the blowpipe, in the loop of a platinum wire, a bead of pure soda, and next passes this bead over the edge of the gas flame, after which the bead is held in the interior of the flame in order to deoxidise the sulphates and sulphites of soda into sulphuret of sodium; the bead is then transferred to a porcelain basin, crushed, and some nitro-prusside of sodium added, whereby the smallest trace of sulphur will be detected. This reaction is fifty times more sensitive than that upon silver-foil; and the test can be performed in about three minutes, whereas Dr. Vogel's sulphur-copper reaction for this purpose takes four hours.

Les Mondes, June 22, 1871.

This number does not contain any original papers bearing upon chemistry or allied sciences.

Annalen der Physik und Chemie, von Poggendorff, No. 4, 1871.

This number contains the following original papers and memoirs:—

Pendulum Observations.—Dr. O. E. Meyer.—Illustrated with engravings.

Periodicity of the Heliographical Dispersion of the Sun's Spots.—Dr. F. Zöllner.

Weber's Compensated Magneto Meter for Measuring the Intensity of the Gas Magnetism.—Dr. F. Kohlrausch.

Refraction and Dispersion of the Light in the Chloride, Iodide, and Bromide of Silver.—W. Wernicke.—An algebraico-optical paper, an observation also applicable to the following paper.

Huyghen's Eyepiece.—Dr. J. B. Listing.

Combination of Sulphuric with Nitric Acid.—Dr. R. Weber.—The author caused the vapours of sulphuric anhydride to pass into very pure and highly concentrated nitric acid, care being taken to cool the flask containing this fluid very considerably by means of ice. By carefully experimenting, there is thus formed a crystalline substance, which, after having been drained by being placed on porous bricks, yields very deliquescent well-defined crystals soluble in water, thereby causing an elevation of temperature. This solution, on being tested with potassium iodide, only produces a very faint yellow colouration, thereby indicating that traces only of nitrous acid are present. The analysis, executed according to a method described here at length, but not presenting any new feature, led to the result that the formula of the body here alluded to is $4SO_3,NO_5,3HO$. In 100 parts, as found by experiment—Sulphuric acid, 66·22; nitric acid, 23·44; water, 11·25. As calculated according to the formula—Sulphuric acid, 66·40; nitric acid, 22·41; water, 11·19.

Construction of Filtering Apparatus According to Bunsen's Apparatus.—E. Zettnow.—This description cannot be understood without the reproduction of the engravings.

Appendix to my Paper "On the Influence the Concentration of Saline Solutions has upon the Electromotive Force."—Dr. L. Bleekrode.—The addition to a lengthy essay published by the author in this periodical some years ago.

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Critical Remarks on Dr. Andrews's Paper on the Ice Calorimeter.—Prof. R. Bunsen.

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Microscopical Structure of Hailstones.—Dr. P. Reinsch.—A paper illustrated by engravings, and, as the immediately preceding and following, chiefly algebraico-physical as regards the contents.

Conduction of Heat in Layers of Liquids.—Dr. Despretz.

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